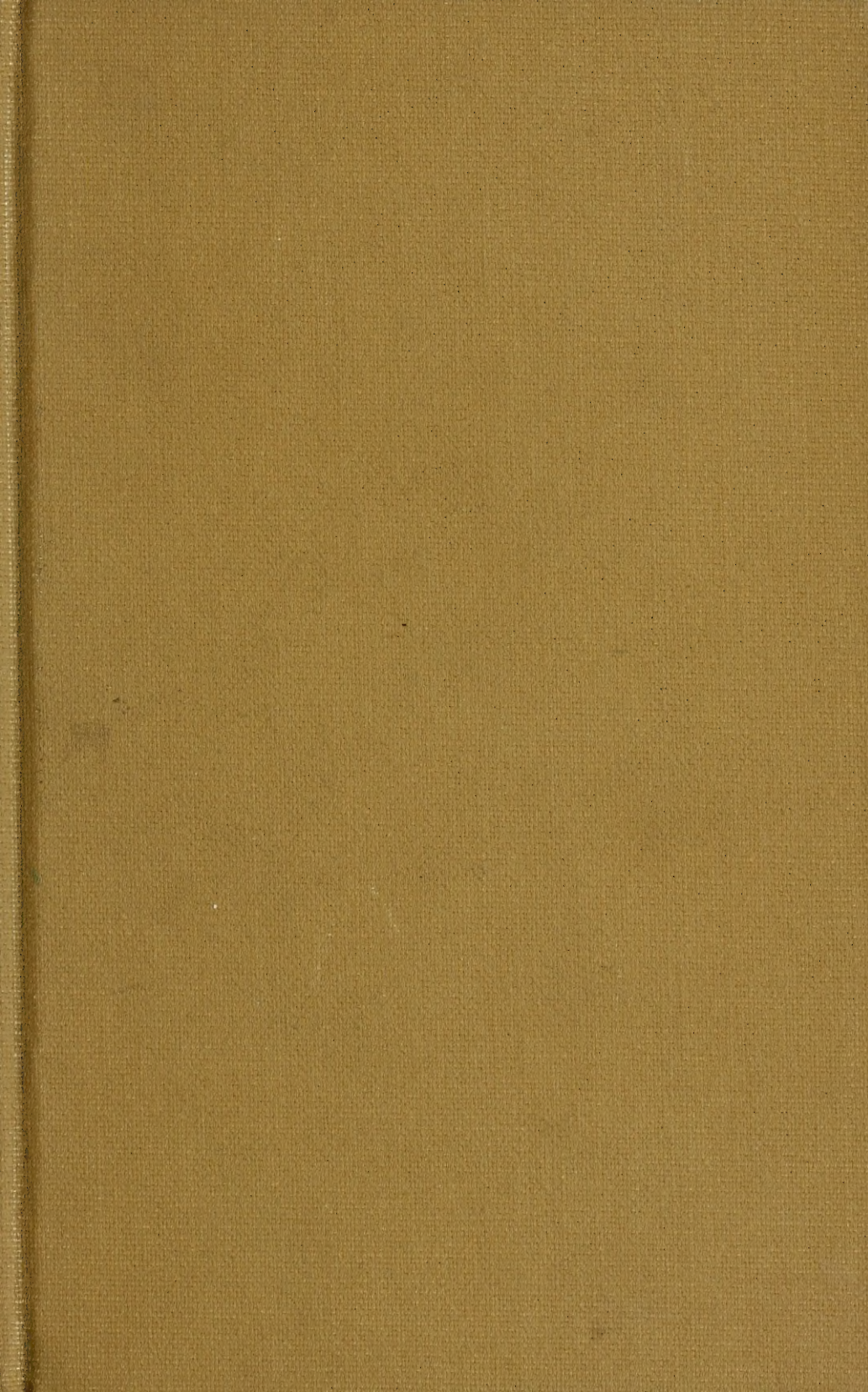
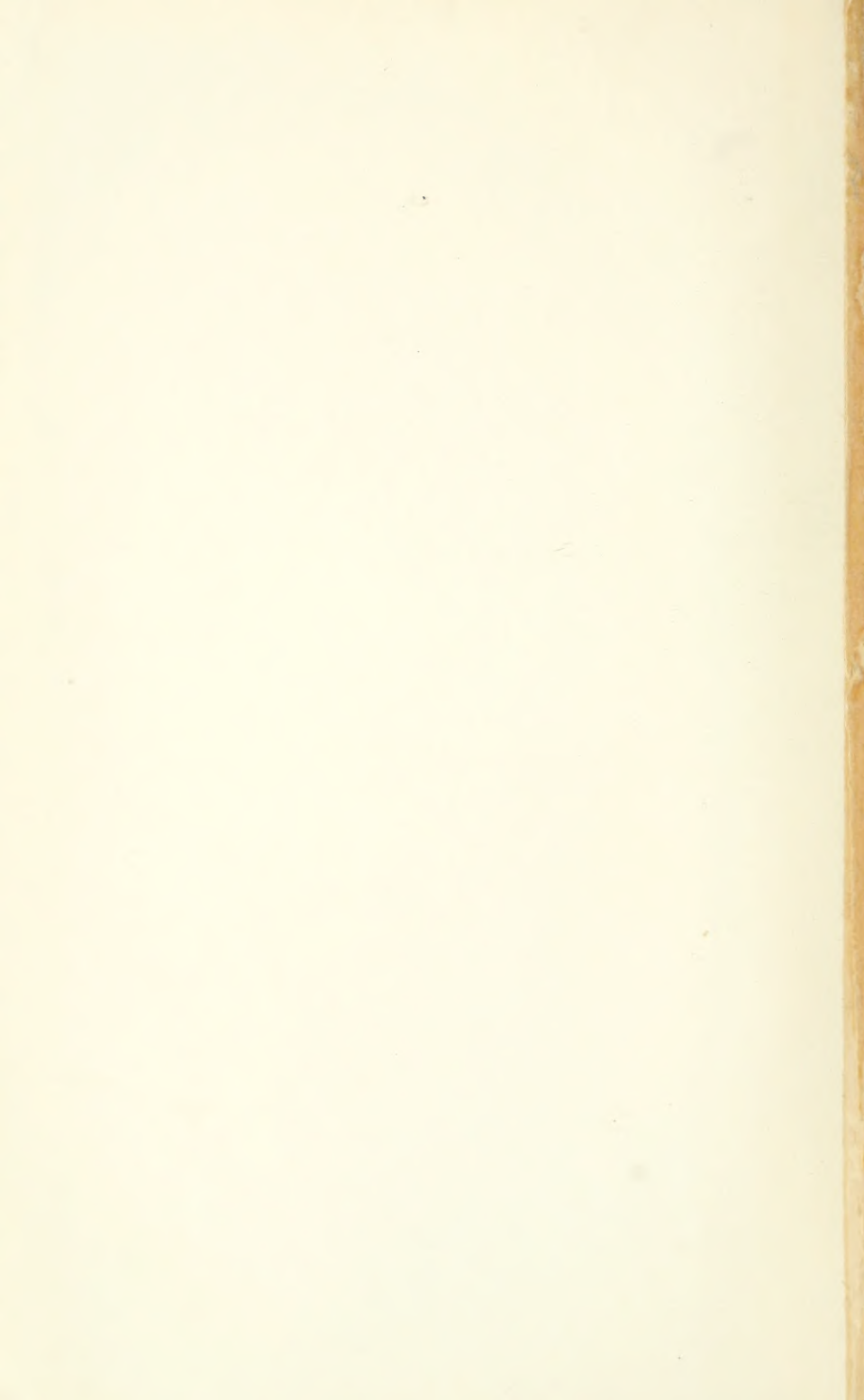






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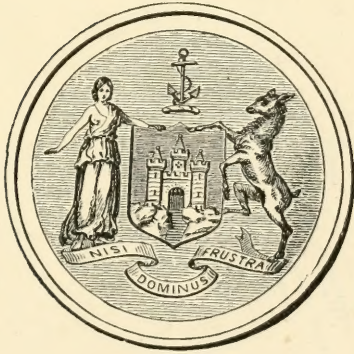
THE MONTHLY JOURNAL OF MEDICINE

AND

THE EDINBURGH MEDICAL AND SURGICAL JOURNAL.

VOL. XI.—PART II.

JANUARY TO JUNE 1866.

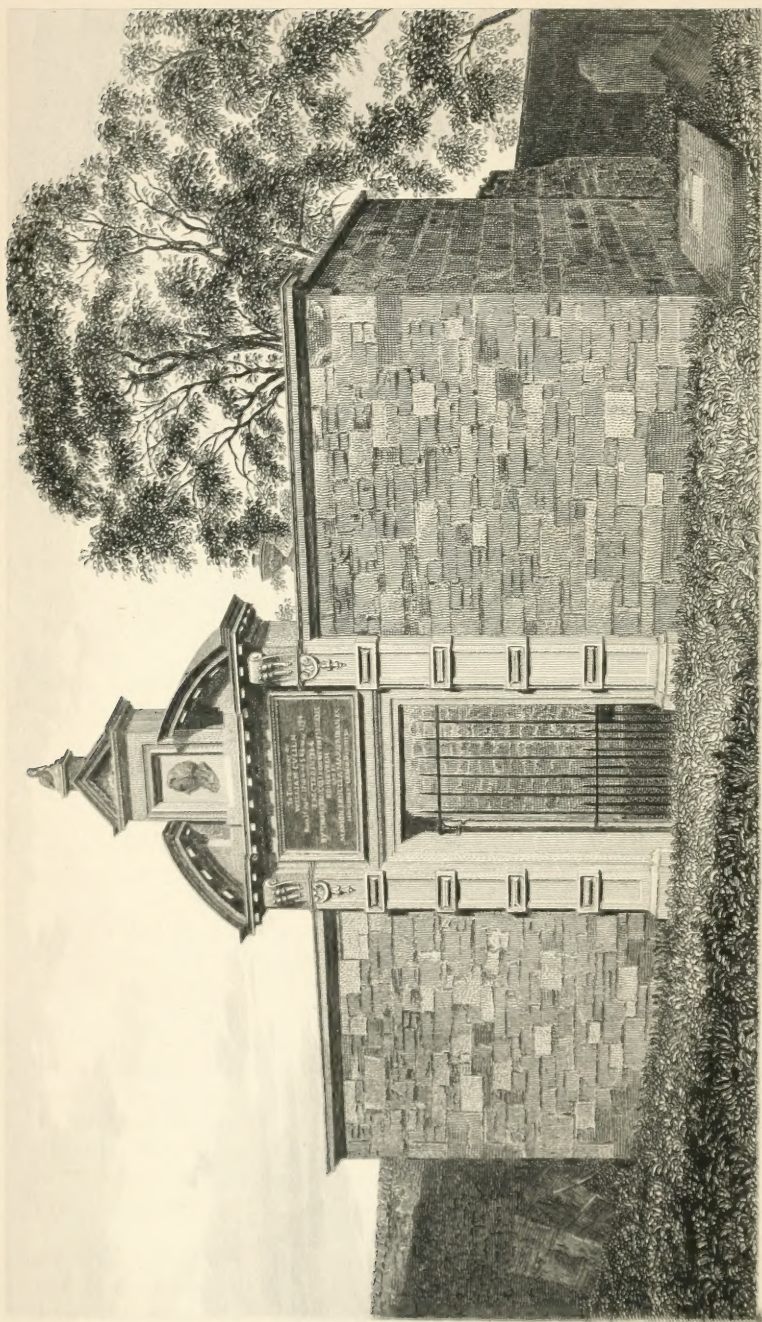


EDINBURGH:
OLIVER AND BOYD, TWEEDDALE COURT.
LONDON: SIMPKIN, MARSHALL, AND CO.

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The Grave of Coulton

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Monument to the late Dr Cullen.*

THE author of the most important course of instruction in Medicine that has ever perhaps been laid down and completed by the same individual, and whose writings continue to the present day, as they must ever do, to exercise an influence on, and to direct the progress of the study and practice of the healing art, was born at Hamilton on the 15th day of April 1710.

Dr Cullen was, confessedly, at once the greatest practical physician and the greatest medical reformer and teacher of the last century. Beginning his medical life as a ship-surgeon, he betimes settled down as a country medical practitioner in Hamilton. Subsequently he was removed to Glasgow, and afterwards to Edinburgh; and in the Universities of these two cities he successively acted as Teacher and Professor of Botany, of Chemistry, of *Materia Medica*, of the Institutes of Medicine, of Clinical Medicine, and of the Practice of Physic. For twenty years before his death he occupied the Chair of the Practice of Physic in the University of Edinburgh, teaching the subject with a degree of power, vigour, and originality that has never been equalled.

Dr Cullen, full of years and of honour, respected and beloved by all, died on the 5th day of February 1790, aged seventy-nine years and nearly ten months, and was interred in the churchyard of the parish of Kirknewton, in the county of Edinburgh.

The family place of sepulture had been enclosed by a high wall by the venerable philosopher, but, strange to say, although it contained a monumental marble slab in honour of his son, Lord Cullen, there was nothing to indicate that there also were the remains of the great and good physician. This might be partly owing to the absence from their native land of some of his family, and the deaths of others, and partly to the quiet rural district in which he lay, causing the omission to be overlooked.

To the honour of the Royal College of Physicians, of which he was so distinguished an ornament, they lately bestirred themselves, and having obtained a design from Mr Bryce, the eminent

Scottish architect, they resolved to proceed with the restoration and embellishment of the enclosure. Dr Cullen's only direct descendant, on learning the intentions of the Royal College, though not wishing to divert the College from its purpose to pay a tribute to her illustrious ancestor, desired to be allowed to share with them in the duty proposed; and the structure, a sketch of which is presented to our readers, has been completed at their joint expense.

Over the lintel of the gateway, and between the side pilasters, is embedded a large slab of Peterhead granite, bearing the following inscription:—

TO THE MEMORY OF
WILLIAM CULLEN,

BORN 15TH APRIL 1710. DIED 5TH FEBRUARY 1790.

ERECTED IN 1864

BY THE ROYAL COLLEGE OF PHYSICIANS, EDINBURGH,

AND BY

MARION FENELLA MARJORIBANKS,

HIS GREAT-GRANDDAUGHTER.

Over the cornice, and in the centre of a broken pediment, is a square pendimented panel, forming a frame and background for a life-size bronze medallion of Dr Cullen, by Brodie,—the whole being finished by the family crest, sculptured in the beautiful stone of which the monument is built.

ARTICLE II.—*Case of Bronchocele and Hæmorrhagic Villosities of the Bladder, with Dissections of the Tumour.* By W. C. M'INTOSH, M.D., F.L.S., Medical Superintendent of the Perth District Asylum.

(Read before the Medico-Chirurgical Society, 6th December 1865.)

THE patient, M. F., is an unmarried female, æt. 45, of dark complexion, and bilio-strumous diathesis. She is a native of Aberuthven, near Auchterarder, and, with the exception of one year, has been resident in that locality all her life. From information gathered from herself and friends, it would appear that the disease commenced after she was twenty years of age, and it has slowly but steadily advanced up to the period of her late mental affection, when it is stated to have perceptibly increased. So far as can be learned, there was no exhausting discharge or anæmia at the time of its appearance. Bronchocele would almost merit the name of endemic in that neighbourhood, for several unmarried females and one widow are mentioned as suffering from the same affection. The water of the

district is stated to be hard, and the land lies in a valley, not far from the foot of the Ochils. Dr A. Mitchell¹ is of opinion that the disease nowhere merits the name of endemic except in the southern counties; but, in addition to the foregoing, I think I have heard Professor Syme state that he has met with several cases from a low-lying district near the Pentlands, when commenting on an example from that locality. In few cases in the Auchterarder district does the swelling grow large, none being so bulky as in the instance of M. F. In one young woman a resulting marriage kept the tumour in salutary subjection.

The enlargement of the thyroid began without evident cause, and seems to have attracted little notice, and less treatment, until it had attained considerable dimensions. When I saw her in 1861, the circumference of the neck at its widest portion was one foot seven inches in the erect posture; and the tumour was decidedly larger than any seen by Dr Mitchell, who limits his maximum example to the size of two oranges. There was not much appearance of lobulation, but the right side of the tumour was decidedly more prominent, while the left extended higher up. By comparing the cast and the drawing (Fig. 1) it will be observed that the appearance of the tumour



FIG. 1.—Sketch of the bronchocele, from a photograph taken in M. R. Asylum in 1861.

differs very much, since the former was taken in the recumbent posture, the latter in the erect, where the weight of the swelling makes it pendulous. Her respiration is considerably affected, as evinced by a wheeze in breathing, especially during excitement. Her speech

¹ Med.-Chir. Review, April 1862.

is generally abrupt, and in snatches. The size of the tumour, which stretches from the hyoid bone in the centre, and nearly to the angles of the lower jaw on either side, down to the sternum, would lead us to expect this, and, further, that the movement in deglutition would be obscure. A peculiar expansion of the rounded nostrils accompanies each inspiration, and indeed they seem to be constantly dilated. Her eyes are large and staring, with a slight tendency to obliquity, and the pupils are often widely dilated,—circumstances probably connected with an irritable state of the sympathetic in the neck. She never complains of inconvenience, but it may be seen that the effect of the mass on the circulation is considerable, for slight causes readily distend the veins of the face and neck almost to bursting. A large artery (ext. carotid) can be felt pulsating at the left posterior border, and two dilated veins, the external jugular, are well seen in the cast. There is a whizz in the vessels of the neck, but no cardiac murmur. The surface of the tumour is covered with scars, the results of treatment before admission. During her periods of excitement, the bronchocele sensibly enlarges, a fact stated by many observers.

The psychological features of the case are these:—With the growing tumour, she followed an active life, and was always a painstaking and industrious person, of exemplary habits. About eight years ago, when 37 years of age, she became depressed in spirits, but was never subjected to medical treatment. For a year or two before admission she was noticed by her relatives to become very suspicious, and eventually much depressed; and for the last ten days she had manifested a persistent suicidal tendency, which induced her friends to bring her to the asylum. She attempted to choke herself on two occasions, and once tried strangulation. On admission, she presented the characteristic features of suicidal melancholia, being restless and emotional, tearing her hair, face, and bronchocele, and demanding to be bled, as she stated that the blood had all gone to her head and neck. She secreted strings, tapes of night-caps, tied the sheets round her neck in the middle of the night, tore up matting, and, when deprived of all, coiled her hair round her neck almost to suffocation. She also used her hands for the same purpose; tried to precipitate herself down stairs; loitered behind in the bath-room to drown herself; and, in short, required the utmost vigilance by night and by day to prevent self-destruction. So terrified, indeed, did the attendants sometimes become lest she should effect her purpose that they sat up all night rather than sleep in the dormitory by her side, although bed, bedding, and person had been carefully searched. During a paroxysm, which lasted about two months, not long after admission, she became much exhausted, and her voice resembled the bleating of a sheep. On one occasion I was called to her gallery to find her lying amidst her companions choked while taking dinner, and to all appearance lifeless, with swollen, purple face and neck, and bursting

eyes. It was stated that she had swallowed a bone. With great difficulty, and with the aid of a horn spoon, the mouth was opened sufficiently to admit the finger, for the jaw was quite rigid, although life was apparently absent. No bone was felt, neither was I sure of feeling anything; however, on putting in the dressing forceps to clear the rima, they came in contact with some obstruction, which, when brought out, proved to be a piece of beef, fibrous textures and fat—an unmastered mass sufficient to choke any one. Faint gasping at considerable intervals ensued on removal of the foreign body, and by evening she was quite better, though her pupils continued widely dilated for some time. It is doubtful whether she meant to choke herself or not, but the occurrence of a like attempt at home makes the former supposition probable. She was, at all events, considerably frightened by the results; and the accident seemed to have no unfavourable effect on her case generally.

As regards treatment, besides careful attention to her diet and general health, the tumour has been frequently painted with iodine, and she has taken the hydriodate of potash for two or three months at a time, without inducing the slightest decrease in the bulk of the bronchocele; indeed, it has slightly increased in size since admission. The knowledge of her early treatment at home is somewhat imperfect, but the tumour bears the scars of a croton embrocation, and she says that its surface was frequently painted and blistered, and much “internal medicine” was given her.

These were the notes made in the spring of 1862, when the patient was an inmate of James Murray's Royal Asylum, Perth; and little could be added to them up to July 1863. I lost sight of the patient from the last-mentioned date till the beginning of April 1864, when she was admitted into the County Asylum. She was now suffering from bloody urine, which also contained pus, and a large quantity of epithelial cells, and her general health was much deteriorated. Anæmia was well marked. She lingered on for about two months, the pus and blood increasing in the urine in spite of dietetics and medication. Symptoms of pleurisy, with effusion, now appeared, and for some days before her death dyspnoea was urgent, caused, as she averred, by the pressure of the bronchocele; and, bloodless as she was, she implored to be bled. My own recollection, and the notes in the daily case book by Dr Rutherford, show that at no time did any cardiac murmur appear.

At the post-mortem examination, the body was found not to be much emaciated, and the tumour in the neck more flaccid than in life. The brain weighed $48\frac{1}{2}$ ounces, and was remarkable only for its general anæmic aspect. There were a few polypoid granulations on the choroid plexus of each lateral ventricle. The heart was soft, weighed $13\frac{1}{2}$ ounces, and its muscular fibres dappled with fatty matter, from degeneration of the primary tissue. In each ventricle, the columnæ presented the peculiarly marked aspect of a woodcock's feather, being barred across with light and dark streaks, the result

of the same degeneration. There were a few fibrinous clots in each cavity; none of the latter (cavities) were of disproportionate size, and all the valves were competent. In each pleural space there was a serous effusion, with bands of old, and a patch of recent lymph at the right back inferiorly. Both lungs were loaded with serum and congested. Right, $24\frac{1}{2}$ ounces; left, $22\frac{1}{2}$ ounces. In the abdomen, the liver¹ was pale, but otherwise normal; the kidneys² also pale and slightly granular. The spleen was not enlarged, weighing only $3\frac{1}{2}$ ounces.

On cutting open the somewhat thickened bladder, its entire mucous surface was found bathed in thick creamy pus, mixed with blood, and one large disintegrating clot lay at the bottom. The mucous membrane of the organ was covered here and there with patches of a villous structure, highly vascular, to which morbid growths were doubtless due the constant hæmorrhages which rendered the urine quite bloody. These villous growths occurred chiefly on the inferior and posterior walls of the bladder, scattered in an irregular manner, the largest villi and the most extensive surface being behind, and to the left of the opening of the left ureter. A considerable patch of long villi was placed just above the opening of the urethra. Several small tufts were on the trigonum vesicæ, and scattered over the regions indicated above. The apertures of the ureters were quite free from the growths. So fine, indeed, were the villi, that the interior of the bladder had the aspect (when removed from fluid) of being here and there covered with masses of wet cotton fibres. If immersed in spirit or other transparent liquid, the downy threads floated out very prettily, and much resembled the delicate villi from one of the cotyledons of a cow's placenta. Under a dissecting microscope the villi presented the appearance of fine threads, with swollen or club-shaped extremities. Some had vaccate or lobulated ends, enormously larger than the stalk; others were nodulated all the way up. In intimate structure, the whole of the villi were fibro-granular, and they stood a considerable strain. These were by no means the simple mucous polypi, now and then found in the bladder, but tufts of fibro-cellular, vascular villi.

So heavy had been the drain on the circulation by this hæmorrhagic bladder that little blood remained in the body, and very few clots. The cellular tissues generally had a tendency to be loaded with serum, a fact well seen in laying bare the spinal cord. The serum in the lungs and pleural cavities was no doubt partly due to the impoverished condition of the circulating medium. It must be recollected, however, that this anemia, in the present case, appears to be only of secondary importance in regard to the bronchocele, since the latter occurred so long before the approach of this condition.

The bronchocele was removed with the trachea, œsophagus, sub-maxillary glands, etc., and, with these, weighed $39\frac{1}{2}$ ounces. The

¹ Weight $4\frac{1}{2}$ ounces.

² Each weighed $5\frac{1}{2}$ ounces.

ordinary weight of the gland is from 1 to 2 ounces, though lately, in an idiot male, the gland was found considerably enlarged, weighing $2\frac{3}{4}$ ounces. This is the opposite of the instances recorded by Mr Curling, in the *Medico-Chirurgical Transactions*,¹ of its total absence in two idiots.



FIG. 2.—View of the tumour laid on a flat surface, with the skin and subcutaneous textures removed so as to expose the muscles. *a, a.* The sterno-mastoid muscles reflected. *b, b.* The sterno-hyoid. *c, c.* The anterior jugular veins. *d.* Tendinous aponeurosis of the omo-hyoid. *e.* Textures covering the hyoid bone. *f.* Submaxillary gland.

Covering the sides of the tumour in front were the sterno-cleido-mastoid muscles, which were flabby and enlarged, Fig. 2, *a, a.* On clearing the exposed portion between these, the sterno-hyoid were found stretching almost over the entire surface, Fig. 2, *b, b.* with the anterior jugular veins, *c, c.* coursing downwards in a pretty normal manner. The muscles were seen to be much expanded; the right, traced from its insertion on the hyoid bone, proceeded downwards, widening as it went, to be attached to the sternum and clavicle. The left muscle also followed a normal direction, but was rather more expanded than the right. The left omo-hyoid was peculiarly widened and altered, spreading at its upper part over the tumour as a broad riband, then narrowing somewhat at its aponeurosis, Fig. 2, *d.* again widening a little as it proceeded downwards, became rounded, and was attached to the scapula as a considerable

¹ Quoted by Jones and Siæveking in their "Pathological Anatomy."

muscle. The action of this muscle on the tumour was very visible during life.

The right thyro-hyoid muscle was well marked, and incorporated with a dense aponeurosis towards the inner side. The right sterno-thyroid went down altogether by the right side of the tumour, and helped to bind its edge, but permitted the bulging portion on the same side to protrude between its inner border and that of the more securely bound left side. The chief support of the tumour on the right side was the strong aponeurosis at the inner border of the thyro-hyoid, which spread over the antero-lateral surface of the upper portion of the tumour, in intimate connexion with its fibrous sheath.

On the left side, the sterno-thyroid, attached by a tendinous ribbon about an inch in breadth to the ala of the thyroid cartilage, rapidly widened as it proceeded downwards, and spread over more than one-half of the anterior surface of the tumour. Muscular fibres



FIG. 3. The tumour suspended by the hyoid bone. *a*. Cricoid cartilage. *b*. The sterno-hyoid muscle. *c*. Thyro-hyoid. *d*. Ono-hyoid. *e*. Sterno-thyroid. *f*. Superior laryngeal nerve.

began to appear about two inches from its superior attachment, and became more abundant below, especially at the left side. The

muscle was fixed to the sternum in the usual manner, and it must have exercised a considerable repressing power on the growth of the tumour, an effect very visible on the left side during life, and noticeable in the cast.

When the tumour was hooked by the hyoid bone, the suspensory powers of the respective sides were readily demonstrated, as seen in Fig. 3, where the preponderating influence of the left side is well shown. So great, indeed, was the repressing influence on this side, that the tumour formed a large projection posteriorly and superiorly, the upper border here being $\frac{3}{4}$ of an inch above the level of any other part, and much further backwards. A process of the tumour, moreover, about three inches in depth, passed backwards by the side of the trachea, and encroached on surrounding tissues. This growth on the left side pushed the trachea to the right, so that the œsophagus only touched the left border thereof at the upper part of the tumour, and was quite apart inferiorly. The oblique disposition was also noticeable in front, as shown by the appearance of the cricoid cartilage in Fig. 3, *a*. There was no distinct levator glandulæ thyroideæ of Sœmmerring.

The lymphatics of the neck were much enlarged. Both carotids were above the average size. The tumour was supplied with blood by the ordinary vessels, which had increased in calibre. The veins in and near the bronchocele were quite empty.

On uncovering the muscular textures from the surface of the tumour, a tough fibrous sheath was found investing it. This sheath, especially towards the right side, had a somewhat honey-combed aspect, denoting cysts or cells. By a perpendicular section through the middle of the tumour, it was seen to be a spongy mass, made up of a vast number of lacunæ of varying size, with intermediate fibrous tissue, which here and there was diversified by masses of fibro-cartilage, sometimes of great density. Some of the cells or spaces were of large size—about an inch and a quarter in diameter, the majority smaller. Such cavities, no doubt, were exaggerated states of the independent membranous spaces found in the normal gland. They contained a semi-fluid mass of cells and granules, chiefly the latter; a few oil globules, and many crystals of cholesterine.

The swelling of the veins of the neck is a constant accompaniment to large bronchoceles; indeed they are to be seen standing out like thick-knotted cords in insane females in Savoy and Switzerland.

In this patient, in the first instance, there was neither catamenial irregularity nor anæmia, the latter, and the staring expression of the eyeballs, being only of secondary and comparatively recent occurrence; thus, the causation of many cases, so ably stated by Dr Begbie, senior, in his valuable paper,¹ could not account for the goitre in this case. The enlargement of the gland, during her periods of excitement, and at the onset of her mental affection, showed the influence of sympathetic irritation, as clearly pointed out by Professor

¹ Chap. iv., *Anæmia and its Conseq.* Contrib. to Pract. Med., 1862.

Laycock in an exhaustive paper lately published,¹ and the same author has shown its connexion with the genetic organs in his *Nervous Diseases of Women*. In vol. i. of *Loudon's Magazine of Natural History* there is an account of a large thyroïdal swelling met with in an adult female hare, which was in excellent health and pregnant. In the case of M. F., there was no increased action of the heart under ordinary circumstances; no anæmic murmur; nor was there a difference between the pulsations in the neck and the wrist. There was at no time any thrill in the tumour, like aneurismal varix, such as is pointed out by Dr Stokes to be a distinctive mark of thyroïd cachexia.

ARTICLE III.—*Where should Typhus be Treated?* By EDWARD LONG FOX, M.D. (Oxon.), M.R.C.P., Physician to the Bristol Royal Infirmary.

IN the course of an epidemic of typhus with which Bristol was visited last year, it was easy to realize the vast difference of opinion that exists, even between members of the medical profession, on the question of the external conditions in which typhus patients should be placed for treatment.

That this difference of opinion is not confined to one locality is proved by the practice of the metropolitan hospitals, as distinguished from the provincial, by Dr Murchison's excellent work on *Fever*, and by the report of Dr Bristowe and Mr Holmes, endorsed by Mr Simon himself, in the *Sixth Report of the Medical Officer to the Privy Council*. This is not the occasion for entering into the question of the identity or non-identity of typhus and enteric fevers. It is, perhaps, presumptuous in any man to speak positively of diseases beyond the limits of his own personal experience; and it is therefore beyond my province to condemn the views of physicians of eminence, who are unable to draw a distinct line of demarcation between these two diseases as they exist in other countries.

It is sufficient for my purpose to say, that here, where enteric fever is always endemic, and where we have lately had, and still have, a considerable epidemic of typhus, where also in our hospitals post-mortem examination of fatal cases is the almost invariable rule, and the marked distinction between the ulcerated intestinal glands in the one set of cases, and the absence of all such ulceration in the other (not to speak of other pathological appearances), must strike the youngest student, he would be a bold man who would connect together as one in nature and in name two diseases, whose effects on the human frame are so opposite, whose course both in period and in symptomatic character is so distinct, and in which the eruption is, generally at least, so diagnostic a mark.

¹ *Med.-Chir. Soc., Edin.*, 3d June 1863, and *Edin. Med. Jour.*, July 1863.

Speaking, therefore, of our English experience, typhus differs from enteric fever in nature, and this difference manifests itself not only in symptoms, in length of illness, and in pathological results, but also in contagious properties.

This point, therefore, being granted, that we are treating of typhus as entirely distinct from typhoid or enteric fever, it becomes a serious question what we should do with typhus cases—where should we place them so as to give a chance of recovery to the greatest number, and at the same time prevent their injuring other individuals?

In a large city like this it is always difficult to trace precisely the commencement of any contagious disease, and this difficulty is often brought forward as an argument for the spontaneous origin of such diseases from filth, drainage-exhalations, overcrowding, and the like.

It is almost foreign to the purpose of this paper to enter upon the spontaneous origin of typhus, but I may state that the present epidemic in Bristol and the neighbourhood has evidently spread by personal contact only, and that this fact, though traceable even in the city, has been particularly noticeable in country districts. At the same time, it would require a very large experience to deny the occasional spontaneous origin of this disease from overcrowding.

A few months ago, in the parish of Wrington, about ten miles from Bristol, typhus occurred in a block of houses surrounded with many sanitary deficiencies. There had never been any typhus there until a woman, who lived in one of the houses, went to a neighbouring town to nurse her daughter in this disease. She herself contracted it, and of her neighbours those only were affected at first who had personal communication with her; and these in turn gave it to members of their own families. The medical man, Mr Horace Sute, established a strict cordon round the infected houses, and brought down a trained nurse, and the disease, by these precautions, was prevented from spreading further.

Occurring in isolated districts, such facts are the more valuable, but the history of our own epidemic will give numerous instances of a similar nature; so much so, that now that the city of Bristol is mapped out into divisions, each under the careful supervision of an inspector of the Board of Health, our excellent medical inspector is enabled, in almost every instance, to trace personal communication with the disease. Especially is this the case when typhus shows itself in a quarter of the city hitherto unaffected. It is not that the drainage is bad, the want of cleanliness extreme, the overcrowding lamentable. All these things serve only to increase the evil when it comes; but here, at least, they seem never to give rise to its production, until some one of the inhabitants of such a district happens to have some communication with the clothes, the bedding, or the persons of patients ill of the disease.

Not only so, but if a person who brings the disease into such a quarter is taken away and isolated in the first week of his illness,

the fever does not spread in the locality, although, in its want of sanitary arrangements, it may be theoretically the very place in which typhus would be likely to increase; whereas, if the patient is not isolated, the malady inevitably spreads through every room of the crowded tenement, and often extends itself on each side of it.

It was particularly striking last winter to see the manner in which typhus spread in the poorest district of Bristol. It seldom did so from one house to the next, unless the neighbours were on intimate terms, or unless they had a common privy. The houses in a court with a common privy were affected quickly, one after another; but where each house had its own convenience of this kind, the disease spread, not next door, but to houses from whence relatives of the sick people had come to visit them, or whither, as occurred in some cases, they had removed the clothes or bedding of their relatives who had died of fever.

This district was then the worst drained portion of the city, the most dirty, the most overcrowded, and the inhabitants were the very poorest of the population, were ill-fed, and many were intemperate.

All parts of the district possessed these points in common; but yet some portions escaped, having all the supposed external qualifications for typhus, but yet wanting, by some happy accident, the one point of personal communication with infected persons.

The disease, doubtless, might have been arrested at an early period had there been any provision for isolating the patients first affected, or had it been possible to draw a cordon round the infected courts and houses. The latter point is rendered practically impossible amongst the poor of a city, not only from the prevalent ignorance as to the nature of contagion, but still more by the neighbourly sympathy and kindness which binds the poorer inhabitants of our cities so closely together.

If, then, we allow the absolute entity of the typhus poison, and its virulent contagious properties, the position in which we should place such cases for efficient treatment becomes a grave question; and this includes, as we said above, not only what is best for them, but what is most safe for others.

There are four conditions under which such treatment may be attempted. The patients may be left in their own homes; they may be treated in general hospitals, mingled with patients ill of other diseases; or in special wards of general hospitals; or in special hospitals.

Always remembering that the question involves not only the patients themselves, but others also, the first idea of treating such patients in their own homes is tolerably easy of solution.

Although it is always difficult to obtain accurate statistics of the results of private practice, I have formed the impression, founded chiefly on our experience in this city last winter, that under the plan of home treatment the death-rate is not particularly high. And in speaking of death-rate, of which I shall have more to say hereafter,

I premise that I am speaking of the present epidemic of typhus, which has lasted now, in various parts of the United Kingdom, for the period of nearly four years, and I only just consider comparisons that are drawn from the present epidemic, believing, as I do most fully, that every epidemic differs in important particulars from its predecessors, and in no respect more than in its tendency to fatal results. All figures, therefore, quoted in this paper will be drawn from records of the last four years.

That the death-rate is not very great when typhus cases are treated in their own homes, may be attributed partly to the misery of these homes. In many cases the single room contains a large family, sometimes even more, but then the glass of the windows has been broken long ago, and its place so inefficiently filled with rags that the ventilation is practically a great deal better than if the window had been perfectly sound. Again, however numerous the occupants of the room may be during the night, through the day they are generally scattered in their various occupations, and the children play all day long in the street or the gutter. The cubic space too, small as it is, is seldom filled up by furniture, as the bed, or the heap of rags and straw, is often the only attempt at furniture in the room ; and last winter I found one woman dead of fever in a room in which there had not been even a bit of straw during her illness.

At the same time, even if the death-rate under such circumstances may be low, such conditions must interfere with the convalescence of a patient, even if there was no need to take into account the effect upon others, of a patient so situated.

This effect is tolerably invariable. Such families are always accustomed to huddle together by night for warmth, and it makes no difference to them, in their ignorance, and their want of space, that one of their members is suffering from disease.

It is a common sight enough (it was seen very frequently in this city last winter) to find the sick mother with a family of children lying close into her, and the husband on the same apology of a bed—the whole number making no change in any of their habits by night, although the sick woman might be wandering in delirium, and exhaling poison at every breath.

The result is certain and rapid. The whole family sickens, and kindly neighbours coming in to render assistance carry the disease to their own families. So, in a never-ending line, until the whole neighbourhood has been infected, or until genial weather tempts the poor folk to live out of doors, or to tramp off into the country in search of field-work, the disease creeps surely on, and for every case left to be treated in his own home we number twenty victims.

Speaking from personal experience of an infected district, I have no hesitation in saying that, with full command of means of relief, it is impossible even to check the spread of the malady, unless each patient is removed in the first week of his illness.

You may treat the individual cases with a fair measure of success, but isolation is the only effectual remedy that will stop the spread of typhus in an overcrowded district.

We arrive, therefore, at the point that isolation is necessary and effectual, and we are immediately met with a great discrepancy of opinion as to the hospitals in which such typhus patients should be placed—some medical men recommending that they should be scattered amongst other sick people in the wards of a general hospital, others that they should be treated in wards or in hospitals specially devoted to their reception.

Now, it so happens that there are no two hospitals of any size in the United Kingdom in which a perfectly fair comparison could be made, inasmuch as in none is there precisely the same cubic space in the wards, with the same lateral space between the beds, the same area between the feet of the beds across the wards, or the same amount of ventilation.

Results of treatment in various hospitals, therefore, will be somewhat modified on this account.

Dr Bristowe and Mr Holmes, in their report, gave a useful admonition with reference to the death-rate of various institutions.

They say, "The great differences in our hospitals depend, as far as we can see, upon the different kinds of cases which they receive, and this upon the different systems on which they are founded."

Of course, it makes a vast difference in the death-rate of an hospital if all infectious diseases, and all chronic incurable diseases, are excluded; and a comparison, therefore, founded on the death-rate, between the largest metropolitan hospitals, and most of those in the provinces, is absurd.

But the death-rate seems to me a fair test for comparison in treating only of certain special diseases admitted into hospitals of various kinds. Typhus cases admitted during the same epidemic into the London hospitals will not be likely to differ very materially from those occurring at the same time at Bristol, Glasgow, Cork, or Dublin. And however anxious the staff of an hospital to which a medical school is attached may be to gather in the worst cases for clinical work in their wards, it is not practically very possible to pick and choose in a disease like typhus. I question, therefore, whether any of the arguments against the fairness of the death-rate test in general diseases in different hospitals will hold when we are considering only contagious fevers.

It may be said that the question has been for ever set at rest by the report to the Privy Council above quoted. Men of great experience have, in the most dispassionate manner, examined the records of various institutions, and have come to the conclusion which we give below. If we venture to differ from their conclusion in any way, it is that we think that this conclusion is not in every respect a fair inference from the data they have collected with so much care, and that our Bristol experience is opposed to their views.

We quote only their opinion with reference to contagious fevers : "Many of our London, and a few country hospitals, admit all febrile diseases, except smallpox, and that such diseases may be treated without excessive danger in the wards of an ordinary hospital their experience sufficiently shows ; but the great majority of country hospitals, which admit fever, place such cases either in a separate building or separate wards, from which smallpox is almost always, and scarlet fever is often, excluded. We do not mean to say that no patients have ever caught fever from other patients in the wards of these hospitals. At the time of our visit to the Middlesex Hospital, two patients were suffering from scarlet fever caught in the hospital. Scarlet fever is so contagious that patients, wherever they are, will sometimes propagate the disease, and this happens in hospital wards as elsewhere ; but it seems to us that it happens far more rarely in hospital wards than elsewhere.

"Now, patients with scarlet fever must be treated somewhere, and we believe that they are much less likely to spread the disease if treated in a general hospital than if kept at home.

"In Scotland, the honourable distinction of the hospital system is that, at the smallest as well as the largest, every disease is received, but we believe the system of separate wards for fever and smallpox is universal. This system of separate fever wards is one which deserves very careful consideration. It is intimately connected with the question of the risk of contagion, and this, there can be little doubt, varies very greatly for different diseases. Thus, if smallpox be received into any part of a general hospital, there is much danger of its affecting patients even in other parts. On the whole, as far as our present experience goes, it appears unjustifiable to expose patients in a general hospital to the risk of having cases of smallpox amongst them.

"It is otherwise with the other exanthemata. Scarlet fever is received at the above-mentioned London hospitals, and, as far as we know (having long experience of two of them), without any detriment ; nor does it appear certain that typhoid fever has manifested any tendency to spread among the patients. With typhus the case is otherwise.

"In the case of separate fever wards, however, or fever houses, the evidence is not of this negative description. Sad experience has shown how extremely dangerous such places are to the medical and other attendants on the patients, and to the patients themselves. During the short time we have spent on this inquiry, and amongst the comparatively few hospitals which comprise a separate fever house, or fever wards, we have visited no less than three in which the resident medical officer had just died of fever caught in the wards, while in others, the house-surgeon, and other residents, had been attacked, but recovered.

"It seems, then, on a review of the evidence we possess, that if in a general hospital fevers of all kinds be scattered about in the wards,

there is some risk of the spread of fever to the patients; if they be collected into separate wards, there is great danger to the medical attendants. In either case, the nurses will be in danger, and, of course, in much greater danger under the latter system. It appears to us that, in endeavouring to point out the most prudent course to pursue, it is necessary to distinguish between different cases. When typhus fever is so prevalent that, if it were indiscriminately admitted, the wards would become crowded with it, it appears that even the best constructed hospitals will be poisoned by it, and become unfit for the purposes of a general hospital.

"Otherwise—that is, when the proportion of cases of fever to general cases is small—there seems no great risk of admitting them, if the hospital is well ventilated, and the beds a sufficient distance apart, and if patients, who are well enough to go about, are carefully kept away from the fever beds. We believe that, if our country hospitals were of perfect construction, a moderate number of fever cases, say one or two in each ward, according to their size, might be admitted without danger; and that this plan would be far preferable to that of special fever wards, by which so many valuable lives have been already sacrificed. But in order that fevers may be safely received, the general arrangements and ventilation of the hospital must be very different from those which we have described at many country hospitals, where, no doubt, the more prudent course has been adopted in excluding these diseases altogether. It must be remembered, however, that the cases must be treated somewhere, and if the hospital be well constructed, it ought to be a far more fit place for the treatment of disease than a poor-house infirmary or a crowded cottage." (Report, p. 470.)

Again, "Our own impression, from what we have seen in the various hospitals visited, inclines us to believe that scattering the cases is the safest course, if the hospital be spacious, and the applications not too numerous; and in contrary circumstances that they should be placed in separate wards, the patients with typhus and scarlatina separated from each other, and, of course, from the rest of the patients.

"At some hospitals, where separate fever wards are used, the beds are placed very wide apart; and this, as far as we have been able to learn, is a successful plan. But perhaps a better one is to place the cases in a general ward, removing contiguous beds, if it appear necessary, as is done at King's College Hospital.

"We have thus far endeavoured to show that the establishment of separate fever wards and fever houses is, in ordinary times, unnecessary." (Report, p. 473.)

It must be remembered that the admission of typhus patients at all, either into general or special hospitals, will only be an occasional requirement for most parts of England and Scotland.

In these countries, the disease is nowhere endemic, if we except London itself, and perhaps Glasgow, and in the metropolis in ordinary years typhus exists to a very small extent.

It is, therefore, scarcely necessary to enter much into the question of what would do for ordinary times.

It is where the disease has taken such hold of a locality that it forces itself upon the notice of the medical men as a body, that the question of where to treat the cases becomes the subject of discussion.

And not only so, we have throughout the United Kingdom to make the best of hospitals already existing. It is not whether, in a locality in which no hospital exists, it would be well to build both a general and a special hospital, or one which, in point of space and arrangements, would be able to accommodate all the acute disease of the district, including contagious fevers. The hospitals are already built, and, speaking generally, are very fairly adapted to their present requirements; but we must endeavour to adapt them for typhus, as is suggested in the Privy Council Report. Suppose we admit one or two cases into each medical ward, or, as the better plan, remove contiguous beds.

What is the result, even granting that this may be done (which we by no means do grant) with perfect safety? Take as a fair specimen of a large hospital, not metropolitan, our Bristol Royal Infirmary. Our experience of last winter, to which I shall refer immediately, tells us that the admission of two cases into each ward, such ward being full of other patients in the ordinary way, is fraught with extreme danger to the fever patients themselves, to the other sick people in the ward, and to the nurses. Whereas, if we remove contiguous beds, or at least as many as would ensure 1750 cubic feet of air to each patient, we are interfering with the benefit of the Infirmary to the extent of nearly one-half. Instead of treating 14 cases of general disease in each ward throughout the winter, at which time our medical wards are usually very full, we should only be able to treat 8, and the medical cases in the institution would have to be diminished from 112 to 64.

Would this be benefiting the largest number? Would it not rather be cutting off the advantages of a sojourn in the Infirmary from a very large number of candidates for such relief, for the possible benefit of a few cases of typhus that would never at any one time exceed 16.

And this point of view begs the whole question of safety to the other patients left in the wards with these fever cases. The reporters seem to think that, with certain conditions of space, there is no danger under proper hospital management, and where the convalescents from general disease are kept from the fever beds, and the fever convalescents from mixing with other patients. But how is this to be done? Under what system of surveillance by the nurses are patients to be wholly kept from approaching the bedside of other cases? They are fearless through ignorance, and it is not only that they should not go near the fever beds, but that two main portions of the ward, one at each of its extremities, including a space round the two beds, must be avoided. In practice, such a rule becomes impossible.

Again, when the typhus cases are convalescing, at which period, I believe, they are more contagious than at any other, what are we to do with them when they are once out of bed? They must either be in convalescent wards without any other patients, or they must sit all day on their own beds.

Again, how is it possible to prevent the nurse from carrying contagion from one bed to another? Must she wash her hands every time she has been attending to a typhus patient (one who, we must remember, will often require attention every ten minutes) before she can perform any of her other duties to patients in her wards? Even if she did, would she not be extremely liable to take the infection in her clothes in her necessary intercourse with the other occupants of the ward?

I cannot help thinking that the reporters have omitted to go into the practical detail of the working of a ward, when they speak of the ease with which their recommendations can be carried out.

Considering, therefore, that there is scarcely an hospital in the kingdom of a perfect construction, perfect at least in the way of being fitted for the treatment of contagious diseases at all, I am only able to agree with the report, to the extent that the treatment of such diseases might be undertaken better in general wards than in a crowded cottage.

So few hospital reports mark the difference between typhus and typhoid fevers, that statistics are often very valueless. At the same time, data for some sort of comparison may be obtained, and I gratefully acknowledge the kindness of a large number of medical men in sending me answers on this subject.

Out of 97 applications, I have received answers, more or less valuable, from 74.

Dr Murchison's facts are so well known as to need no more than a passing reference here, and go far to prove the advantages of special hospitals for the treatment of contagious fevers.

Taking the present epidemic, I have collected a few facts from some of the larger hospitals of the United Kingdom.

In the *North Staffordshire Infirmary*, contagious diseases are mingled with others in general wards. Fever has spread to patients in the house and to nurses. In the last few years, of 15 typhus cases, 8 died. The cubic space is 818 cubic feet; the lateral space between the beds is 4 feet.

In *Sir Patrick Dun's Hospital*, Dublin, typhus is said to spread rarely, and the mortality from 1861 to 1865 was 47 cases in 425, or about 11·1 per cent. Here, however, the cubic space is 1864 cubic feet.

In *Bristol Royal Infirmary*, typhus was admitted into the wards during the winter of 1864-65. Two cases were placed in each ward. Forty-eight cases were treated altogether, of whom 12 died, or 25 per cent. Of these 48, 31 were cases admitted from without, and of these 7 died. Seventeen contracted the disease in the Infir-

mary. Of these 17, 11 were nurses, and 5 out of the 11 died. The space between our beds is very small, and the cubic space, most of which is from the height of the wards, is only from 1035 to 1057 per patient. In the face of such an experience, is it to be wondered at that the physicians of that institution have made a rule by which typhus cases are for ever excluded from the wards of the Infirmary?

In *Bath United Hospital*, cases are also mixed. Fever is said to spread sometimes. Indeed, in 1862, the hospital authorities recorded a most interesting series of examples of typhoid spreading in the house. Before, however, we theorize too much upon this, we should remember the fact, not mentioned in the Privy Council report, that at the time they were opening a large drain which ran under the hospital, and which, therefore, had been receiving the excreta from their ordinary typhoid cases. The spread of the disease may therefore be explained without having recourse to the view of personal contact with typhoid cases in the wards. During the last year only 4 cases of typhus were admitted, and of these one died.

In *St Bartholomew's*, cases are mixed. In 1864, 19 people contracted typhus in the wards, of whom 5 were nurses. Of these 19, 5 patients and 3 nurses died. Whenever a disease is epidemic, it spreads in this hospital. In 1864, the percentage of deaths from typhus was 29·76; but taking the three last years together, it was 23·48. The cubic space varies from about 1400 to 1800 cubic feet.

In *King's College Hospital*, under similar conditions, the disease spreads occasionally, and the late percentage of deaths from typhus has been 26·7. Dr Beale, however, assures me that the cases are picked out as severe, and I have no doubt typhus cases of great severity are admitted into this hospital. But, after all, so few cases in the second week of typhus are not severe, that I cannot help thinking that the severity depends more on the period of illness in which the patients are admitted than on any inherent tendency in them to do badly. With a cubic space extremely good, varying from 1800 to 2500 cubic feet per patient, the mortality is high. I do not see that it is possible that King's College Hospital can admit a larger proportion of grave cases than the Fever Hospital, St Bartholomew's, or our Bristol institutions, and of the latter we have seen that one, the Royal Infirmary, lost 25 per cent., whilst another, the Temporary Fever Hospital, of which we shall have to speak, lost 8·82 per cent.

In the *Edinburgh Royal Infirmary*, typhus was at one time admitted into the general wards, but the consequences were so disastrous that the present arrangement, by which such cases are carefully isolated, was made.

It is worthy of remark that, all through the United Kingdom, the rule for the exclusion of typhus from the general wards of hospitals is almost universal. Whether our predecessors had wiser views on

the subject of contagion than many medical men have at the present time, or whether, as has lately happened in our own case at Bristol, they had learned the necessity of such a rule by bitter experience, the fact remains that typhus cases are either excluded altogether, or admitted into separate wards or separate buildings, except in some few of the metropolitan hospitals, and some few out of London, which may be counted on the fingers of one hand.

Not only is this the case in those parts of England in which typhus is a rare visitant, but much more specially in those localities of Scotland and Ireland which are never wholly free from this scourge.

At the *Greenock Hospital* in 1847-8, fever spread among the surgical patients, and attacked nearly all the nurses and other officials.—(Report, p. 699.) At the time referred to, the infirmary and surrounding neighbourhood were all badly drained, and the fever wards occupied the lower floor of what was at that time the general hospital. At the present time the fever portion of the hospital is perfectly divided from the general wards.

At Aberdeen, at Arbroath, at Dumfries, where typhus is rare, at Dundee, at Edinburgh, at Glasgow, at Montrose, at Paisley, and at Perth, indeed I believe I may say at every hospital of any size in Scotland, the separation of typhus is strongly insisted on, and is carried out practically and with success. Moreover, this success seems to be in direct ratio with the care in isolation, as the reporters, speaking of the Montrose Infirmary, say that in 1855, when many fever and smallpox cases were admitted, patients recovering from smallpox took fever, and conversely.—(Report, p. 700.)

In Ireland,—the very home and nursery of maculated typhus, the country most rich in lamentable experience, and from the medical profession of which, in the person of the late Dr Graves, we have derived more valuable information with reference to its treatment than from any other source,—I believe I may say there exists but one hospital, Sir Patrick Dun's, in Dublin, in which typhus cases are mixed in the same wards with patients ill of other diseases. In the numerous county infirmaries throughout the country, fever cases are either excluded, or, as at Londonderry, Limerick, Meath Hospital, and Dr Steevens', Dublin, admitted into separate wards. In some localities, there are separate hospitals for the reception of such cases, as in Dublin, Cork, Belfast, and Galway; or they are sent to fever wards in connexion with union infirmaries, as at Wexford, Omagh, etc.

It is hardly a point to be passed over lightly that, in Ireland, where the contagious properties of typhus can always be tested, and where the experience gained about it is not of an occasional epidemic, but of a disease always endemic in many localities, the universal practice obtains that, under all the difficulties and dangers attending the treatment of this disease, in the choice of evils consequent on the very nature of the question, it is considered by far the lesser

evil to treat typhus cases in separate wards or separate hospitals. It is not a question that resembles that of treatment.

It is utterly distinct from theories which rule points of practice, the giving or withholding stimulants, the feeding or depleting fevers, —points that may depend on the fashion of the day, or the teaching of a particular school. It is distinct even, as the arrangements of some hospitals show, from the commonly received views both as to ventilation and cubic space. Yet the fact remains that, taught by bitter experience, the Irish mind is so imbued with the knowledge of the contagious properties of typhus, and so certain of the risk of admitting it among patients in the general wards of an hospital, that, with one exception, no hospital in Ireland will admit such cases except under the strictest provision for their isolation.

The evils consequent on the treatment of typhus cases are threefold,—

1. Under certain conditions a larger number of them may die than under other.

2. In general wards, other patients may become infected.

3. The nurses and other attendants may contract the disease.

It is part of the present inquiry to look at each of these three points.

Taking, as we said before, the present epidemic as a standard of comparison, and speaking only of those hospitals in which a distinction is made between typhus and enteric fevers, we are enabled to find some data, though unfortunately not very many, that may throw light on this question.

I class together, then, special fever hospitals, and those hospitals to which separate fever wards are attached, although of the latter, those which possess fever wards built distinct from the rest of the institution, and not under the same roof as the general wards, seem to me to be safer as to the spread of the disease.

Of Special Fever Hospitals we have—

	Date.	Death-rate.
London Fever Hospital, ...	1864	17·76 per cent.
Newcastle-on-Tyne Fever Hospital, ...	1864	16·19 „
Glasgow Fever Hospital, ...	1865	12 „
Belfast Hospital for Contagious Diseases, ...	1864	11 „
Dublin Fever Hospital, ...	1864	8·75 „
Temporary Fever Hospital, Bristol, ...	1865	8·82 „
Temporary Fever Hospital, Cork, ...	1864	6·5 „
Netherfield House, Everton, Liverpool, ...	1863, 4	8·7 „
Leeds House of Recovery, ...	1864	Uncertain for typhus; 13·6 for all diseases.
Carlisle House of Recovery, ...	1864	30·2, typhus & typhoid.
Leicester Fever House, ...		Typhus very rare.
Omagh Workhouse Fever Hospital, ...	1864	Uncertain for typhus; 10 for all diseases.
Cork Fever Hospital, ...	No return.	
Galway Fever Hospital, ...	No return.	
Wexford Workhouse Fever Hospital, ...	No return.	
Arklow Fever Hospital, ...	No return.	

Of Hospitals with Special Fever Wards we have—

	Date.	Death-rate.
Aberdeen Royal Infirmary, ...	1864	11 per cent.
Dundee "	1864, 5	11·55 "
Glasgow "	1864	12·33 "
Greenock General Hospital, ...	1862	10·2 "
Bristol Union Workhouse, ...	1864, 5	12·0 "
London Hospital, ...	1861-4	30·0 "

The Hospitals mentioned below have also Special Fever Wards, but I have been unable to obtain any special data for Typhus. These are—

	Date.	Death-rate.
Stafford General Infirmary, ...	1864	29·5, for all fevers.
Birmingham General Hospital, ...	1864	No typhus.
Birmingham Queen's Hospital, ...	1864	No typhus.
Sussex County Hospital, Brighton, ...	1864	No return for typhus.
Bedford Infirmary, ...	1864	Mortality low for all fevers.
Dumfries Infirmary, ...	1864	No typhus.
Derby Infirmary, ...	1864	15·15 p. cent. for all fevers.
Arbroath Infirmary, ...	1862	15·2 "
Perth Royal Infirmary, ...	1864	8·35, typhus and " typhoid.
Paisley Infirmary, ...	1862	11·7, typhus and typhoid.
Limerick County Infirmary, ...	1864	5·0, all fevers.
Londonderry County Infirmary, ...	1864	7·0 "
Cashel County Infirmary, ...	1864	5·0 "
Meath Hospital, Dublin, ...	1861, 2	9·0 "
Dr Steevens' Hospital, Dublin, ...		Few fever cases apply.
Edinburgh Royal Infirmary, ...		No return.
Manchester Royal Infirmary, ...		No return.
Cambridge Addenbrooke Hospital, ...		Typhus rare. No returns of death-rate.

Nothing is more embarrassing in a question of this kind than the different opinions held as to the identity or non-identity of typhus and typhoid fevers, and the various arrangements which obtain in different hospitals.

Thus it is an exceptional circumstance to find these diseases classified as distinct maladies; in many of the British hospitals, and in the Irish, except perhaps at Belfast, at one institution at Cork, and perhaps one or two in Dublin, the distinction is entirely ignored.

In the central portions of England, typhus is almost unknown; even in Birmingham the disease is scarcely met with.

Whether, as Dr Graves suggested, there is some climatic influence, favourable to typhus, peculiar to the shores of the Irish Channel; or whether the disease only exists in England by means of individuals who have brought it from Ireland, and therefore affects mainly, if not solely, great Irish ports, like Liverpool, Bristol, and London, and ports lying very near such localities; or cities such as Newcastle-on-Tyne and Carlisle, whither the malady may be easily brought from the Scotch cities,—it is an undoubted fact that, from year to year, large towns, the centre of industry and

manufacture, and subject to all the drawbacks of such conditions, in the way of dirt, overcrowding, and an extension of habitations, for which the drainage system soon ceases to be commensurate, are perfectly free from this disease, though possessing, in a marked degree, the elements for its spontaneous origin, if the disease was apt to originate at the present day in this manner.

The fact, however, of its rarity in many places makes a comparison between the arrangements of the hospitals in which it is treated more difficult, because the data are really so few.

If, however, we take the small data that exist, and exclude all that are doubtful in the way of diagnosis, we find of special hospitals, the London Fever Hospital, Newcastle-on-Tyne Fever Hospital, Glasgow Fever Hospital, Belfast Fever Hospital, Dublin Fever Hospital, Temporary Fever Hospital, Bristol, Temporary Fever Hospital, Cork, and Netherfield House, Everton, Liverpool. In these, the mortality in 1864 varied from 6·5 per cent. in the Temporary Fever Hospital, Cork, to 17·76 per cent. in the London Fever Hospital.

From want of returns, or of distinction between the two diseases, we are unable to give the percentage at the Cork Fever Hospital, the Galway Fever Hospital, Omagh Workhouse Fever Hospital, Wexford Workhouse Fever Hospital, Arklow Fever Hospital, Leeds House of Recovery, or Carlisle House of Recovery.

In Leicester Fever House, typhus is said to be very rare.

If we add the percentage of these first fever hospitals, and divide it by the number of institutions, we find a death-rate slightly exceeding 11 per cent.

We proceed to those institutions in which fever wards exist, but more or less isolated from the general wards of the hospital; and of these we have no return from the Manchester Royal Infirmary, whilst at Arbroath, Perth, Paisley, Limerick, Londonderry, Cashel, and Meath Hospital, Dublin, the two fevers are classed together.

As at Cambridge, Stafford, Derby, Birmingham, and Bedford, the disease is almost unknown, or at least since these towns have in no way suffered from the present epidemic, we are reduced to a very few institutions. These are,—the Glasgow Royal Infirmary, the Dundee Infirmary, the Aberdeen Infirmary, the Greenock Hospital, the Bristol Union Workhouse Hospital, and the London Hospital; and we are tempted to exclude the latter, partly because the return includes only 20 cases in four years, and partly because there are no wards in the London Hospital specially set apart for the reception of typhus cases, such patients being treated in the attics.

Taking, then, the first five of these hospitals, we find the death-rate to vary from 12·33 to 10·2 per cent. for typhus, and taking the average of the five hospitals, the percentage will be 11·41. Even including the London Hospital, the percentage will only be 14·5.

Comparing these results with those of hospitals in which typhus

cases are or were treated in the general wards, the North Staffordshire Infirmary, Bristol Royal Infirmary, Sir Patrick Dun's, Dublin, St Bartholomew's, King's College Hospital, and Bath United Hospital (we exclude the latter, because the number of cases in the return is so small), and we have five institutions with a typhus death-rate varying from 53·3 per cent. to 11·1 ; the five united giving an average death-rate of no less than 29 per cent.

I know that much may be said of the uncertainty of statistics, of the small number of hospitals referred to, of the poverty of the data, but such arguments will tell equally against each side ; and I am inclined to believe, from death-rate statistics, that, under average hospital conditions, the treatment of typhus in isolated wards, or in separate buildings, is decidedly more successful than in the general wards of an hospital.

This being so, we come to the second point : " Under which system are typhus cases most likely to infect other patients ? "

This point scarcely requires the slightest consideration.

It is evident that, in separate hospitals for contagious diseases, there are only convalescents from other fevers that can be liable to this accident. This is merely a matter of hospital arrangement ; and if the principle of isolation is admitted, as it is in most parts of the United Kingdom, the precautions for preventing such an occurrence in fever hospitals become merely matters of detail.

Thus, in the infirmary of the Bristol Union Workhouse, where contagious diseases are admitted into the upper storey, but cannot be completely isolated from each other, two or three convalescents from typhoid have this year contracted typhus there, and convalescents from typhus have taken smallpox, but there has been no instance of the disease spreading to the ground floor.

At the Derby Infirmary, where such cases are excluded by rule, but are isolated as far as possible if they are admitted accidentally, it has occasionally spread ; so to convalescents at the Leeds House of Recovery ; at the Newcastle-on-Tyne Fever Hospital it has very seldom attacked patients ; at St Bartholomew's it spreads whenever it is epidemic ; at Guy's rarely ; at King's College Hospital occasionally ; at Nottingham General Hospital, where syphilitic patients are admitted into fever wards, typhus has spread to some extent ; at the North Staffordshire Infirmary, with fever patients mixed indiscriminately with others, and a cubic space of only 818 cubic feet, it has spread to patients ; at Sir Patrick Dun's rarely, if ever ; it has spread at the Wexford Union Fever House ; and out of 48 cases in the Bristol Royal Infirmary, 8 contracted fever in the house ; whereas, at the London Hospital, at the London Fever Hospital, at the Glasgow Royal Infirmary, and Glasgow Fever Hospital, at Belfast Hospital for Contagious Diseases, at Cork Fever Hospital, at Limerick Fever Wards, and Londonderry Fever Wards, at the Cashel County Infirmary, the Omagh Union Fever Hospital, the Bristol Temporary Fever Hospital, and at Netherfield

House, Liverpool, patients have not contracted the disease in the house; whilst at Perth there has been no case of its spreading to patients in the hospital since isolation has been more completely carried out.

Doubtless, in some cases, the amount of cubic space, and the excellence of ventilation, have modified results, but at St Bartholomew's, the space in the wards in which such patients are mixed with others, amounts to 1800 cubic feet, and at Bristol Royal Infirmary to 1040, and even more; whilst at others, in which the disease has not spread, and especially in some of the Irish hospitals, the cubic space has not exceeded 600 cubic feet.

At the Meath Hospital, Dublin, "Dr Stokes affirms that, notwithstanding that the fever cases are accommodated in the same building with the other hospital patients, and even on the same floor with some of them, fever has never been known to spread, not even to medical men, to pupils, or to nurses" (Report p. 783); but, then, in this hospital fever cases are treated in separate wards.

Since complete isolation has been carried out at Greenock Hospital, infectious diseases never spread from one house to the other.

At the Edinburgh Royal Infirmary, an attempt was made some years since to treat fever in the general wards, with the precaution of withdrawing contiguous beds, the very plan of which the report to the Privy Council speaks with approval, but it was abandoned in consequence of fever spreading in the wards. At the present time, "it has not been established to the satisfaction of the medical officers that the disease has been contracted in the house in any instance." The rules for the prevention of any such mischief are here very strong. "The fever wards are kept separate from the rest of the house, the door of communication between them and the general hospital being locked. The house-surgeon has orders not to visit the general hospital for half-an-hour after he has been to the fever wards."—(Report, p. 691.)

At Dundee, the present resident medical officer, Dr Maclagan, writes me word, that generally there is no tendency on the part of contagious diseases to spread amongst the patients, but the report, so often quoted, states that "several patients admitted with other maladies have been attacked with typhus. This has occurred chiefly, of course, in the fever wards, but a few cases of typhus have originated also in the general wards."—(Report, p. 690.)

I suspect that such an occurrence taking place when the cases are isolated, is due to the difficulty of keeping nurses quite away from the other parts of an hospital.

At the Arbroath Infirmary the fever wards are not cut off from the rest of the house, but fever cases are placed by themselves, and are not mixed with general cases, and here fever does not spread.

Judging therefore from the data afforded by various hospitals, it is evident that the disease affects other patients when it is brought into the same ward with them. That this danger should be acknowledged, it is not in the least necessary to deny that the infectious property of typhus radiates in a well-ventilated ward only to a certain definite distance around the bed where the patient lies. This may be, and probably is the case. But it will always be practically impossible to arrange a general ward, especially in its details of nursing, so as to prevent the poison being carried by nurses or patients to other beds, and still more difficult will it be to do so when the fever cases are allowed to get up, and to mingle with other patients, either in their own or in convalescent wards.

Judging also from these data, we should say that next to a general mingling of cases, patients ill with other diseases are more likely to contract typhus from typhus cases in an hospital in which the fever wards, though isolated, are under the same roof as the other wards, and that an entirely separate building is the safest course; the only evil of the latter place being, that convalescents from typhoid, smallpox, or scarlatina, may contract typhus, and *vice versa*, if they are allowed to mix together, or if the nurses of one ward are not restricted from entering the others.

It is, however, only fair to point out that Sir Patrick Dun's Hospital, Dublin, seems an exception in this respect. It admits all contagious diseases, and mixes them in general wards, and Dr Moore considers that typhus and allied diseases rarely, if ever spread. Its cubic space, however, is magnificent, amounting to 1804 cubic feet per bed.

And again, in our opposite experience, at the Bristol Royal Infirmary, it is to be noted that the lateral space between the beds is lamentably small, in some wards not exceeding 2 feet.

The third point is, practically, a very serious one; the liability of medical attendants, nurses, etc., to contract the disease.

At Liverpool Royal Infirmary, where contagious diseases are not admitted, except accidentally, and where, if so admitted, they are not isolated, the nurses suffer.

At the Hull General Infirmary, in 1864, a nurse contracted typhus from the only case of that disease admitted that year. The case was treated in a general ward.

At St Bartholomew's, in 1864, 5 nurses contracted typhus, of which 3 died.

At the North Staffordshire Infirmary, the nurses have suffered from this disease.

At the Bristol Royal Infirmary, 11 nurses took it, of which 5 died.

At the London Hospital, Dr Woodman writes, that in the last 4 years, of 20 cases of typhus 10 were nurses.

At the Newcastle-on-Tyne Fever Hospital, typhus invariably affects the nurses and attendants sooner or later.

I am informed by one of the resident medical officers of the London Fever Hospital, that the nurses invariably take typhus fever if they have not previously had it. And with reference to others, we may quote the Report for 1864 of this excellent hospital, in which it is stated, "out of 83 attendants on the sick (including laundry-women), either permanently or temporarily engaged during the year, 21 caught typhus, viz., the assistant resident medical officer, the matron, the engineer, who had been attached to the hospital for 16 years, and who had never before had fever, although his duties brought him daily into the wards, and 18 nurses. The committee regret to report the death of the engineer, and of 4 nurses from typhus."

Of the Dundee Infirmary, Dr MacLagan writes, "any attendant on typhus cases, unprotected by a previous attack, is almost certain to catch the disease if much among the patients. Since the opening of this building 11 years ago, there have been 5 superintendents. The first had typhus, and recovered. The second had typhus, and recovered; he afterwards died of enteric fever in 1863. Three months afterwards the third died of typhus, having only held office a few weeks. Within three months after this the fourth died of typhus. The fifth (myself) was protected from typhus by a previous attack, but had enteric fever last summer (1864). At this moment, October 1865, my assistant is down in typhus, so that not one resident medical officer has escaped. About the time of the death of the fourth superintendent, the matron died of typhus."

At the Bristol Union Workhouse, Mr Grace informs me, that in the past year 6 nurses have taken typhus, and all have died.

Of the Edinburgh Royal Infirmary, quoting from the Report of the Privy Council, we find that, "unless where fever prevails extensively in an epidemic form, the nurses and medical attendants rarely suffer. During the year ending September 30, 1863, however, one of the resident officers in attendance on the fever wards caught fever and died. One of the nurses also had fever, but recovered."—(Report, p. 693.) I hear, however, that this year some of the students have contracted typhus.

At Montrose, in 1855, the assistant surgeon, the matron, the fever nurse, her 2 assistants, and the cook, all contracted fever, and some had it severely.—(Report, p. 700.)

At Paisley, it was reported, in 1862, that recently the house-surgeon and one of the nurses have caught fever.—(Report, p. 702.)

At the Hardwicke Fever Hospital, Dublin, where the majority of the cases are fever, the disease does not spread to the officials, neither does it at the Meath Hospital, or Dr Stevens'."—(Report, p. 708.)

At the Manchester Royal Infirmary, it was said, in 1862, that "lately 3 nurses and 1 of the physicians have caught typhus, and the resident medical officer has just died from the same disease contracted in the discharge of his duties."—(Report, p. 616.)

At Guy's Hospital, in 1862, of 13 nurses and assistants tending the typhus cases, 4 fell victims to the disease.—(Report, p. 580.)

At St Mary's Hospital, in 1862, the Report says that "there is an unusual spread of infectious fever, and one or two nurses died from typhus."—(Report, p. 588.)

At Middlesex Hospital, in 1862, 2 patients and 2 nurses died from typhus, apparently contracted in the wards.—(Report, p. 590.)

At the New St Thomas's, in 1862, 1 nurse had contracted typhus and died of it. In 1858, out of 11 cases 2 were nurses, who died.—(Report, p. 593.)

At Westminster Hospital in 1862, typhus had spread, and some nurses and officials were attacked, but none died.—(Report, p. 596.)

These are the few data I have been able to collect on this subject; and the results at first sight seem to point to the great danger to attendants of treating typhus in institutions specially devoted to this purpose. But if these facts are examined more closely, they show that wherever typhus cases are treated at all, those who attend on them are liable to contract the disease.

The general hospitals in London and elsewhere which admit such cases, do not suffer apparently so much as special fever hospitals, because the number of cases they admit is comparatively so small. But really they suffer in at least an equal proportion, considering the number of cases.

At St Bartholomew's, Dr Andrew tells me, that from what he has seen of typhus for the last 3 or 4 years, he is certainly of opinion that there ought to be special fever wards.

If at St Bartholomew's in 1864, with 84 cases of typhus, 5 nurses caught the disease, and if at the Bristol Infirmary, with 48 cases, 11 nurses suffered from it, out of 16 who attended on the wards in which these patients were, the proportion does not show particularly well in contrast with the London Fever Hospital, in which, in 1864, 21 persons contracted typhus, whilst 2497 cases were treated. In the first case, it is 1 nurse affected to every 17 cases of fever; in the second, 1 nurse to every 4 to 5 patients; whilst in the London Fever Hospital, it is 1 official to every 119 patients.

Again, looking at the question in another way, in fever hospitals there is not only often a greater cubic space, but the regulations for the health of the nurses are better. In some they are allowed better food, and, probably in most, more open-air exercise, than they get in general wards.

Of course, the experience of the Dundee Infirmary, with reference to the resident medical officers, is harrowing in the extreme; and it is satisfactory, by way of contrast, to mention our Temporary Fever Hospital at Bristol. Here, in addition to a house-surgeon and some women who had had typhus, were 8 nurses, a matron, 2 servants, and 2 physicians, who had not had it. The only official who contracted typhus was the matron, and 102 cases were treated.

A fact like this is the more satisfactory, following immediately after our experience at the infirmary, and still more after the terrible results which ensued in our attempt at house-to-house visitation in the district in which the disease was then most rife. There, in the first week of our operations, a scripture reader and a trained nurse contracted the fever, the former dying of it. During the next few months a band of from 20 to 30 poor women were organized to tend the sick in their own homes, and all of these, with scarcely a single exception, took the disease.

Considering, therefore, that the patients have to be treated somewhere, that those who attend upon them in their own homes almost invariably take the disease, that where such patients are admitted into a general hospital the nurses contract it in direct ratio with the number of patients admitted, and that in special fever hospitals, the number of officials attacked, though apparently large, is in reality small compared with the number of patients treated, we cannot avoid the conclusion, that, even on this ground, the treatment of typhus in special hospitals is most advantageous, if only we look at the question as involving the doing the greatest good to the greatest number at the least sacrifice of human life.

We approach the termination of our subject, and have to give the following opinions, as the result of much inquiry, and of some personal experience of typhus, although we regret that, in some respects, these opinions are opposed to those of the writers of the Sixth Report of the Medical Officer to the Privy Council.

1. It is impossible to check the spread of typhus in a crowded locality, without taking away the patients from their own homes.

2. The admission of such cases to the wards of a general hospital must impair the advantages of such an institution, by necessarily restricting the number of beds it might otherwise contain.

3. That no general hospital in the United Kingdom is fitted by its construction for the admission of such cases into the wards with other patients.

4. That no hospital regulations are possible, which could prevent the spread of typhus, if admitted into general wards.

5. That the death-rate of typhus is lower when such cases are separated, than when they are mixed with others.

6. That in the former case the tendency to the spread of the disease amongst other patients is materially diminished.

7. That by treating typhus cases in special fever hospitals, although the danger to officials is very great, yet the danger to the community is much lessened, from the fact that the number of attendants affected, compared with the number of cases treated, is very small.

8. That under certain regulations the danger to the nurses in such special hospitals may be lessened.

9. That, as a conclusion of the whole question, typhus cases are better treated in the general wards of an hospital than at

their own homes; better still in separate isolated wards of a general hospital than when mingled with other patients; best of all in separate institutions, with an entirely distinct staff of officers.

10. That in a separate institution, situated on high ground, with 1750 cubic feet of air for each patient, a large lateral space between the beds, and good ventilation and drainage, the death-rate of typhus, though varying according to the idiosyncrasy of the epidemic, may be considerably diminished.

ARTICLE IV.—*On the Treatment of Virulent and Zymotic Diseases.*
By H. HJALTELIN, M.D., Inspecting Medical Officer of Iceland.

WHAT is now-a-days our general treatment of virulent and zymotic diseases? I think for the most part only symptomatic; many specifics seem altogether amongst the great physicians in some discredit. This is no doubt the contrary state to the old maxim, especially that of the iatro-chemical school. The universalia of Paracelsus, van Helmont, and others of their followers, are now only laughed at; they had three or four great universal medicaments, viz., *iron, sulphate of copper, and nitrate of potash*, besides their many renowned organic specifics. All this is now looked upon as a mere superstition and twaddle; good for nothing; and only worthy to be buried in oblivion. The maxim of *Celsus* was,—“Non interest scire, quid morbi sint, sed quo modo curentur.” The maxim of our time seems to be exactly the reverse of this maxim, viz., “interest scire, quid morbi sint;” and it is from this principle that the many microscopical investigations and the most exact chemical experiments on dead bodies have been instituted of late with the utmost accuracy.

Alas! poor sons of *Æsculapius* as we are, what is come out of all this? The French physicians have some time ago told us, that the Roman physicians, two thousand years back, were fully as able as we are now, to cure diseases, and a very ingenious and able German physician has some years ago told us, that we in vain laughed at the old alchymists, who, according to his view, knew very well what they intended, and had more correct ideas of physic than we are inclined to believe in our time. This view is in some way supported by the great chemist *Liebig*, who, in some of his chemical works, is looking on them with no small esteem, while he, in his last chemical letters, is speaking of our healing art with considerable contempt. In the meantime this is not all; the method of our science is now still further attacked, not only by the homeopaths, but we have the mortification to see one of the most renowned journals in the world, the *Times*, speak of our healing art, not only with great distrust, but even with contempt. “Medical men cannot tell

us," says the *Times*, "with any strictness what the type of disease is which is decimating our flocks and herds."

They are not agreed upon, whether the disease is imported from Russia, or whether it is the result of some subtle poison in the atmosphere called into sudden and virulent action by atmospheric influences. "As they differ as to the disease, so also they differ about the remedy." This is, no doubt, a melancholy state, and a frightful stroke, not only to the medical men of Great Britain, but also to the high-minded British nation.

It has fallen to my lot to have a great deal to do with some of the most frightful contagious diseases of our time, not only in men but also in animals, and I have never been in the slightest doubt what to do; my principle has always been to destroy the contagious matter by the most effective disinfecting compounds, using at the same time such specific remedies as I have thought most fit according to the atmospheric influences and other circumstances. I will shortly tell my experience; which, in truth, has been a very extensive one.

In the years 1848 to 1853, including the former year, I was appointed as superintendent physician to the quarantine establishment near Copenhagen; my duty was to defend the Danish capital from the threatening Asiatic cholera, which in these years often raged frightfully in the surrounding countries, especially Germany and Sweden; the cholera was then sometimes raging very near us, as in *Stettin*, *Lubeck*, and many other places in the north: the nearest infected place was, at one time, Malmöe. Steamers were then running from and to the quarantine establishment in twelve to twenty hours, and many passengers came to the quarantine, not only with severe symptoms of choleric diarrhoea, but also with the first symptoms of the advancing disease itself. Disinfecting compounds, especially chlorine gas, were daily made use of in the most consequent manner, while I, at the same time, stopped every diarrhoea with the most effectual remedies, amongst which I used tannic acid and sulphate of copper. No deaths occurred among the many thousands that visited the quarantine in the aforesaid period, and no cholera was then introduced into the capital of Denmark. During the winter 1853, the quarantine for the kingdom of Denmark was abandoned by law, according to the advice of the Diet (*Rigsdag*); but about four months afterwards, the Asiatic cholera made its first appearance in Copenhagen. I did not, in the beginning, trust my own eyes, because I was, with some others, accustomed to the view that cholera was, by some unknown causes, prohibited from making its ravages in the Danish capital; but soon I got convinced of the real truth, and I then saw that it was the same plague which I for some years since had seen in Berlin. The people of Copenhagen were struck with astonishment, exclaiming, "Oh! that we had not abandoned the quarantine! It has surely saved us for many years; but now, since it is abandoned, we are to have the

plague." Before the outbreak of cholera, I was determined to visit my native country, and the vessel which was to carry me sailed two weeks after the appearance of the cruel Asiatic guest. I had, some years back, written a little pamphlet on the preservative remedies for cholera, in which my opinions about some disinfecting compounds were exposed, and in which I had shown that the cholera fright was nearly as dangerous as the cholera itself. I told some of my medical brethren that the best course to take was, according to my conviction, to stop the diarrhoea as soon as it came on, to use strong disinfecting compounds, and to keep the cholera fright down amongst the people as much as possible.

When I had arrived in my native country, I had to deal with another frightful contagious disease amongst the sheep; it was a sort of plague that in some places had nearly decimated the flocks, and had lasted for some years. The Danish Government, which was aware that I had taken much interest in the veterinary science, and had for that purpose for some time visited the veterinary school of Copenhagen under the guidance of the excellent Veterinarian, Professor *Wiborg*, asked me to do what I could against the plague amongst the sheep in Iceland. After having made some autopsies I wrote a statement about the disease to the Danish Government, and this having been laid before the College of Veterinarians of Copenhagen, was afterwards printed in the Veterinary Journal, with very flattering remarks on my description of this plague. The college animated the Government to let me continue my measures. And so I continued to look for new autopsies; but I am now not a little surprised to read of nearly the same pathological conditions in the description of the raging rinderpest in England as I then found in the dead sheep here.

I had, in my aforesaid statement, tried to show that the then raging sheep disease of Iceland was in its nature a most dangerous and contagious *sheep-typhus*, which, according to my conviction, ought to be treated with effective disinfecting remedies. The chlorine gas was now again employed in the most strict manner, the sheep stables were to be filled with that vapour, and the sulphate of natron (salt of Glauber) was administered to all sheep where the plague was raging, in order to clean the intestinal canal effectually. The salt of Glauber was often mingled with sulphur (about half-an-ounce to two ounces of the salt of Glauber) with the best effect. I looked upon these remedies as true preservatives, which ought to be administered as soon as possible and given to all the flock where the disease was raging. The effect was really wonderful, and the frightful disease decreased rapidly in all the flocks where this treatment was followed. The belief in both those remedies is still so strong amongst many peasants, that I annually am getting letters from very remote parts of the island, convincing me of its reiterated efficacy. Unhappily enough, was this frightful disease after some years followed by another far less dangerous

complaint, viz., the scab of the sheep, which, in the former century, during 16 years had made considerable ravages in this island, but was supposed to have been stopped by an extensive slaughter of these animals. In the meantime, this slaughtering cure was then in some measure followed by a frightful famine, which nearly decimated the people themselves.

In the beginning of the new sheep scab my advice was asked, and my answer was, — “You must not think to stop this disease by slaughtering the sheep; nobody can know how far it already may be spread, and it is, at all events, quite curable.” I then advised the inhabitants to make a bold use of one *specific* remedy against this complaint, and this was the scab-bath of *walze*, which I had seen used in other countries with perfect success. In vain I told my countrymen this; they would not believe me, except very few of them. A general public clamour arose: “We have got an intractable disease into our country, like that scab disease of the former century: it is a great plague, quite incurable; it is come from England, like the scab plague of former century; slaughter is the only remedy.” This was the general talk. But to this I answered: “Do not disbelieve in the veterinary science; the sheep scab is quite tractable; we have a specific remedy against it. The disease is not imported; it is only a higher degree of the well-known milder scab of these animals, which, on account of the great heat and dry weather of the last summer (this was in 1855), that has been extremely much favouring the evolution of the *acarus scabiei*, has thus become an epizootic.” The governor in the south was of my opinion, but the governors of the west and north part of this island strongly opposed us, regarding a general slaughter of all affected and suspected animals as the only admissible remedy. A general slaughter of no less than 138,000 sheep then followed, the greatest part of which was only suspected, but not affected at all. The fear for the contagion amongst the people now went to its highest degree, and reminded one of that extreme fear for the Asiatic cholera contagion when letters and newspapers were believed to spread that disease to uninfected localities. In the meantime, some people who believed in my opinions also about this disease began to cure their sheep with the remedy of *walze*, and were completely successful. By this, two parties arose amongst the peasants,—the curing party and the slaughtering party. The latter party was strongly assisted by the governor of the west and the governor of the north, who, in an open letter, declared the disease to be the most intractable plague introduced from England. The matter came to the decision of the Danish Council, which, after a conference with the veterinary college, decided for my opinion. The King of Denmark himself sent his chief veterinary physician, the Professor Ischerning, a very learned and practical veterinarian, up to us, followed by the most popular Icelander, the Chairman of the Althing, and some other

veterinarians. The Danish Government ordered to stop the slaughter, and the curing principle was introduced in all places where any scab was to be found. Many of the inhabitants were reluctant against this measure, and are still so obstinate that they must be compelled to do their duty. It was now found that the *acarus scabiei* of the sheep was no new animal introduced from England into this country, for it had been found in the northern part of this island in considerable quantity for more than forty years back, and this was stated by the Professor Ischering from a very nice picture of this little animal.

The curing principle is now universally in use against the sheep scab in the southern part of this country; and although there may still be found some blockheads who are unwilling to cure their flocks, the *specific virtue* of the walze remedy against scab is established, and the sheep flocks are increasing to a considerable extent.

In the years 1858-61 a frightful contagious typhus raged all over this island, as may be seen from my description of that malady inserted in the Edinburgh Medical Journal, 1863, p. 217, and I then again took refuge in my usual disinfectant treatment, and it was executed on many hundreds of patients with great success. The belief in this treatment is grown so strong amongst the population that they look upon it as quite specific in this disease.

During this summer a dangerous puerperal fever broke out in the western part of our country. The physician there asked my advice and counsel, and I advised him to make bold use of chlorine gas in all the houses where this malady had broken out, and ordered him to have all utensils and clothes which had been in contact with the sick or their attendants washed in chlorine-water. This was done, and the physician now writes to me that the fever has been instantly much mitigated, and no deaths have occurred since this measure was brought into execution.

It would lengthen too much these few remarks if I were to expose my humble individual opinions on virulent and zymotic diseases, and I will only make the remark, that it seems to me that the name of zymotic disease is most applicable to such maladies, where we, by induction, seem justified in accepting an agent like that of fermentation, putrefaction, or of a catalytic nature. I know that the learned Mr Simon, in his admirable lectures on pathology, scouts the notion of any true fermentation in virulent and zymotic diseases; but I have always thought that the great chemist, Liebig, has only used these words analogically, to signify some still hypothetical subtlestuff, causing, like diastasis, an unusual but very destructive metamorphosis in some albuminous compounds of organised bodies. The deleterious actions of such a promoter may, according to my view, be stopped by many chemical compounds, as fermentation and putrefaction are unquestionably counteracted by chemical remedies, and, according to the empirical facts of medicine, we seem justified in calling them disinfecting remedies. They are also, in more or less degree, parasitocidia, and belong, for the most part, to

the metalloids or metals; the most effective seem to be chlorine, bromine, iodine, nitrogen and sulphur in their chemical combination with oxygen, hydrogen, and some of the metals, as iron, copper, mercury, zinc, calcium, and formyl. The chlorine gas, muriate of iron, sulphate of iron, sulphate of copper, iodoform, and muriate of zinc, have all been tried by me in several zymotic diseases, and, as it seems to me, with good success. Thus, I have tried chlorine gas, iodoform, and muriate of zinc in typhus, as well in men as in animals, with no small success; chlorine gas, sulphate of iron, and sulphate of copper in cholera, dysentery, diphtheritis, and adynamic pneumonia; chlorine gas and muriate of mercury in puerperal contagion; and muriate of iron in epidemical erysipematous inflammation. It is evident that, if the morbid cause of an infectious disease has already produced extensive pathological destruction in men or animals, then the disinfecting compounds will be of little or no use: they must be had recourse to in the outset of the diseases; for they are, for the most part, aimed against the real cause of the virulent diseases, and not against the pathological products resulting from such a cause. "*Sublata causa, tollitur etiam effectus.*"

The disinfecting remedies of vegetables are certainly far inferior to the organic compounds; among the most trust-worthy of them are charcoal, ethereal oils, camphor, some aromata, creosote, and empyreomatical oils and fluids. In miasmatic diseases, whether of an atmospheric or telluric origin, some of them, as chinine, salicine, coffeine, etc., are of great value; but that they can destroy real virulent and zymotic diseases is still very uncertain.¹ Many remedies of this great class are excellent parasiticidia, and their neurosthenic, sedative, and anti-spasmodic virtue is undeniable. The specific organic remedies of the alchymists are nearly put down by the modern systems of medicine; and it is undeniable that the modern pathological schools, whose maxim it is to leave most acute diseases to their own course, to the healing art of nature, or to the so-called expectative method, have nearly shaken down the public belief in medical agents, and made the modern followers of Hippocrates equal to the decillion doctors in public opinion. No wonder, therefore, that the homœopathists are gaining ground in reverse proportion to the followers of Rokitansky, who are losing it. Eminent physicians of our time are begun to feel the uncertain state of our science, and the excellent physician, *Thomas R. Chambers*, has, in his remarkable work, "*The Renewal of Life*," remembered this when he says,—"*To the practitioner I will urge the necessity for having firm faith in the work he is busy upon, and in the tools he uses. Without this he can look back with no conscientious pleasure*

¹ There is also one remedy which must be named here, and it is the *cold bath*, ice internally, and cold, often reiterated cold super-infusions. This remedy has often, both in epidemics and epizootics, showed its great and wonderful efficacy. It promotes the reaction of the whole surface of the body, and restores, often wonderfully, the natural heat better than warm applications.

on his daily toil; his moral nature will degenerate day by day; he will have but a slighting scorn of those whose applause he would value in old age; and, worse than all, he will deserve it. The sooner a sceptic leaves our profession the better. He that complains of the want of something to believe in, I am sure has not sought for it." These are remarkable words, but, according to my conviction, true and most worthy to be remembered by every medical man, whether a veterinarian or a doctor for men.

The afore-named article in the *Times* of 4th October last,—“The Doctors and the Public Health,”—contains some sad, but, I am sorry to say, true hints of our disbelief in physic, and of our indetermination to meet dangerous pandemic and epizootic diseases; but what can give a more deadly blow to the medical profession than these words of this remarkable and most valued newspaper in the world?—“It does so happen at the present moment that these two purely physical ailments (cholera and the rinderpest) are the only clouds of any importance which darken the national horizon. We turn to the medical profession for aid, and we are sorry to say that, as seems invariably the case under such circumstances, the answers we receive are so various and contradictory as to afford us little or no guidance.”

I myself have never seen the “rinderpest,” but read a good deal of it; it is, no doubt, a zymotic disease of a very serious kind, but I think it cannot be intractable: it must, like all other acute ailments, be curable and preventible, only that we are not unhappy disbelievers in chemical remedies, but have a founded conviction of disinfecting and specific physic, being at the same time not afraid to interrogate the *nature* of the true effects of such remedies. A mere symptomatic treatment will hardly do, or have any great effect in such cases; the specifics must be detected, not only the preventive, but also the curative ones, and this is a worthy task for the compatriots of Jenner.

The discovery of the eminent physiologist, *Professor Virchow*, of Berlin, that the ciliated epithelium, when once out of motion, is again only put into this action by potash and soda, is one of the most interesting discoveries in medical science, and cannot fail to strengthen the nearly quite gone belief in specifics. Dr Rutherford Haldane's most interesting lecture in the Royal College last winter, which has been printed in the *Edinburgh Medical Journal* of February last, is very worthy a man of great learning and strict logical reasoning, and it cannot fail to open the eyes of many disbelievers in specifics, and lead the young students to a better and clearer insight in physic than has hitherto been common amongst medical students; on the other side, we look upon this way of reasoning as the only one able to lead us from the disheartening and humiliating manner of killing sick domestic animals. We know very well from medical history that bad and dangerous pandemics and epizootics have very often visited Europe in former days, and

will still continue to do so, notwithstanding all our sanitary measures, which only serve to mitigate them, and may render them less dangerous, but can never totally prevent unforeseen atmospheric and telluric agencies, which, in all time, have been the first promoters of all pandemic and epizootic diseases.

It is to be hoped that the disheartening, and, for all medical science, so degrading a manner of curing epizootics by slaughter will soon be followed by a better plan, and more worthy the medical profession; for the slaughtering cure can only be of any real use where we are able to draw a reliable line of demarcation between sound and sick animals, and where physical agencies, which have occasioned the epizootic, are no more at work; but these conditions seem now not to be present in England, and the English veterinarians will therefore be compelled to make use of the curing principle as the only saving measure for their country. We wish them most heartily a good success, and hope that they will trust to the effective disinfecting compounds, and some of the afore-named mineral and vegetable remedies, and use them energetically and with the utmost strictness and consequence, taking, at the same time, all due and applicable quarantine measures where those might be applicable. This being done with energy, we have little doubt of their good success in saving their great, high-minded, and most noble nation from a tremendous loss of the most useful domestic animals. "Deus adjuvat fortes."

REYKJAVIK, 4th November 1865.

ARTICLE V.—*Cases of Ovariectomy.* By THOMAS KEITH,
F.R.C.S.E.

(Continued from page 535.)

CASE XXVII.—*Multilocular Ovarian Tumour, with Fibroid
Tumour of Uterus. Ovariectomy. Recovery.*

M. G., from Gourrock, 54 years of age, was sent to me in January 1865, by Dr Drummond, of Glasgow. She was a pale, thin, nervous woman, but her health had been good till August last, when she found that her dresses would not fit her as formerly. She increased rapidly in size, and when I saw her, the girth at the umbilical level was 41 inches. The tumour occupied the whole abdomen, extending under the sternum. Fluctuation was much more distinct at some places than others. On internal examination, the finger came at once against a solid tumour, filling up the recto-vaginal fossa. The cervix was central and very movable; the sound passed $2\frac{1}{2}$ inches, and, on turning it about, no impression whatever was made upon the posterior tumour, which had the feel-

ing of a hard fibroid. It could not in the least be pushed upwards. After repeated examinations, I was not able to satisfy myself whether the mass was adherent or not, till one time I felt on it, far back, a small projection the size of a pea. On pressing my nail against this, and drawing it towards me, the whole, forming a tumour about the size of a very large orange, suddenly gave about a quarter rotation. I was then satisfied that it had no connection with the ovarian tumour above, and the impression left was, that it was a pediculated fibroid of the uterus. It was explained to the patient that two tumours existed, that it was unsafe and unnecessary to interfere with the smaller one, but that the ovarian tumour, which was killing her, could be taken away. To be quite sure of the diagnosis, it was thought better to have a preliminary tapping: 26½ lbs. of dark bloody fluid were removed. This fluid was peculiar, and more resembled that which is found in fibro-cystic growths of the uterus. The tumour was now found to consist of a large, very thick-walled cyst, with some semi-solid part at its base.

Much relief followed the tapping, and on examination a few days afterwards, the position of the parts in the pelvis was found to remain the same. The solid tumour could be rotated as before, without in the slightest affecting the cervix uteri. On placing her on the elbows and knees, the mass was pushed out of the pelvis, and it did not come down again. Still the position of the cervix was unchanged. On now introducing the sound, and moving it as far as possible from side to side, the solid tumour, which could easily be felt by pressing downwards above the pubes, was slightly but distinctly moved with it. Some of my friends who examined the case for the first time at this stage could arrive at no satisfactory diagnosis, for it was now difficult to isolate the solid tumour from the lower semi-solid portion of the ovarian cyst.

It was left to the patient to decide whether she should return home till the cyst should refill, or whether the operation should be gone on with. As she wished the latter, ovariectomy was performed on the 30th of January, with the distinct understanding that the smaller tumour was not to be interfered with. The diagnosis written down before operation was, "Multilocular tumour of right ovary,—pediculated fibroid of uterus." The incision extended from an inch below the umbilicus downwards six inches. Slight parietal adhesion was separated, and a very thick multilocular cyst, arising from the right side of the uterus, easily removed. The pedicle was long, and twisted upon itself, which might account for the clots found inside the large cyst. The solid tumour was then brought into view. It was the size of a large orange, and arose from the fundus of the uterus. It had no very distinct pedicle, but appeared to be rather a continuation of the fundus. The body of the uterus was very much elongated. Indeed, I believe the sound had not entered into the cavity at all, and it was difficult to imagine how the tumour, as it lay in the recto-vaginal fossa, was unaffected by

the sound when passed into the cervix. It was thought better not to examine it too curiously, for it got at once of a dark-cherry colour from handling, and I was glad to let it sink back into the pelvis. The cyst walls weighed 3 lbs. 13 oz., and the largest cyst contained 26½ lbs. of fluid.

She suffered from sickness and distention for some days, but otherwise no bad consequences followed, and she returned to Gourrock a month after the operation. I saw her a few weeks since. She had got very fat, and was well in every respect, except that she complained of pain in her back, which always disappeared when she lay down. The fibroid had not descended into its old position, but was felt in the pelvis, and seemed smaller and harder than before. The cervix uteri was still quite unaffected by any downward pressure upon it, or by any other movement; and had I not had the tumour in my hand nine months before, nothing would now convince me that it is a uterine growth.

CASE XXVIII.—*Nearly Solid Ovarian Tumour, surrounded by Ascitic Fluid. Five Tappings. Ovariectomy. Death from Exhaustion.*

In August 1864, Dr James Young asked me to see Mrs F., aged 34, in whom he had detected a small ovarian tumour, after her first confinement in August 1858. After her second confinement, in June 1861, the tumour rapidly increased, and she was tapped in the following November. At this time she was seen by Dr Simpson, and the case pronounced an unfavourable one, on account of adhesions. She was tapped a second time in April 1864, and when I saw her, four months after, the fluid had again accumulated to a considerable extent.

The patient was a little, dark, wiry-looking woman, of healthy parents. She had enjoyed excellent health, and, until recently, the tumour had given little inconvenience. The abdomen measured 43 inches at the umbilicus, and was occupied by a solid tumour about twice the size of the adult head, surrounded by ascitic fluid. The tumour was well defined, except near the false ribs on the left side, where the omentum seemed adherent to its upper part. I called Dr Young's attention to the presence of a thick band connecting the tumour to the abdominal wall near the angle of the ribs. The uterus was normal and movable. At this time her general condition was favourable to ovariectomy, but there were no urgent symptoms, and the patient had not made up her mind on the subject. I heard no more of her till the end of November, when I was asked to perform ovariectomy, and a day was fixed for me on which to do it. I found she had been tapped a third time about a month before, and was already larger than she had yet been. Her pulse was quick, her emaciation extreme, and her health thoroughly broken up. The general condition was so unfavourable for ovario-

tomy that I at once declined interference. Tapping was however requisite to relieve her urgent symptoms, and nearly five gallons of fluid were removed, leaving the solid tumour as high as the angles of the ribs on either side. I saw her next day, and found her with a pulse at 130, and apparently sinking. Indeed, I arranged with Dr Young that he should try to have an examination. Some time afterwards I heard that she had rallied, and I saw her, and again tapped her on the 20th of January, her girth being then 49½ inches. From this tapping she rallied well, but the pulse continued high, and the fluid accumulated with alarming rapidity. Still it was agreed to give her the chance of ovariectomy, as she was now anxious for it, though it was faithfully represented to her, and those interested in her, that of three such cases two would probably die after operation. Some delay was occasioned in procuring a suitable lodging for her, and ovariectomy was performed on the 11th of February; and though she had been tapped but three weeks before, the girth at the umbilical level was already 53 inches. On the morning of the operation, though she had passed a good night, the pulse was 118, the breathing rapid, and her feebleness great. I told her that the chances were now as ten to one against her recovery after ovariectomy, and that I could not recommend it. She said she wished to have even that small chance for her child's sake.

But on putting her under chloroform, her appearance became so alarming, and the pulse so feeble, that I hesitated to proceed. After waiting a little, it was agreed to allow some of the ascitic fluid to escape slowly, then to enlarge the opening to admit the hand, and do no more should it be found that any great extent of adhesion existed, the separation of which would prolong the operation, and in her feeble state render it deadly. This was done. Some friable adhesion of the omentum to the upper part of the tumour, and the broad parietal band formerly alluded to, were easily separated, the wound was enlarged, and the tumour removed entire, for it was too solid to break up. The intestines were very red, and I intended to put in a drainage tube through the vagina, to allow the secretion which might go on after the operation to escape. She got so feeble on sponging out the pelvis that I was glad to close the wound, and get her to bed. There was no loss of blood, and the whole proceeding did not occupy twenty minutes. She had severe chloroform vomiting, and some of the vomited matter seemed to get into the larynx, and a troublesome, incessant cough came on, which continued for nearly two hours, completely exhausting her. Though she took stimulants freely, she never fairly rallied, scarcely spoke after the operation, and died in thirty-two hours. The tumour was nearly solid, and weighed 11 pounds. There were 60 pounds of ascitic fluid, which formed a complete jelly on cooling.

The following remarks, appended by Mr Spencer Wells to his

twenty-fourth case of ovariectomy, are so applicable to the above case that I quote them entire:—"Is it right to perform such an operation as ovariectomy in unfavourable cases? It may be said that by doing so the surgeon not only risks his own reputation, but lowers the operation he performs in the estimation of the profession, and thus lessens the number of favourable cases who might be willing to undergo it, were it not known that one in two, three, or four who do submit to it die. It is quite clear that a surgeon, who will only operate on very favourable cases, ought to show far better returns than one who consents to stake his own reputation, in order to give a dying patient a small chance of recovery; and it may possibly be right to follow the more prudent course. But in a case where the poor woman says, as many have said to me, 'I suffer from a disease which must kill me. I cannot live very long. My life must be a life of suffering. If you operate, I know the risk I run, but *I may* be cured, and return to my husband and children, and I would rather die than live as I am. In such a case as this, I do not envy the feelings of a man who, unless he saw the case was absolutely hopeless, would let any consideration for the general character of surgery, or for his own reputation as a successful operator, induce him to refuse the prayer of the poor dying creature who placed her life in his hands."

CASE XXIX.—*Nearly Unilocular Ovarian Tumour. Pelvic Adhesions. Death four days after Ovariectomy.*

Mrs D., thirty-two years of age, was sent to me, in the spring of 1864, by Dr Kilgour, of Aberdeen, to consider the question of ovariectomy. The patient was a remarkably delicate, phthisical-looking woman, and had always had indifferent health. The tumour was small, not extending much above the umbilicus. It was unilocular, and there was no evidence of parietal attachments. The uterus lay very far back, and low down in the pelvis. The cervix was movable, but the body of the uterus was quite lost in the cyst; indeed it seemed to form part of it. She had got cold on her way by sea, and when I saw her she had a slight pleuritic attack, with some effusion. There was also some subcrepitant rale here and there through the right lung, especially below the clavicle. She had a good deal of cough, without much expectoration. The right side of the chest was contracted, and there was a history of old pleuritic attacks.

Under these circumstances, I recommended no interference, and she returned home. Her health improved during the summer, and no increase of size took place. In the following spring, however, she had a severe pelvic peritonitic attack. Restless nights and rapid loss of flesh followed, and in the end of March I was disappointed to find her again back to me, very anxious, as she was before, to have the tumour removed. The tumour now reached to the ensiform cartilage, and the uterus was even more fixed than it was

before. The general condition was also much less favourable than it had been a year ago. She was paler and more emaciated; still had some cough; her voice was husky, and her nights were restless.

She was tapped on the 28th of March, with immense relief. The cyst seemed quite loose, and the uterus now felt movable; and having just read in the *Medical Times* a case of ovariectomy by Mr Wells, in which, before operation, he described the uterus as feeling as if lost in the cyst, whereas it turned out that the pedicle was of good length, I was in hopes that the same might be found to be the case in my patient. Still her general condition was so unfavourable that it was thought better to wait the results of the tapping. The cyst, however, filled with amazing rapidity, and I most unwillingly operated on the 8th of March,—some symptoms of pleuritic irritation, which appeared the first day she was out of bed after the tapping, having subsided. The cyst was already half-filled; it was tapped and easily drawn out, but it was found to be so closely adherent to the bladder and anterior surface of the uterus that it was deemed unwise, in such a delicate subject, to attempt their entire separation, for this could not have been effected without a careful and tedious dissection. The bladder was, however, freed for several inches, and the clamp was placed round the neck of the cyst, which fortunately was thin walled, and it was secured outside. She rallied well; there was copious secretion of urine, and profuse perspiration. Next day, though the pulse had risen to 120, her expression was so good that her cheerful and smiling face made me almost hopeful. But by evening there was a copious secretion of red, somewhat foetid serum. This continued to flow in large quantity, the pulse became more and more feeble, and she died on the fourth day after operation.

CASE XXX.—*Multilocular Ovarian Tumour. Ovariectomy.
Recovery.*

A. R., aged twenty-four, was sent to me on the 4th of May 1865, by Dr Fergus, of Glasgow, as a very favourable case for ovariectomy. Increase of size had been gradual since three years ago, but neither her comfort nor usefulness had been much interfered with, till within a few weeks before. Her general health was good, but she was pale and thin about the chest and arms. The tumour was multilocular, extending under the ribs on either side, and to within two inches of the ensiform cartilage. The girth at the umbilical level was 38½ inches, and from ensiform cartilage to pubes 18 inches. The vagina was funnel-shaped, and the uterus was drawn up almost out of the pelvis to the left side, but it was quite movable. There was no evidence either of parietal or pelvic attachment.

As the period had just passed off, and as the secretions were healthy, there was no occasion for delay, or for any preparatory treatment. Ovariectomy was performed, in the Edinburgh Surgical Home,¹ on the

¹ A small, nearly self-supporting, institution, where patients suffering from ovarian disease may have perfect quiet, perfect cleanliness, and perfect nursing,

6th of May. Dr Fergus was present from Glasgow. The incision commenced two inches below the umbilicus, extending downwards four inches. The upper central cyst was tapped, and a number of others emptied through it without removing the trocar. The tumour was then withdrawn, after freeing it from the omentum, which was partly adherent to the upper portion. The pedicle was secured about an inch and a-half from the right side of the uterus; a very broad expansion went off to the meso-cocum, upon which the clamp, when placed at the lower angle of the wound, made a considerable drag, and it was necessary to fix it in the centre of the incision. The edges were brought together by deep and superficial silk sutures, after sponging the pelvis from some turbid serum. The weight of the whole was $19\frac{1}{2}$ lbs. There was a large deposit of recent lymph on the anterior surface of the cyst-wall.

She had no sickness, no pain, and required no opium. The sutures were removed forty hours after operation, and the wound healed without discharging a drop of matter. She was up most of the fourteenth day, and returned to Glasgow on the 29th of May, having been absent from her situation only twenty-four days.

CASE XXXI.—*Single Unattached Cyst. Ovariectomy. Recovery.*

An unmarried lady, from Dumfries, forty-eight years of age, of a very strongly marked nervo-bilious temperament, consulted me, in May 1864, as to the propriety of having ovariectomy performed. She had been tapped in March 1863, and the tumour had become nearly as large as before. First signs of ill-health came on after a severe chill eight years ago. She had then repeated attacks of pelvic and abdominal pain, with occasional vesical irritation, but no swelling was detected till four years after this. For some time she was treated for cardiac dropsy. Her girth at the umbilical level was 37 inches, and the tumour extended only 3 inches above it. It consisted of a single unattached cyst. As her general health was good, and as she was able to walk about and enjoy life, I strongly advised her to let things alone in the absence of any symptoms interfering either with life or her comfort. In the following October I again declined interference.

In May 1865, her girth had increased to 40 inches. She had become thin and out of condition; had restless nights, and was getting nervous about the operation some time before her. Ovariectomy was now fully justifiable, and as the house she had taken was noisy, being close to a rifle-practising ground, she came into the Home on the 6th of June, and the cyst was removed on the 8th. Dr Imlach, of Liverpool, and other friends, were present. The operation was most simple. The incision was barely 3 inches in length, and the pedicle, which was very long and slender, was secured by a small clamp about 4 inches from the right side of the uterus. The cyst, conditions essential to even an average success after ovariectomy, and which cannot be obtained in a large hospital, and rarely in private lodgings.

which weighed only 10 ounces, contained 26 lbs. of clear fluid. The wound was closed, as usual, by silk sutures.

She got very sick with the chloroform, and had severe attacks of bilious vomiting for the first twenty-four hours. She had little pain, and required no opium. She had a foul tongue, and a good deal of headache for the first ten days; otherwise she recovered well, and went home twenty-five days after the operation,—the cicatrix measuring only an inch and a-half in length.

CASE XXXII.—*Multilocular Ovarian Tumour. Ovariectomy. Recovery.*

M. M., aged thirty-two, was sent to me for ovariectomy, in July last, by Dr Campbell, of Dunse. The tumour was multilocular, of rapid growth, and reached to midway between the umbilicus and ensiform cartilage. It stretched more to the left side, but both loins were clear on percussion. No cyst of any size could be detected, and there was no evidence of adhesion. The girth was 35 inches. The uterus lay almost in the opening of the vagina, being pushed down by a part of the tumour which projected into the pelvis; it was slightly inclined to the left side, but was quite movable. Recently the patient's health had begun to be impaired, and she was able to do but little. Had her circumstances admitted of her leading a quiet life, the disease might have been temporized with. But this was not the case; and Dr Campbell and I felt justified in recommending ovariectomy as long as her health was fairly good.

The tumour was removed on the 24th of July. Dr Campbell was present. The incision commenced 3 inches below the umbilicus, and extended downwards 4 inches. There was no adhesion. After tapping some small cysts, the tumour was opened and broken up. The pedicle was short and thick, and was secured by a large clamp within an inch and a-half of the left side of the uterus. There was a decided strain upon the uterus and upon the tissues of the left side of the pelvis, and the upper part of the rectum was slightly drawn towards the clamp. By placing the clamp in the middle of the wound, this traction on the bowel was almost removed. Some bloody serum was sponged from the pelvis, and deep superficial silk sutures closed the wound as usual. The whole weighed only 12½ lbs., and it is the smallest ovarian tumour I have yet removed.

She had chloroform sickness in the afternoon, but little or no pain, and no opium was necessary. By evening flatulence was troublesome, and for the first three days there was great distention of the abdomen. This was most distressing during the second night after the operation, when the gas was from time to time forced up with great violence. Much relief was given by passing up a long rectum-tube, and allowing it to remain for some hours. After this she did fairly well till the seventh day, when uterine epistaxis came on, and there was foetid discharge round the clamp. As this was much depressed and some pain felt, it was removed next day, and the

slough cut away. The stump of the pedicle sunk at once far inwards, and there was pain in the left iliac region. On the ninth and tenth days, she was flushed and looked yellow, had a dry tongue and quick pulse. The urine also became ammoniacal and concentrated. This condition, which was suggestive of septicæmia, passed off with profuse perspiration. After this she improved rapidly, and left the Home for Dunse twenty-three days after operation.

CASE XXXIII.—*Double Ovarian Tumour. Pelvic Adhesions. Ovariectomy. Recovery.*

In July last, Dr Gordon, of Old Aberdeen, took me to see Mrs K., æt. 42, who had been under his observation for about four years with ovarian disease. She had been a healthy woman, and had had only one daughter, now twenty years of age. She was fair complexioned, and her appearance, though somewhat anæmic, was still healthy. She was emaciated about the arms and chest, and had a remarkably dry skin. The abdomen was covered with a slight eczematous eruption. The girth at the umbilical level was $40\frac{1}{2}$ inches, and from ensiform cartilage to pubis $19\frac{1}{2}$ inches. Percussion was clear in right, dull in left lumbar region. The dulness of the tumour extended $2\frac{1}{2}$ inches above the ensiform cartilage, and the liver was much displaced upwards. The uterus was not very movable, the cervix was elongated and large, and the whole organ felt heavy. The roof of the vagina felt soft, but the examination was somewhat hurried, and the diagnosis I gave was, "single unattached cyst of left ovary."

After this she came to town for operation, and on again examining the condition of the tumour in the pelvis, it was observed that there was some depression of the right side of the vagina. When the patient lay on her back the roof of the vagina was soft, when she was turned on her face the roof of the vagina became tense, and rose from the finger. It seemed, therefore, very probable that some pelvic adhesion existed. Ovariectomy was performed on the 16th of August, with the assistance of Dr Carruthers, of Cramond, and Dr James Carmichael. A large thin-walled cyst, containing 35 lbs. of fluid, was easily removed through an incision 3 inches in length. The pedicle, which was long and thick, was secured by a clamp. On putting down my fingers to examine the state of the right ovary, I came upon a second tumour, about the size of a newly-born child's head, fixed in the pelvis. The body of the uterus was seen to be very large, and closely connected with this second cyst. The incision was extended, and after a little manipulation I found that the pedicle was adherent to the side and back of the uterus. This was separated, and the ovary itself, apparently quite healthy, was seen lying close to the uterus. The other attachments to the side of the pelvis were then torn, the cyst tapped and drawn upwards, and I was able to free it from all its attachments, except a broad firm band at its base, which resisted

tearing. This was cut, and then ligatured, the ends of the ligatures being cut short and left behind; and by embracing the neck of the cyst in the clamp, it was secured outside. Free bleeding was now going on, partly from the surface of the uterus, but chiefly from a rent in the ovary. A clot, the size of a bean, was turned out of a Graafian follicle, and the bleeding point secured by a ligature; but the tissues were so friable, and the oozing from the cavity so general, that the more we tied the more it bled. The bleeding was finally stopped by transfixing the ovary, and tying in everything within half an inch of the bleeding points. After waiting till all oozing from the pelvis had ceased, the two clamps were arranged outside, the fundus of the enlarged uterus was brought up against the pubes, and the wound closed by silk sutures. The operation lasted upwards of an hour.

Severe inflammation at once followed the injury done in the pelvis by the removal of the smaller tumour. Vomiting set in immediately after the operation; there was great restlessness, and a constant moaning, which was very distressing. At the end of twenty-four hours, serum had begun to escape from the lower angle of the wound. This was at once opened by removing first the lower stitches, and then the lower clamp. Uterine epistaxis commenced on the second day, and continued four days. The pulse rose, the abdomen became enormously distended, and she suffered very severely from colicky pains, requiring opiates every three or four hours for the first fortnight. By the end of the first week the discharge amounted to several pints in the twenty-four hours. It was of a thin, dirty, pea-soup character, with flakes of lymph, and was of the most putrid, horribly offensive odour, felt in every corner of the house. On the tenth day, the pulse was below 100, and the state of matters was this,—the wound was lying quite open, forming an irregular cavity 3 inches in length: in front, was seen the fundus of the uterus covered by granulations; behind, this cavity was bounded by two folds of small intestine, coated with a thick layer of lymph, and shutting off the pelvis completely from the general cavity of the abdomen. The sides were bounded by the remains of the two pedicles,—the right had sunk about two inches, the left was still adherent to the wall. All this could be seen and fingered. On the fourteenth day, a large collection of matter, which had been gradually forming in the right iliac fossa, began to discharge itself into the wound. About this time diarrhoea came on, and continued at intervals for two or three days. This was looked upon as curative, and rather encouraged than otherwise, as she was the better of it, and it put an end to a tendency to drowsiness, which had been threatening for some days. All this time Douglas's space had been carefully watched from day to day. That it contained a large quantity of purulent fluid there could be no doubt; but there was no tension when examined either by the rectum or vagina, and I was afraid to put a trocar into it. After this she

gradually improved. By the end of three weeks the abdominal distension had nearly subsided, and though feeble, and the wound still discharging daily about a tablespoonful of matter, she was able to leave by the morning train for Aberdeen thirty-five days after operation. The journey was well borne, and when Dr Gordon called next morning she was up and dressed. I heard of her a few days ago. She was getting stout, and was in better health than she had been for many years.

CASE XXXIV.—*Thick-walled Single Cyst. Ovariectomy. Recovery.*

In July 1865, I saw, at Glasgow, with Dr Wilson and Dr Fleming, a lady, thirty-two years of age. She had been married nearly a year, and had enjoyed excellent health till the preceding May, when, on a visit to Edinburgh, she had an attack of menorrhagia. For this she was seen by Dr Matthews Duncan, who detected an ovarian tumour, of the presence of which she had, till then, been quite unaware. After this she increased rapidly, and when I saw her the girth at the umbilical level was 36 inches. The tumour was unattached, and seemed to consist chiefly of one cyst, though the fluctuation was more distinct at some points than others. The uterus was movable, but it was drawn slightly upwards by the tumour. Hitherto she had suffered no inconvenience, and her general health was as yet quite unaffected. The case was therefore clearly one for non-interference.

After this she went to the country, and though her health continued good, her girth increased at the rate of an inch a month. Towards the end of September I was asked to perform ovariectomy. This was done, at her own home in Glasgow, on the 29th September. Dr Wilson gave chloroform, and Dr Fleming kindly assisted me. A thick-walled almost single cyst containing 15 lbs. of fluid was removed through a small incision. The pedicle was of fair length and moderate thickness, and was secured by a clamp about $2\frac{1}{2}$ inches from the left side of the uterus. The proceeding was remarkable only for its simplicity, and the easy recovery that followed is an argument for early operation in ovarian disease.

CASE XXXV.—*Multilocular Ovarian Tumour. Ovariectomy. Recovery.*

M. S., æt. 56, was sent to me by Dr Husband in the beginning of November last. She had laboured under ovarian disease for four years, and had been frequently recommended by Dr Wilson to have the tumour removed. Though not a robust woman, she had enjoyed an average amount of health. Latterly she had suffered much from sickness, and had become exceedingly emaciated. Her nights were bad, and she could go on no longer. The pulse varied between 90 and 100. Her girth at the umbilicus was 43 inches, and from ensiform cartilage to pubes she measured $22\frac{1}{2}$ inches.

The uterus was small and freely movable; but it lay low in the pelvis, which was occupied by part of the tumour.

Ovariectomy was performed on the 10th of November. The tumour was nearly unilocular, and was removed through an incision 3 inches in length. A long slender pedicle, arising from the right side of the uterus, was secured by a small clamp, and the wound closed by two deep silk sutures, the clamp being placed between them. The large cyst contained 33 lbs. of fluid.

She was feeble for some days after operation. She slept profoundly the first night, better than she had done for a very long time. She had no pain, no sickness, and required no opium. She took her food well from the first day, and though one could hardly imagine a more emaciated individual, she regained strength rapidly, was on the sofa the greater part of the eleventh day, was walking about quite well on the fourteenth, and left the Home a few days after.

In addition to these cases, in the following table are included two which have been operated on within the last six weeks. Both are now quite convalescent.

Table of Thirty-seven Cases of Ovariectomy.

No.	Date.	Age.	Condition.	History, etc.	Result.
1	1862. Sept.	49	Married,	Multilocular; 25 lb.; surrounded by ascitic fluid;	Remains well.
2	1863. Jan.	55	Married,	Multilocular; 45 lb.;	Remains well.
3	Feb.	24	Married,	Multilocular; 63 lb.; tapped once;	Died 23 hours after.
4	March.	27	Married,	Multilocular; upw. of 120 lb.; tapped 4 times;	Remains well.
5	May.	22	Unmarried,	Multilocular; 33 lb.; since married;	Remains well.
6	July.	52	Married,	Fibro-sarcomatous, and cystic; both removed;	Died 5th day.
7	Aug.	23	Married,	Multilocular; nearly 80 lb.; tapped 7 times;	Died 38 hours after.
8	Sept.	23	Unmarried,	Multilocular; 40 lb.; tapped twice;	Remains well.
9	Nov.	16	Unmarried,	Semi-solid; very large; tapped once;	Remains well.
10	1864. Jan.	55	Married,	Semi-solid; 23 lb.; tapped twice;	Remains well.
11	Feb.	40	Unmarried,	Multilocular; 37 lb.; tapped once;	Died 6th day.
12	March.	50	Married,	Large single cyst;	Remains well.
13	April.	68	Married,	Cystic and adenoid;	Remains well.
14	May.	23	Unmarried,	Semi-solid; 24 lb.;	Died 9th day.
15	May.	35	Unmarried,	Large single cyst;	Remains well.
16	May.	29	Married,	Multilocular; 65 lb.; tapped 4 times;	Died 46 hours after.
17	May.	47	Unmarried,	Semi-solid; 35 lb.; tapped twice;	Remains well.
18	May.	27	Unmarried,	Multilocular; 35 lb.;	Remains well.
19	July.	50	Unmarried,	Semi-solid; 36 lb.; tapped once;	Remains well.
20	July.	33	Unmarried,	Very large single cyst, containing 55 lb. of fluid;	Remains well.
21	Sept.	34	Married,	Multilocular; 23 lb.; tapped twice;	Remains well.
22	Oct.	44	Unmarried,	Single cyst; tapped twice;	Remains well.
23	Nov.	50	Married,	Multilocular; 56 lb.; tapped 3 times;	Remains well.
24	Dec.	51	Married,	Multilocular; 57½ lb.; tapped twice;	Remains well.
25	Dec.	44	Unmarried,	Multilocular; tapped once;	Died 3d day after.
26	1865. Jan.	36	Unmarried,	Multilocular; 30 lb.;	Remains well.
27	Jan.	54	Unmarried,	Multilocular; tapped once;	Remains well.
28	Feb.	34	Married,	Solid; 11 lb.; 60 lb. ascitic fluid; tapped 4 times	Died 32 hours after.
29	March.	32	Married,	Nearly single cyst; tapped once;	Died 4th day.
30	May.	24	Unmarried,	Multilocular; 194 lb.;	Remains well.
31	June.	48	Unmarried,	Single cyst; 26 lb.;	Remains well.
32	July.	32	Unmarried,	Multilocular; 124 lb.;	Remains well.
33	Aug.	42	Married,	Both removed; 36 lb.;	Remains well.
34	Sept.	32	Married,	Thick single cyst; 164 lb.;	Remains well.
35	Nov.	36	Unmarried,	Multilocular; 35 lb.;	Remains well.
36	Nov.	23	Unmarried,	Multilocular; 33 lbs.; tapped once;	Remains well.
37	Dec.	21	Unmarried,	Semi-solid; 20 lbs.;	Remains well.

ARTICLE VI.—*On the Action of Medicinal Preparations of Iron on the Teeth.* By JOHN SMITH, M.D., F.R.C.S., Surgeon-Dentist to the Royal Infirmary, etc.

(Read before the Medico-Chirurgical Society, 6th December 1865.)

THE preparations of iron in medicinal use are very generally supposed to exert, in a direct manner, an injurious influence on the teeth of patients for whom they are prescribed. Complaints regarding their destructive tendency in this respect are familiar both to the general practitioner and to the dentist. With the view of ascertaining how far such complaints are well grounded, the experiments I have here briefly to notice were made.

Eight of the compounds of iron in most general use as remedial agents, and along with them one or two other non-ferruginous compounds, sometimes suspected of injuring the teeth, were selected, and in solutions of these compounds the same number and the same kinds of human teeth were immersed. In each of the separate solutions four teeth were placed; and in all cases these four teeth consisted of a sound canine and bicuspid, and a decayed upper and lower molar. The solutions consisted of:—

- I. Sach. Carb. Ferri, gr. xx.
Aqua, ℥j.
- II. Carb. Ferri, gr. xx.
Aqua, ℥j.
- III. Syrup. Phos. Ferri, ℥ss.
Phos. Ferri, gr. x.
Aqua, ℥ss.
- IV. Syrup Iod. Ferri, ℥ss.
Aqua, ℥ss.
- V. Citrat. Quinæ et Ferri, ℥jj.
Aqua, ℥j.
- VI. Vinum Ferri, ℥ss.
Aqua, ℥ss.
- VII. Sulphat. Ferri, ℥ss.
Aqua, ℥j.
- VIII. Tinct. Mur. Ferri, ℥jj.
Aqua, ℥j.

At the same time the following non-ferruginous solutions were tested:—

- I. Sulphate of Quinine, gr. v.
Sulphuric Acid, gtt. i.
Water, ℥ss.
- II. Phosphoric Acid Dilute, ℥ss.
Water, ℥ss.
- III. Condyl's Fluid (crimson), ℥ss.
Water, ℥ss.

On examining the respective solutions after twenty-four hours, the teeth were found unaltered in those of the carbonate and saccharine carbonate of iron, the phosphate of iron, the iodide of iron, the citrate of quinine and iron, and in that of the sulphate of quinine. In the solution of the *vinum ferri*, the liquid itself was somewhat turbid, the teeth, however, seeming to be untouched. In that of the muriate of iron, a turbid sediment filled the bottom of the bottle, and covered up the teeth from view; the fangs of the teeth were somewhat soft and flexible, and the enamel easily scraped down. The sediment under the microscope presented an amorphous granular appearance. In that of the phosphoric acid they seemed somewhat flexible at their more slender parts, such as the points of the fangs; and the enamel looked opaque and chalky, but did not feel crumbling or soft. In the solution of Condyl's fluid they were deeply stained, but in no way altered in texture.

The teeth in the different solutions were allowed to remain ten days longer, and on examining them at the end of that period neither those in carbonate or saccharine carbonate of iron, the phosphate of iron, the iodide of iron, nor the citrate of iron and quinine, presented any further change, except that those in the saccharine carbonate were slightly blackened—the discoloration, however, being superficial, and nearly all removable by brushing. A ropy sediment adhered to those in the sulphate of quinine, and perhaps a very slight softening of the surface of the fang might be present. The ropy sediment presented, under the microscope, the appearance of a mass of acicular crystals of various sizes, interspersed with threads or fibres of some kinds.

In the solutions of the *vinum ferri*, the sulphate of iron, the muriate of iron, the phosphoric acid, and of Condyl's fluid, certain changes were observable. In the *vinum ferri* the cloudy precipitate was somewhat increased, and the teeth were dark and discoloured, especially in the fangs, but otherwise uninjured. In the sulphate of iron solution a very copious precipitate had formed. The teeth were not softened throughout their whole thickness, but had a layer of soft substance covering in what of the enamel and dentine remained hard beneath. They were also considerably discoloured. In the solution of the muriate of iron, a large deposit of the cloudy precipitate had accumulated, and in this the teeth lay buried. Their fangs were very soft, and quite flexible, and the enamel was wasted away, and on being touched crumbled down like chalk. In those, again, immersed in the phosphoric acid solution the fangs were quite flexible, and were diminished in bulk; the enamel could be deeply scratched with any blunt instrument, and felt like Derbyshire spar. It was more wasted, but not so soft as that of the teeth in the muriate of iron solution. Notwithstanding this loss of salts of lime, on drying by exposure to the air these teeth have again become hard and unyielding. In the Condyl's solution, the teeth were covered by a very dark incrustation, which, however, could be nearly all re-

moved by brushing, or still more effectually by applying dilute muriatic acid; otherwise they were uninjured. The solution itself had become almost colourless.

From these facts it would appear that certain preparations of iron, when directly applied, do exercise a powerful effect on the substance of the teeth. And the ratio of the effects obtained would seem to prove, that of all the preparations employed in these experiments that of the tincture of the muriate of iron acts most powerfully, the sulphate of iron next, and next to that again, although in comparison very immaterially, the *vinum ferri*,—the other preparations of iron appearing to be inert.

Of the other substances experimented with, phosphoric acid seems the only one producing injurious effects on the teeth, which it does, however, to a very marked extent.

ARTICLE VII.—*On a Case of Acute Atrophy of the Kidneys and Liver in a Pregnant Woman.* By T. GRAINGER STEWART, M.D., F.R.C.P., Pathologist and Extra-Physician to the Royal Infirmary, Lecturer on General Pathology, Surgeons' Hall.

(Read before the Medico-Chirurgical Society, 6th December 1865.)

IN a paper which was published in this journal for October 1865, I drew attention to the fact that, in cases of acute yellow atrophy of the liver, the same changes may exist in the kidneys that occur in the hepatic organ, and endeavoured to show that in both the process depends upon a blood-poison. I have now to describe a case apparently of the same nature, in which the kidneys were primarily affected, the liver only secondarily, and to a slighter extent.

For an account of the symptoms I am indebted to Drs Graham Weir, M'Cowan, Mitchel, and Oman, who saw the patient during the few days of her illness.

M. M., aged 22, was of dissipated and abandoned habits, but always enjoyed good health till the commencement of this illness. In October she was far advanced in pregnancy, and on the 20th of that month, without known cause, she became suddenly ill, with symptoms which were referred to inflammation of the uterus. But as no satisfactory treatment was possible where she resided, and as she became rapidly worse, she was sent to the Maternity Hospital. She was admitted on Friday evening, 22d October. She was then very weak, and was vomiting stringy mucus, with streaks and clots of blood, and now and then matter like coffee-grounds. This continued, notwithstanding the employment of suitable remedies, until Sunday morning, October 24, when she began to sink rapidly. At that time premature labour was brought on by direction of Dr Graham Weir. She was delivered of a putrid child about 4 P.M., and died

at 8 o'clock the same evening. She was never unconscious, but was quite intelligent until within a few minutes of her death. She had also no convulsions, nor any distinct delirium. She passed urine several times after her admission, but it was in small quantity, and its qualities were not ascertained.

Autopsy forty hours after Death.—The body was well nourished. There was no dropsy. Decomposition was rapidly advancing. The pericardium was natural. The heart contained a moderately firm clot, but the blood generally was dark and fluid. The muscular substance of the heart was flabby, but the valves were healthy. Both lungs were congested throughout, particularly towards the base posteriorly. There were traces of old tubercle at the left apex. The liver was of natural size, soft, and flabby, but not friable; the outlines of its lobules were distinct. The spleen was pulpy. The kidneys were small, and weighed together $6\frac{3}{4}$ ounces. They were flabby, congested, and in the cortical substance there were numerous extravasations. That part was not increased, but rather diminished in size, and contained some yellowish-white deposit, which was rendered the more distinct by the surrounding congestion. The stomach contained some dark green matter, like altered blood; its mucous membrane was catarrhal, and was perforated by numerous minute ulcers. Some parts were deeply congested, and at others there were patches of extravasation. The upper part of the small intestine contained bile and altered blood. The coats throughout were normal. The large intestine was natural. The ovaries were natural. The uterus presented the ordinary appearance of one recently delivered, and was moderately contracted. The bladder contained almost no urine; its coats were normal. The head was not examined.

Microscopic examination of the Kidneys.—The whole organ was found to be altered. The tubules, both in the cortical substance and in the cones, were opaque, and appeared as if distended with fine dark injection. On examination with a higher power, the distention of the tubules was found to depend partly upon enlargement of the epithelial cells and partly upon free exudation, which occupied the cavity of the tube. The cells were in some parts opaque, of a brownish colour, and full of dense granular matter, which obscured the nuclei; in others, they contained fatty granules of large size, surrounded by numerous firm fatty molecules; in others, they were represented by groups of oil globules, enclosed by an indistinct cell wall, which broke down under very slight pressure. So great was the friability of these fatty cells, that when a scraping of the kidney was examined, the pressure of a light covering glass sufficed to break down the tube-casts to a fine molecular debris. In transverse sections of the tubules the changes were well seen; in some the epithelium enlarged, dense brown, filling up the lumen; in others, the opaque matter replaced by fat, the outlines of the cells remaining entire; in others, the cells broken down, and represented by groups of fat granules; others were entirely empty.

The condition of the stroma was also interesting; it was somewhat enlarged, and contained numerous fatty granules. Many of these were arranged in little groups and lines, and appeared as if situated in the connective tissue-corpuscles. The corpuscles were certainly somewhat enlarged, from which it appeared that the lesion was not confined to the tubules.

From the fact that, although every tubule which was seen was distended, the cortical substance was diminished rather than increased, and the organs were below their average weight, it is evident that considerable wasting of the organ must have taken place, and this we can well understand when we think of the rapid fatty disintegration which was going on. The distinct diminution of bulk of the organs, and the rapidity with which it was taking place, seem to warrant the application of the name, "acute atrophy."

The very striking similarity between the conditions just described and those accompanying acute yellow atrophy of the liver cannot be overlooked, whether we regard the general appearance of the organ or its microscopic characters. The whole series of changes, from the exudative infiltration into the complete disintegration of the cells, was identical with what was found in the case recorded in this journal for last October.

Microscopic examination of the Liver.—Under a low power the hepatic cells were seen to be unusually opaque, while towards the margin of each lobule there was a dark zone of fatty degeneration, and outside of that—*i.e.*, at the point of contact of neighbouring lobules—there was a clear space in which no cell was visible, only a little granular debris. Under a higher power, the cells were found to present exactly the characters of those met with in acute yellow atrophy—that is, some were enlarged, opaque, full of dense granular matter; others were fatty; others were in an advanced state of fatty degeneration, extremely friable, easily broken down. These different stages were also distributed in the lobules, just as was described in my former case—the swollen infiltrated cells being in the centre, surrounded by those in a stage of degeneration, these again surrounded by the space in which no cell element remained. There was no breaking down of the stroma of the organ, nor any marked bile-staining of the cells, nor retention of bile in the ducts.

It may be suggested that this change was an ordinary fatty degeneration, occurring in its usual seat, towards the margin of the lobule, and not at all to be wondered at, considering the character and habits of the patient. But I think it was something more,—firstly, because the appearance of the fatty cells was unlike what we ordinarily see; secondly, because of the peculiar opacity of the cells in the centres of the lobules; thirdly, because fatty degeneration never leads to destruction of the cells, as was seen in the periphery of the lobules, while in all these points the appearances are characteristic of the acute yellow atrophy.

It appears to me that we are entitled to regard this case as an example of a disease essentially identical with acute yellow atrophy, for the following reasons:—

1st. The pathological appearances in the kidneys and liver were the same as those met with in acute yellow atrophy.

2d. The leading symptoms of this case; the peculiar malaise; the sickness and vomiting of blood; the rapid sinking; the congestion of the lungs,—are among the chief of those looked for in the other disease.

3d. The circumstances of the patient. She was advanced in pregnancy, was leading an exposed and wretched life,—two circumstances very commonly associated with the other affection.

From these considerations, my view seems to be well established; at the same time, it is to be noticed that three frequent symptoms were entirely wanting, viz., delirium passing into coma, icterus, and diminution of the volume of the liver.

The absence of coma and convulsions seems unaccountable from the condition of the kidneys; for one would have expected well-marked uræmic symptoms. The bladder was empty, and the kidneys were obviously incapable of secretion. But the symptoms of neither the acute nor chronic form of uræmia were present. This fact can, I think, only be explained on the hypothesis, which is consistent with the other facts of the case, that the patient died before the uræmic symptoms had time to be developed.

The icterus and the diminution of the volume of the liver often occur late in the case, sometimes not at all; and in this instance the absence of diminution was well explained by the fact that the disease was not far advanced in the liver, and this circumstance also accounted for the absence of icterus.

In regard, again, to the symptoms which were present, this case seems to teach an important lesson; for the hæmatemesis, which is so frequent a symptom in acute yellow atrophy, occurred here, although the liver was comparatively little diseased; thus showing that hæmatemesis is not dependent on the hepatic affection. May it not be the result of a blood-poison?

If it be admitted that this case is of the same nature as acute yellow atrophy, it certainly gives the *coup de grace* to the theory which would make the renal affection secondary to the hepatic.

This case remarkably confirms the opinion that acute yellow atrophy is a blood disease. The general appearance of the body; the marked hypostatic congestion and rapid decomposition; the softness of the spleen; the darkness of the blood and its imperfect coagulation; the occurrence of identical morbid conditions in the liver and kidneys; and the peculiar nature of the process in both organs, differing, as it does, from ordinary inflammation and from ordinary fatty degeneration, and the peculiar circumstances under

which it occurred, point strongly to some cause operating within the blood.

The case, then, supplies an additional point in the history of acute yellow atrophy, and decidedly supports the view of its being dependent on a blood-poison.

The only similar cases that I have found are three, which were published by Rokitansky in 1859, under the name of fatal steatosis of the liver and kidneys.¹ I shall not detail his cases, but merely mention certain general facts in regard to them. All were females, unmarried, aged 23, 38, and 8 years respectively. Two had been in depressed spirits. All were stout, well-nourished, and died after about a week's illness, during which the leading symptoms were—fever, headache, vomiting of dark matters, convulsions, and coma; in two there was slight icterus. On post-mortem examination, ecchymoses were found in many of the internal organs. The livers were large, pale, flabby, and fatty; the kidneys were also large, fatty, congested, and mottled with extravasation of blood; the urinary bladders contained almost no urine; in two, the stomach and intestines contained altered blood. Microscopic examination showed in all three cases extreme fatty degeneration of the liver and kidneys.

Rokitansky distinctly recognises a poisoned condition of the blood, which, however, he regards as a consequence, not a cause, of the hepatic and renal affections; and, in accordance with a favourite view, he regards the renal affection as secondary to the hepatic. He refers the fatty condition of the liver to that state of the system in which an excess of fat is present and a tendency to fatty deposition exists, and having excluded, with regard to the kidneys, the theory that it might depend upon Bright's disease, he concludes by saying, "There can be little doubt that the steatosis of the kidney was secondary to the primary steatosis of the liver; just as little can we doubt—from the diminution of urine consequent upon kidney disease on the one hand, and the slight degree of cholæmia on the other hand—that the sudden development of the fatal symptoms depended upon uræmia. It would accordingly appear that fatty degeneration of the liver may occur in an individual inclined to excessive fatty deposit, and to this, sooner or later, a fatty condition of the kidney may be superadded, both of which, gradually and unobserved, may attain such a degree that secretion of bile and urine is suspended, and death supervenes suddenly, after the appearance of slight icterus, by uræmia, and an hæmorrhagic decomposition of the blood."

Notwithstanding the profound respect to which every opinion of Rokitansky is entitled, I would venture to suggest that the cases are capable of another interpretation, especially when examined by the light of the remarkable case which I have just described. It is

¹ Ueber lethale Leber- und Nieren Steatose, von Prof. Rokitansky. Zeitschrift der k. k. Gesellschaft der Aerzte zu Wien, Aug. 1859.

very difficult to believe that so general and important an affection of the liver and kidneys should have gradually advanced, and yet for long led to no symptom; still more is it difficult to imagine how so insidious an affection should have suddenly produced such violent symptoms, how the uræmia should have been unaccompanied by any trace of dropsy, or should have led to a hæmorrhagic condition of the blood; and, on the other hand, all the symptoms correspond closely with those of acute yellow atrophy, and many of them with that peculiar variety of the disease which I have described. These cases, indeed, seem to constitute a connecting link between the affection in its ordinary form, in which the liver is mainly involved, and this peculiar variety in which the kidneys are the primary seat of disease.

ARTICLE VIII.—*On the Relative Mortality from Injuries and Burns on the Trunk of the Body, above and below the Level of the Diaphragm.* By JAMES CUTHILL, M.D., Parochial Surgeon, Denny and Dunipace.

THE well-known law of the prize-ring, that pugilists are not permitted to strike below the *belt*, is a regulation founded on an observation which must have forced itself upon the attention of every one who has watched the effects of severe injuries inflicted on the abdominal region, such as kicks from horses and mechanical forces of a like severity, viz., that such injuries almost invariably prove fatal. An analogous law holds good in the case of severe burns of the same parts.

During the past nine years, I have practised among the miners working an extensive mineral field, the strata of which are largely impregnated with fire-damp (carburetted hydrogen), and explosions from the ignition of this gas are of considerable frequency. These burns, however severe they are, seldom prove fatal.

The miner's working dress consists of flannel drawers or trousers, firmly secured round the waist by a leather strap, a loose-fitting woollen shirt, the upper part of the chest and arms being bare, and a small leathern skull-cap, into which the lamp is fixed, completes the equipment. Consequently, the parts scorched in these explosions are the face, neck, arms, and upper part of the chest. Notwithstanding that I have treated dozens—I had almost said hundreds—of such cases, I can only recall one instance in which the result was fatal, and in this one the death could be easily referred to other elements in the case.

The almost universal fatality in crinoline accidents, where the abdominal surface is the part involved, is also worthy of remark. In the three cases which have come under my notice, the result was death.

To what circumstances, then, are we to ascribe the difference in the fatality resulting from injuries and burns in these two regions? After careful consideration, I can offer no better explanation than that the cause is to be sought for in injury or shock inflicted on the large and numerous ganglia of the sympathetic system so plentifully distributed in the abdominal region, and the ramifications from which are so closely intertwined round the viscera and large blood-vessels, and, by direct communication, must influence that source from which the heart and lungs derive their principal nervous supply,—in this way accounting for the rapid sinking and the low form of the inflammatory action excited in these cases.

I can only regret that my position as a country surgeon debars me in great measure from consulting the literature connected with this subject.

ARTICLE IX.—*Some Statistics of Idiocy.* By ARTHUR MITCHELL, M.D., Deputy Commissioner in Lunacy.

I. *Sex.*

THE number of idiots and imbeciles examined was 1345. Among these the distribution of the sexes was as follows:—

TABLE I.

	Male.	Female.	Male.	Female.
Idiots, ...	430	284, or	100 to	66·0
Imbeciles, ...	321	310, or	100 to	96·5
Total Idiots and Imbeciles, 750	594, or	100 to	79·2	

From this it appears that amongst idiots and imbeciles there are more males than females, there being only 79 of the latter to every 100 of the former. The reverse of this holds good, under every circumstance, among those labouring under the acquired, as opposed to the congenital forms of insanity. The terms *acquired* and *congenital* are here used for the sake of convenience and brevity, as it is well understood what they are intended to cover.

It further appears that this excess of males over females is chiefly manifested among those labouring under the graver forms of the disease,—among *idiots* the excess rising as high as 100 males to 66 females.

In order to test the *steadiness* of these results, I shall divide the 1345 into two groups of 566 and 799,—the first having been chiefly seen in southern, and the last in northern, counties of Scotland.

Taking the first group, consisting of 566, we have:—

TABLE II.

	Male.	Female.	Male.	Female.
Idiots, ...	205	143, or	100 to	69·7
Imbeciles, ...	104	114, or	100 to	109·6
Total Idiots and Imbeciles, 309	257, or	100 to	83·1	

Here, again, we find a similar and well-marked excess of males over females, when we deal with the total number of idiots and imbeciles, and in this instance we find that the excess occurs entirely among the *idiots*, since here the female *imbeciles* actually exceed the male.

If we take the second group, consisting of 779, we have as follows:—

TABLE III.

	Male.	Female.	Male.	Female.
Idiots, ...	225	141, or	100 to	63·5
Imbeciles, ...	217	196, or	100 to	90·3

Total Idiots and Imbeciles, 442 337, or 100 to 76·2

There is a very striking and intimate similarity between these figures and those in the first table,—a similarity, indeed, which amounts to a substantial identity. When all three tables are considered together, they lead us very safely and surely to the following conclusions:—

(1.) That the excess of male over female idiots and imbeciles, taken together, is considerable and steady.

(2.) That the excess of males is much greater among idiots than among imbeciles; in other words, that the excess of males is most marked in the graver forms of the disease.

(3.) That even among imbeciles, when we deal with large numbers, it will probably be always found that males exceed females, though not to a great extent.

II. Age.

The following tables embrace the same cases, 1345¹ in number, which were embraced in the tables having reference to sex; and, in the analysis which is made to determine the *steadiness* of the results, the same grouping is resorted to.

TABLE IV.

Age.	Idiots.	Imbeciles.	Total Idiots and Imbeciles.	Per 1000.
0-5	5	0	5	3·7
5-10	44	7	51	37·9
10-15	87	18	105	78·1
15-20	100	50	150	111·5
20-30	194	133	327	243·1
30-40	122	146	268	199·2
40-50	81	109	190	141·3
50-60	57	94	151	112·3
60-70	21	48	69	51·3
70-80	3	22	25	18·6
80-90	0	4	4	2·9

¹ It must be borne in mind that this number probably includes 90 per cent. of *all* the idiotic and imbecile persons in the districts in which the observations were made, —the *very young*, however, being excepted.

It appears, therefore, that more idiots and imbeciles are found between the ages of 20 and 30 than in any other decennial period. The ten-year periods, on each side of this one, are those which come next. Of the 1345 idiots and imbeciles examined, the ages of 850, or about 63 per cent., lay between 10 and 40 years.

The number below the age of 10 is small—much smaller than it should be, for reasons which will be afterwards explained. But even if all those below 10 were known, it is almost certain that their number would not be so large as that of those between 10 and 20, or 20 and 30. The mortality of idiots in infancy and childhood is very large, and in this period there is little difference between the rate of receiving fresh cases and the rate of loss by death. With idiots, however, those who reach 15 or 16 acquire a tolerably certain tenure of life for a second stretch of 10 or 15 years. That period, therefore, receives additions at a rate above the rate which represents its losses by death, and an accumulation follows. After the age of 30 the reverse holds good. More are passed into the decennium—30 to 40—than leave it for the next. In other words, there are more deaths than admissions, and thus a thinning of the numbers begins, which goes on with accelerated force as greater ages are dealt with.

I think this table also shows that a larger number of idiots and imbeciles attain the full measure of life than we have been accustomed to believe to be the case. Out of 1345 we have no fewer than 249 above the age of 50, 98 above 60, 29 above 70, and 4 above 80.

Fewer idiots than imbeciles, however, reach an advanced age. Thus, while we have only 11·3 per cent. of the idiots, we have not less than 26·6 per cent. of the imbeciles above 50. It would appear, in short, that the lower the type of the mental defect the sooner comes virtual old age and death.

The actual proportion of weak-minded and idiotic *children* is to a certainty much larger than appears in this table. In very early life the deficiency of intellect in many cases is not acknowledged, or perhaps is not seen, by the parents and friends of the child, and so his defect becomes practically an unknown thing. It is thus, I think, we must account for the seemingly small number of *young* idiots and imbeciles. Of course, the greater the defect is, the more likely it is to be seen and admitted in childhood, and accordingly we find that we have 161 *idiots*, or 22·5 per cent. below the age of 15, while we have only 25 *imbeciles*, or 3·9 per cent. below that age. This fact comes forward as a proof of the accuracy of the manner in which the absence of the young from the table has been accounted for.

As illustrative of the substantial steadiness of the distribution of ages among the idiotic and imbecile, Table IV. has been divided into two groups, and the result is shown in the table which follows :—

TABLE V.

Ages.	First Group of 566. Idiots and Imbeciles.	Second Group of 779 Idiots and Imbeciles.
0-5	1	4
5-10	12	39
10-15	35	70
15-20	60	90
20-30	134	193
30-40	117	151
40-50	93	97
50-60	80	71
60-70	26	43
70-80	7	18
80-90	1	3

We are here dealing with those who labour under what is called congenital insanity. As the vast majority of the inmates of our public asylums labour under what may be regarded as acquired insanity, the distribution of ages among them may be taken for the purposes of comparing the distribution in the one class with that of the other. The ages of 3630 of the inmates of 28 of our public asylums were ascertained during the taking of the census of 1861, and these, reduced to the proportion in 1000, are given in the following table, and contrasted with the ages of the 1345 idiots and imbeciles similarly reduced.

TABLE VI.

Ages.	Inmates of Asylums. Per 1000.	Idiots and Imbeciles. Per 1000.
0-5	0.0	3.7
5-10	0.0	37.9
10-15	1.6	78.1
15-20	14.0	111.5
20-30	147.9	243.1
30-40	249.6	199.2
40-50	252.9	141.3
50-60	186.5	112.3
60-70	111.8	51.3
70-80	29.8	18.6
80-90	4.9	2.9

From this table it appears that we have three times as many idiots and imbeciles below the age of 30 as we have of those labouring under the acquired forms of insanity—the proportions being as 47.4 per cent. to 16.4 per cent.—nearly one-half of all the idiots and imbeciles known being below the age of 30. It follows, of course, that the difference between these two classes of the insane above the age of 30 will differ greatly in an opposite way,—83.5 per cent. of the inmates of asylums, and only 52.5 per cent. of idiots and imbeciles, being above that age.

III. *Comparative frequency of Births of Idiots in First and Subsequent Pregnancies.*

Among 443 idiots and imbeciles consecutively examined, I found 138 first-born, or 31·1 per cent., and 89 last-born, or 20·1 per cent.

When it was known, however, that almost every sixth idiot in Scotland was illegitimate (663 idiots and imbeciles giving 108 illegitimate, or 17·1 per cent.), it was thought that an element of disturbance was probably thus introduced into the foregoing figures, which might affect their value. The great majority of illegitimate children are known to be *first-born* and *only* children; while not a few of them are *last-born*, though the last of a small number of pregnancies,—say of two or three. It was, therefore, thought desirable that a fresh series of observations should be made, excluding the illegitimate, and dealing only with those born in marriage. It was also thought well to confine these observations to those cases in which not more than one idiot occurred in a family, and in which the idiocy was noticed very soon after birth—that is, in which it was probably congenital. Further, no cases were accepted but those in which the mothers at the time of the inquiry had passed the age of child-bearing, though some of them I think were widows before that age was reached. All these restrictions made it difficult to obtain a large series of observations, and account for their number not exceeding 85,—44 males and 41 females.

I sent my results in detail to Dr Matthews Duncan, who kindly drew up for me the two Tables VII. and VIII., embodying the facts in a way which makes their teaching apparent.

TABLE VII. *Showing the Comparative Frequency of Births of Idiots, and of ALL Births in First and Subsequent Pregnancies.*

No. of Pregnancy.	Per-centage of <i>all</i> Births.	Per-centage of Idiot Births.
1st	22·8	33·0
2d	17·7	18·8
3d	15·5	17·6
4th	12·1	2·4
5th	9·4	2·4
5th	7·4	2·4
7th	5·2	7·0
8th	3·9	3·5
9th	2·6	2·4
10th	1·3	7·0
11th	·9	3·5

This table is read in this way:—Of *all* the children born in Edinburgh and Glasgow in 1855, 22·8 per cent. were first pregnancies, while of the 85 idiots 33 per cent. were first pregnancies, and so on.

What the table appears to teach is briefly this:—That idiocy is more likely to occur among first and latest (7th to 11th) pregnancies than among others. This is substantially the same thing

as was taught by the first inquiry, which included 443 cases, and in which all that was asked was whether the patient was first-born or last-born.

IV. Age of Mother of Idiot at time of Birth of Idiot.

The same 85 cases are used in the following table which were used in Table VII.

TABLE VIII.—*Showing a Comparative Percentage of the Children born at different ages of Mothers to ALL Children born, and of the Idiots born at different ages of Mothers to ALL Idiots born.*

AGE, ...	20-24	25-29	30-34	35-39	40-44	45-49
P. C. of all Children, . .	22·62	39·99	23·61	14·76	5·15	0·58
P. C. of Idiots, ...	25·88	25·88	10·58	10·58	23·53	3·53

This table is read thus:—Of all children born in Edinburgh and Glasgow in 1855, 22·6 per cent. were born of mothers whose ages were from twenty to twenty-four years; while of the 85 idiots 25·8 per cent. were born of mothers of corresponding ages; and so on.

What we learn from the table is this:—That mothers under twenty-four years of age and above thirty-five are those more specially liable to have idiocy in their children.

V. Size of Idiot's Head.

During some months I measured the head of *every adult* idiot examined. There was thus no selection of cases. The lowest age of those examined was twenty-one. It will be at once apparent, that it was necessary to confine the observations to *adults*, in order to make a comparison with the ordinary sane population possible. Various measurements were made, but I shall here deal only with one, viz., the circumference, which was ascertained in the usual manner. The whole number of observations was 123, and the results are given in the subjoined table.

TABLE IX.

Circumference of Head in Inches.	MALE IDIOTS.		FEMALE IDIOTS.		Average Male Head in Scotland. From Hatters' Sales.
	Total.	Per Cent.	Total.	Per Cent.	Per Cent.
From 16 to 17 inches	...	...	...	...	...
" 17 to 18 "	2	2·9	2	3·6	...
" 18 to 19 "	2	2·9	4	7·3	...
" 19 to 20 "	6	8·8	11	20·0	...
" 20 to 21 "	17	25·0	19	34·5	6·2
" 21 to 22 "	23	33·8	16	29·1	29·9
" 22 to 23 "	12	17·7	3	5·5	55·5
" 23 to 24 "	5	7·4	...	...	7·7
" 24 to 25 "	1	1·5	...	...	0·7
Total,	68	100·0	55	100·0	100·0

(1.) As regards *males* the sum of the circumferences of the 68 was $1434\frac{1}{2}$ inches, which gives an average of $21\frac{1}{8}$, corresponding to the hat the size of which is known as " $6\frac{3}{4}$ small." Most of the six heads which had a circumference above 23 inches were unmistakably hydrocephalic, and so I think were some of those between 22 and 23 inches.

The average male head of Scotland, as learned from Hatters' sales, is "size $7\frac{1}{8}$," which is understood to represent a circumference of $22\frac{3}{8}$ or rather less. This being the case, the average adult male idiot's head is more than an inch less in circumference than the average head of the ordinary adult male population.

It will be seen from the last column of the table, that it is so rare a thing for a hatter in Scotland to sell a hat below 20 inches, as to be practically a thing which never occurs.

(2.) As regards *females*, the sum of the circumferences of the 55 was $1129\frac{1}{2}$, which gives an average of $20\frac{1}{2}$, corresponding to the hat the size of which is known as " $6\frac{1}{2}$." We do not know the average size of the ordinary adult female head, with which to compare this.

The difference between the size of the heads of male and female idiots is on the average $\frac{1}{8}$ ths of an inch, and this is not more than the difference between the heads of the two sexes in the ordinary sane population.

The general conclusion to which these figures distinctly lead is this:—That the heads of idiots as a rule are abnormally small, but that a small head is not an essential in idiocy.

ARTICLE X.—*Case of Death while under the Influence of Chloroform.* By JAMES D. GILLESPIE, M.D., F.R.C.S.E., Surgeon to the Royal Infirmary, Edinburgh.

(Read before the Medico-Chirurgical Society, 20th December 1865.)

I THINK it is the duty of every member of this Society to bring under its notice anything very unusual or startling in his practice; and, though in the present instance it is a most painful task, I have taken the first opportunity in my power of narrating to you a case of death while under the influence of chloroform which unfortunately occurred lately, and which, notwithstanding the innumerable cases of inhalation of this anæsthetic agent that I have witnessed since its introduction, is the only one attended with fatal results in my personal experience.

About the middle of November, a young lady, nearly seventeen years of age, who had repeatedly taken chloroform for the purpose of painless tooth extraction, begged of me as a great favour to

come to her house, exhibit chloroform, and extract a tooth (an upper molar), which for some days had been causing her intense pain. On examining the tooth, and finding it had been carefully stuffed, and observing, moreover, that a number of teeth had already been removed, I strongly advised her to try and bear the pain a little longer, as, from the prevalence of changeable damp weather, I thought it not unlikely that the complaint was rheumatic in its character.

I must here explain that the girl, who had known me from her infancy, told me that she applied to me for aid in consequence of the dentist who had previously drawn her teeth while under the influence of chloroform having given up employing anæsthesia.

Some days subsequently I again saw my patient, and found her still suffering acutely, and most anxious for relief; so it was arranged that on the 20th November I was to extract the tooth. On the afternoon of that day, accordingly, I went to the house, but without chloroform, and urged on her to allow me to remove the tooth at once; but as she was in a state of great excitement, and still prayed for the anæsthetic, her mother sent to the nearest druggist and procured a small quantity.

Having ascertained that the patient's clothes were loosely fastened, and with another young lady and a servant in the room to give assistance if necessary, I placed her in a recumbent position on a sofa, sprinkled a small quantity of chloroform on a napkin, and commenced its administration. This I did by holding the napkin at a short distance from the face. She had not taken above a very few inhalations when she became violent, struggling, and screaming out very loudly, as if she felt the extraction of the tooth. Having got a very small further supply of chloroform on the napkin, I was proceeding to administer it, when I thought she was sufficiently unconscious, so put it aside, and, taking up the forceps, found further operations impeded by the jaws being firmly clenched. I managed, however, to force them open, applied the forceps, and extracted the tooth. I then rose from the side of the sofa, and went to the table, which was scarcely a yard distant, to get a tumbler of water to make her wash out her mouth, when I was startled by one or two long gasping respirations, the peculiar character of which I cannot explain, but which I have only heard when a patient was dying. Alarmed, I dashed some cold water over her face, which was deadly pale; examined the pupils, and found them greatly dilated; pulled out the tongue, which was not, however, retracted; felt for a pulse, but in vain, and then became convinced she was hopelessly dead.

I commenced artificial respiration at once, and, owing to the very elastic character of her chest, I felt that air freely entered under my manipulations; the hands were placed in hot water, and the feet and chest vigorously rubbed. Her brother was despatched to try and find additional medical assistance, and, in certainly less than five

minutes, Dr Graham Weir, who was in the immediate neighbourhood, entered the room. We continued our efforts for about half-an-hour, but to no purpose, the pupils still retaining their dilated state.

The amount of chloroform altogether employed by me was 50 minims, and of this not more than a half, I should say, was actually made use of for the purpose of inhalation. I subjoin a report on an examination of the chloroform, which was kindly made for me by Dr Douglas MacLagan, Professor of Medical Jurisprudence in the University of Edinburgh, and who at present holds the office of President of this Society.

"The chloroform was contained in a small glass phial of the capacity of one ounce, which was duly corked and sealed. The bottle was labelled, and a short distance below the label a strip of paper had been pasted. The upper edge of the strip of paper indicated the level at which the chloroform stood *after* the portion used by Dr Gillespie had been removed from the bottle; whereas, the level of the fluid *before* the chloroform was taken, corresponded with the lower margin of the label. Assuming these facts to be correct, the amount of chloroform in the bottle before the operation amounted to 2 drachms and 75 minims, after the operation to 2 drachms and 25 minims, the quantity used being only 50 minims.

"The fluid contained in the bottle possessed the usual odour of chloroform. Its reaction was neutral. Its density at 60° Fahr. was 1497·7.

"1. On allowing a few drops to evaporate, a faint white residue was obtained, which, when gently heated, exhaled an *exceedingly* faint odour of turpentine.

"2. The fluid contained no trace of free chlorine.

"3. When mixed with sulphuric acid an extremely faint yellowish colorization was noticed.

"4. Potassium, when added to the fluid, disengaged gas freely.

"The above reactions indicate that the chloroform examined did not come up to the standard of purity which the British Pharmacopœia enjoins. According to the Pharmacopœia, pure chloroform 'is not coloured by agitation with sulphuric acid, leaves no residue, and no unpleasant odour after evaporation, and evolves no gas when potassium is dropped into it.' That any sample of chloroform found in commerce ever comes up to this theoretical purity is, however, highly improbable.

"Two samples of chloroform, obtained at different times from Messrs Duncan and Flockhart, and one obtained from Messrs Macfarlan, were examined and compared with the suspected chloroform. Although these samples appeared of admirable quality, they did not come up to the Pharmacopœia standard, and resembled very closely the sample under examination, with regard to which chemical analysis has completely failed in discovering the cause of any deleterious or poisonous action which it may have exerted."

An examination of the body was made by Dr Haldane, in the presence of Dr Graham Weir, Dr Wylie, and myself, and nothing whatever abnormal was found, save that the left ventricle of the heart was very firmly and unusually contracted, and that it, as well as the left auricle, was perfectly empty. The right side of the heart was not gorged with blood, and there was no clot in any of the cavities. The lungs were quite natural in appearance, as also the brain, and all the other organs.

I am at a loss to explain the cause of death in this case, but am satisfied it commenced at the heart. Were I to hazard an opinion, I would say my own impression is, that the patient, who was of a very excitable temperament, and who had been for many days suffering much pain, and intensely anxious for the removal of her tooth, must have been seized by a peculiar variety of syncope, characterized by spasm of the left side of the heart; and the fact of her being under the influence of chloroform prevented a healthy action of the heart being re-established.

I have ascertained that a very much larger quantity of chloroform was always previously required to produce anæsthesia, and when I administered it to her myself some years ago, I recollect it required a large dose to produce the desired effect.

I have taken a much longer time to narrate this painful case than actually occurred, for the whole time occupied should be counted by seconds, not minutes.

The chloroform bottle was sealed up by me immediately after the death of my patient; it was exactly half full, reaching up to the lower margin of the label when I commenced its administration, and the upper margin of the paper mark lower down indicates the amount altogether employed. This was found to be only 50 minims, a very small quantity indeed, when you recollect that it was sprinkled, at two successive periods, over the wide surface of a napkin.

Part Second.

REVIEWS.

Contributions to Dermatology. No. III. On Psoriasis and Leprosy.
By T. McCALL ANDERSON, M.D., Glasgow. London: Church-hills: 1865.

DR McCALL ANDERSON is already favourably known to our readers as the author of two works on Dermatology; in the first of which he described the parasitic affections of the skin, in the second the

varieties of eczema. We heartily welcome a third volume, in which the modern views as to lepra and psoriasis are most clearly expounded. The author's views are founded upon diligent study of cases in the large hospitals of Paris and Vienna, and in many respects are those taught by Hebra, but they are modified and corrected by the result of his own experience, and embrace several interesting new facts in regard to the disease.

Dr Anderson agrees with most recent authorities in regarding lepra as merely one of the varieties, or rather stages, of psoriasis, and he gives the following general description of the affection :—

“Psoriasis may be defined as a chronic eruption, characterized by the appearance of dusky red or coppery, slightly elevated patches of various shapes and sizes, covered with silvery white, imbricated, and very adherent scales; the elbows, knees, and head being the parts most certainly attacked; there being a very varying amount of itching, but no exudation on the surface of the skin, and no general reaction.”—P. 2.

Of this disease he describes varieties—*P. punctata*, *guttata*, *nummularis*, *universalis*, *circinata* or *lepra*, and *gyrata* or *figurata*—whose characters are sufficiently indicated by their names; but, in addition, he gives an account of a new variety, *P. rupioides*, which he was the first to observe. He says,—

“There is a peculiar appearance which psoriasis sometimes assumes, which I first observed a good many years ago, and which I first described. When it occurs, it may be regarded as a stage intervening between the so-called *Psoriasis guttata* and *nummularis*. In it, the accumulation of epidermis takes place to an unusual extent, so that on many of the patches it assumes the shape of large conical crusts, marked by concentric rings. In fact, they exactly resemble in shape limpet-shells, and, from their likeness to crusts of rupia, I have called this variety *Psoriasis rupioides*. Except in the shape of the crusts, however, there is no connexion whatever with rupia; and on removing a crust there is no ulceration beneath, but a slightly elevated dusky red, rounded surface, is exposed to view, which sometimes bleeds a very little. When the first case came under my observation, I executed a drawing of it, making use of the actual crusts removed from the patient who was affected, and pasting them upon the picture, so as to give a very accurate idea of the appearance of the sufferer. This drawing is now hung in the museum of the Glasgow Royal Infirmary.”

We remember inspecting this ingeniously constructed picture, and can testify to the accuracy of the description given. We have also lately had the opportunity of seeing a case in the Edinburgh Infirmary which bore a considerable resemblance to those described by Dr Anderson. The only point of difference was the absence of concentric rings on the crusts. Dr Anderson has, since he first noticed it, met with several examples of the variety, and an excellent chromolithograph of one of them is prefixed to the volume.

The remainder of the first chapter is devoted to an account of the symptoms and seat of the disease; the elbows and knees being most commonly affected. The head, the trunk, the face, and the backs of the hands and feet, and the nails, being among its less frequent seats.

With regard to etiology, Dr Anderson gives evidence of its hereditariness, of its rarity in strumous individuals, its frequent dependence upon debility and overwork, its relations to sex, age, and social position, and to season; he finds that it commonly commences before the age of 25, and often as early as the eighth or ninth year; that sex, according to his own statistics of 196 cases, is unimportant; while the statements of Hebra and Erasmus Wilson directly contradict one another. Its comparative mildness, or complete absence in summer, and its severity during winter and spring, the unimportance of local irritation, and of social position, as causes, are also brought out.

In a chapter on the diagnosis of psoriasis clear rules are laid down for distinguishing it from similar diseases, and particularly from the syphilitic variety. The extent, size, and shape of the patches, and still more, their seat; the simple choosing, almost without exception, the elbows and knees, the specific affecting the inner aspect of the limbs, and being the only form met with on the palms of the hands and soles of the feet; the colour, the appearance of the scales, the duration of the eruption, the age of the patient, the itchiness of the eruption—a condition almost constantly absent in the syphilitic—the occurrence of relapses, the origin of the disease, the concomitant symptoms, and the effect of remedies, are the other points which help to the differential diagnosis.

With regard to the general history of the disease, the author dwells upon the frequency of relapses, which occur in 90 per cent. of the cases, and render the prognosis, which would otherwise be favourable, on the whole, unsatisfactory.

The work concludes with a sketch of the treatment, constitutional and local, which we commend to the attention of every practitioner. The advantages of hygienic measures, of attention to diet and regimen, of change of air and scene, rest from work, freedom from business care, are clearly shown. The value of arsenic and alkalies internally is generally known, but Dr Anderson recommends a plan of treatment in some cases by cod-liver oil—a remedy not generally held in high repute in this disease. He says, "In those rare cases in which the eruption appears in strumous persons, there can be no doubt theoretically that cod-liver oil should be used, while I have myself had practical demonstration of its value."—(P. 43.) He narrates several very striking examples of the benefit resulting from this plan. Among the local applications, preparations of tar, of course, occupy the chief place. *Pix liquida*, *oleum cadini*, *oleum rusci*, and the *tinctura saponis viridis cum pice*, are the forms most suitable. He recommends that they should be firmly rubbed in night and morn-

ing, and so far as possible the skin should be kept in a tarry atmosphere. It should also be borne in mind that occasionally a smart inflammatory attack follows this application, and that some constitutional disturbance is not unusual.

The uses of blisters, of localized and general hydropathic treatment, of mineral baths, of mercurial ointments, etc., we have not space to consider, but all of them are judiciously discussed in the closing chapter of the book.

We heartily recommend the work to the notice of the profession, and congratulate Dr Anderson on having produced a third valuable contribution on a subject which has hitherto been too little studied in this country.

One word in closing as to the dedication. It is to the memory of Dr Andrew B. Buchanan, the colleague, companion, and friend of the author, and a valued contributor to this journal, whose untimely end, at the dawn of a life of the brightest promise, has been deeply regretted here as well as in the western metropolis.

On the Temperature of the Body as a means of Diagnosis in Phthisis and Tuberculosis. By SYDNEY RINGER, M.D., Professor of Materia Medica and Therapeutics at University College. London: Walton and Maberly: 1865.

OF late years considerable attention has been paid to the temperature of the body as a means of diagnosis and prognosis in various diseases. The results have been of considerable practical importance, and Dr Ringer's little work is a valuable addition to this branch of medical literature.

After a few introductory observations on the method of using the thermometer, Dr Ringer lays down in the form of propositions the conclusions he has arrived at. These conclusions are then considered seriatim; and the remainder of the work is occupied by an account of the twenty-four cases on which it is more particularly based.

Dr Ringer's propositions are the following:—

"1. There is probably a continued elevation of the body in all cases in which a deposition of tubercle is taking place in any of its organs.

"2. This elevation of the temperature is probably due either to the general condition of the body (tuberculosis), or to the deposition of tubercle in its various organs (tuberculization).

"3. This elevation is probably due to the general condition (tuberculosis), rather than to the deposition of the tubercle (tuberculization).

"4. The temperature may be taken as a measure of the amount of the tuberculosis and tuberculization, and any fluctuations in the

temperature indicate corresponding fluctuations in the severity of the disease.

"5. The temperature is a more accurate indication of the amount of tuberculosis and tuberculization, than either the physical signs or the symptoms.

"6. By means of the temperature we can diagnose tuberculosis and tuberculization long before the physical signs and symptoms are sufficient to justify such a diagnosis.

"7. By means of the temperature we can diagnose tuberculosis even when during the whole course of the disease there are no physical signs indicative of tubercular deposit in any of the organs of the body, and in which cases the symptoms (apart from the temperature) are inadequate to enable us to arrive at such a diagnosis.

"8. It is probable that by means of the temperature we can conclude that the deposition of the tubercle has ceased, and that any physical signs that are present are due to obsolescent tubercle and the chronic thickening of the lung tissue between the tubercular deposit.

"9. It is probable, though further observations on this point are necessary, that the temperature of the body affords a means by which we can diagnose between diseases in which the symptoms and physical signs are either too scanty or too much alike to enable us to decide between them."

We cannot follow Dr Ringer through his illustration of these propositions, but can merely remark that, although we cannot go the full length of all of his statements, he has proved that a continued elevation of temperature is an important sign of phthisis, and that it is of considerable value in the determination of doubtful cases.

Lectures chiefly Clinical. By THOMAS K. CHAMBERS, M.D., etc., etc. Fourth Edition. London: Churchills: 1865.

WE have, on two former occasions, directed the attention of our readers to this excellent work, which originally appeared under the title of the "Renewal of Life." It has grown much in bulk since its original publication, although there is but little difference between this edition and its immediate predecessor. The reason of this is explained by Dr Chambers in the following touching words:—"While that edition was in the press I was struck down by a dangerous illness, and have been out of work nearly eighteen months. On my return, by God's grace, to health and labour, I find it sold off, and a new one called for before I have again occupied the teacher's chair. I have, therefore, no fresh matter to offer, and must content myself with a thorough revision, and the addition of an index unavoidably omitted last year."

We have to congratulate Dr Chambers on his restoration to health

and usefulness, and have observed with pleasure that he has given the result of his personal experience of the Italian climate in a series of three excellent lectures lately published in the "Lancet."¹

We have no doubt that, after no long interval, we shall have the pleasure of welcoming a fifth edition of the Clinical Lectures.

Part Third.

PERISCOPE.

MIDWIFERY.

FATAL CASE OF TUBAL EXTRA-UTERINE FÆTATION.

At a recent meeting of the Obstetrical Society of London, Mr Truman read a report of a fatal case of tubal extra-uterine fætation where death took place from sudden rupture of the tube at the end of the third month of pregnancy.

Mr Roper remarked that in the two cases of tubal pregnancy which had occurred in his practice, complete rupture of the cyst took place at the seventh or eighth week of pregnancy, and death rapidly followed from hæmorrhage into the peritoneal cavity. Coincident with the first symptoms in each case there were symptoms of menstruation. He believed that when the ovum was in the free part of the tube (not that part which passes through the uterine wall), rupture, partial or entire might be expected at the seventh or eighth week of pregnancy, as at this period the ovum became so large that the tube would not allow of further distention without giving way; it was therefore at this period that we might look for the first symptoms of the accident. Diagnosis of the nature of the case would be importantly confirmed by noting the time at which the first symptoms happen (whether at a catamenial period), and the actual existence of a menstrual discharge. The uterus being unoccupied, the discharge might be regarded as truly menstrual, coming from the lining membrane of the uterus; and the increased functional activity of the reproductive organs at this particular period, the additional afflux of blood, and the turgesence of the vessels concerned in nourishing the ovum, were coincident circumstances likely to determine or hasten rupture.

The *President* called attention to the fact that previously to the rupture of the cyst—that is, previously to the shock and collapse which mark the epoch of rupture—there was usual to observe a discharge of blood per vaginam. The hæmorrhage he believed was commonly produced in this way: The ovum growing at about two or three months, the period when chorion is rapidly forming placenta, the rate of growth of the villi greatly exceeds that of the sac. The Fallopian tube is unfitted to harbour the ovum; it cannot keep pace with the shooting out of villi. These become detached, and hæmorrhage results. Then, under combined distention from growing ovum and hæmorrhage, rupture takes place. It was by a perfectly analogous process, in his (*Dr Barnes*') opinion, that hæmorrhage took place in placenta prævia. Here is an *error loci*, as in tubal pregnancy: the ovum grows in the wrong place. The cervical zone is not well adapted to grow with the growth of the ovum. Hence a time arrives, about the fifth or sixth month generally, when the shooting villi get detached from the uterine wall, and hæmorrhage results. This explanation, the *President* observed, differed entirely from the theory generally received, which attributed the hæmorrhage to the lower segment of the uterus growing

¹ See the *Lancet* for Oct. 21, Nov. 4, and 18.

away in some manner from the ovum. The cases of tubal gestation and placenta previa were strictly analogous in this respect. Both were examples of ectopic gestation: the ovum growing in a structure not fitted to grow in proportion with it.

Dr Playfair asked whether *Dr Barnes* thought that, either with the assistance of the symptom he had pointed out or by any other means, he considered it possible to arrive at the diagnosis of a tubular foetation before rupture had occurred. There could be no doubt that the question of diagnosis was one of extreme importance, because if the existence of a Fallopian pregnancy could be ascertained with a tolerable degree of certainty, it might perhaps be possible to adopt some method of treatment which would save the patient from the almost certainly fatal result following rupture. Thus for example, it might be worthy of consideration whether the operation of gastrotomy and the ligature and removal of the Fallopian tube and its contents might not be successfully resorted to. The risk from such an operation would be infinitely less than that which follows the rupture of the cyst. At present the difficulties of diagnosis are undoubtedly almost insuperable; but *Dr Barnes'* interesting observation seemed to indicate a means by which they might in time be lessened or overcome.

Dr Braxton Hicks thought the collection of the histories and of the post-mortem examinations of such cases very valuable, inasmuch as it would help to guide us as to the possibility of operative interference, even if the diagnosis could be clearly made out. From what he had seen he thought, in the majority of these cases, there would co-exist circumstances, such as adhesions, etc., which would entirely prevent our getting below the sac, so as to tie it. Even many of the cases of tubal foetation were caused by curving of the tube by old contractions or other causes, rendering any chance of benefit hopeless.

Dr Playfair said that he believed, as a rule, extensive adhesions would not be found in cases of tubular foetation before rupture had taken place. After the foetus had escaped into the cavity of the abdomen, if the patient survived the shock of the rupture, an adventitious cyst was formed round it, which was usually extensively adherent to the surrounding parts; and it would doubtless be very difficult to remove this *en masse* by gastrotomy; although *Mr Spencer Wells*, in a previous discussion on the subject, had suggested the possibility of this being done. But in a tubular foetation before rupture he (*Dr Playfair*) apprehended no such difficulty would be met with.

Dr Graily Hewitt observed that the diagnosis of cases of the kind now under consideration was, and he believed always would be, very difficult; and he feared that but little reliance could be placed on the circumstance adverted to by the President,—viz., the occurrence of hæmorrhage as a precursor of rupture, inasmuch as it was well known that in cases of extra-uterine pregnancy hæmorrhage from the uterus was not unfrequent, and that at periods some time antecedent to the rupture. With reference to the time at which rupture occurred in cases of Fallopian pregnancy, there was a very important fact to be borne in mind,—viz., that, as *Kussmaul* had pointed out, many cases of supposed Fallopian pregnancy were really cases of bicorned uterus; and the difference in the result between such a case and one of Fallopian pregnancy with a normal uterus was necessarily very great. In the one case, rupture at a comparatively early period usually occurred; whereas, in the other, the pregnancy might proceed normally.

Dr Ritchie made some remarks on the interesting question of the period at which rupture of the sac usually occurs in tubal pregnancy. He referred to two cases which had been observed by a French accoucheur, where there was some reason to believe that the foetus, after nine months of intra-tubal life, had been extracted by the natural maternal passages. He surmised that perhaps those cases were to be explained in the manner just referred to by *Dr Graily Hewitt*: that they were perhaps instances of double uterus, where the entrance to the second horn or loculus had been mistaken for the uterine orifice of the Fallopian tube.

The President, *Dr Barnes*, observed that the question now started—whether

gastrotomy might not be performed *before* rupture of the sac—must greatly depend upon means of diagnosis. He did not think that the preliminary hæmorrhage to which he had called attention, even aided by other indications, could ever raise more than a strong presumption: the evidence could hardly be great enough to justify the operation. Again, rupture commonly took place not later than three months. He had performed post-mortem examinations in two such cases. In both there was great difficulty in discovering and isolating the parts concerned. In the living subject the task would often be impossible.

ON THE DIET SUITABLE AFTER CHILDBIRTH. BY HENRY LOWNDES, ESQ.,
LIVERPOOL.

AFTER giving an account of the views on this subject entertained by many distinguished obstetricians, Mr Lowndes describes his own practice in the following terms:—

"When I began practice, and for some years, I used to go by the precepts that almost all agree in, giving gruel and other thin slops for the first few days; and I can remember what repugnance many patients had to this substance, which resembles no ordinary article of human food in this country. I had to preach sermons to them on the extraordinary virtues of this—I do not know what to call it. We are told by Edgar Poe of beings that are "neither man nor woman; they are neither brute nor human; they are ghouls;" and this material is neither food nor drink, it can only be described as gruel. It can neither satisfy the cravings of hunger, nor minister to the sense of taste or that of smell. It is unknown who first invented such an unlikely article of diet. However, I found, in many cases, that patients would not be persuaded by my eulogies of this preparation, and would sometimes take broth, sometimes beer, and sometimes meat, and would confess to it, and certainly used to look all the better. Gradually, whether it was from this experience, or from something I had read, or from seeing some one else's practice, I cannot tell, I began to try a good diet, without over-stimulating. For at least three years, I have acted in nearly all cases, except after first labours or labours unusually severe, on the following system.

"I give some stimulus, either wine or ale, directly after the labour is over. On the day of confinement, I restrict the patient to tea or (if she likes indeed) gruel, with toast or bread and butter. [In addition to these, I find a little bread and milk very desirable.] On the following day, the same, with a large breakfast-cup of good strong broth for dinner. The third day, a small lean chop or a little chicken. The next day, the same, with a little wine and water or ale, if the patient be accustomed to these beverages. Of course, all cases cannot be treated alike, and a lower diet may sometimes be necessary; but I much more frequently find it useful to begin with stimulants rather sooner than I have indicated.

"In first cases, and in cases of severe protracted labour, we know that the soft parts must have undergone great pressure and bruising; and I generally keep these patients a longer time without alcoholic stimulus.

"I shall not trouble you with a series of cases and their results; but I can say with confidence, that I have met in no case with any ill consequences following the mode of diet I have mentioned. It is true, that I have seen mischief occur in more than one case where the patient has thought because she might have a chop for dinner she might therefore have one for supper also, and so forth; but the evil results of excessive indulgence are not to put a stop to all moderate use of good things. By this mode of diet, I believe the patients escape, in a great measure, those muscular pains that are so frequent when a patient begins to move about, and also those affections of the breast and nipple, in which the pain is out of all proportion to the actual lesion.

"As it is always agreeable to make one's theories and practice agree, I will submit a few considerations in favour of a more 'feeding' system.

"Supposing for a moment that we even take labour to be what it has been called, a severe traumatic lesion, do we condemn a patient that has suffered

from a compound fracture to an immediate course of gruel and tea? and if we did adopt such a course, should we render him more likely to escape, or better able to bear, the inflammatory action that will sooner or later occur? Experience proves the reverse. I will speak again of a severe compound fracture. For some time after the accident, no change of any consequence takes place in the damaged parts; they, in common with the whole system, have received a shock. In a varying time, inflammatory action shows itself in the part, and if the external wound do not close at once, that action will be violent and the whole system suffer great febrile disturbance. Now, a cooling and restricted diet may be necessary; but the patient will be much better able to bear the shock of the inflammation, if that first interval of repose has been employed in overcoming, by suitable diet and stimulants, the shock of the accident. Indeed, nothing is so certain to bring on one evil, traumatic delirium, as too low a diet after a severe surgical shock.

"There are, however, many points that sufficiently make a distinction between the process of labour and a severe traumatic lesion; and I need, I think, only refer to one—viz., the absence after labour, as a rule, of any inflammatory reaction.

"Then, instead of considering labour as a severe surgical accident, let us consider it in the light of a great physiological process; one of those processes in which the voluntary and reflex nervous systems are both concerned. No one would think of calling the act of micturition or of defæcation a traumatic lesion, and yet these acts seem hardly to differ from the act of labour except in degree and in frequency of occurrence. The frequency of these troublesome acts deprives them of the terrors that might perhaps invest them if, like labour, they came at distant intervals. The act of sneezing may be quoted as a most alarming convulsive phenomenon to which we have got quite used.

"Labour differs from these other actions in being generally of a much more severe character; it is often accompanied with much severe muscular effort, and it is necessarily followed by much fatigue. This fatigue, and the waste of tissue that has taken place, must be recruited by stimulants and nutriment; and these I believe, in moderate amount, may be given during labour when that is long continued, and immediately after labour, with the greatest advantage.

"Puerperal fever is perhaps the only complication of an inflammatory character that we need have much fear of during the first few days; and this disease is of so low a type, and bears so much resemblance in its nature to typhus and typhoid fevers, that we may very well suppose that, like these, it will be more apt to attack those who are in a low feeble state than those who are in better condition. That good feeding does not conduce to this disease, my own limited experience would certainly show.

"Beyond the evidences of its contagious nature, the causes of this insidious and fatal disease seem wrapped in obscurity; but we suppose that a good diet and a cheerful mind must be powerful prophylactics to those exposed to this contagion.

"At one time all the apertures for the entrance and exit of air from the lying-in room were carefully stopped up, on the idea of the patient being now especially liable to suffer from cold; bed-clothes were heaped on, and at one time spiced cordials were diligently given. Now we take pains to ventilate the room well; but, while we do not keep off the access of cool air, we are very apt, if we keep to another part of the old system, to have our patient in a very unfit state to bear the air. For a constant course of gruel in vast quantities and tea relax all the tissues, and the skin among the rest. The female acquires, after a time, a perfectly sodden appearance, which is quite characteristic; the flesh pale and soft and moist, the abdomen full of flatus.

"But we are told that such a diet is good for the flow of milk. In the early period of lactation, however, there is likely to be too much, rather than too little milk; and, with regard to the quality of that milk, if we compare it with other secretions, we must suppose that it will be most perfect in its character, and so most suitable to build up the child, when the general health of the

mother is in its most perfect state. And here I would say a word of milk-fever, which, perhaps, I ought to have mentioned before. We read much of it, but I believe that, except in primiparæ, the secretion of milk commences without any febrile phenomena whatever, and in the case of primiparæ, I have already explained that other causes lead us to adopt a rather more severe regimen than in other cases.

"The whole subject of diet is so extensive, for it must be adapted to the season, the constitution, and the habits of life, that I shall be excused if I have only on this occasion dipped a little into the matter; and I shall be glad to know from those who have a more extended experience in midwifery, what line of practice they adopt; and whether there are any who still adhere to the rigid rules that we seem to have learnt rather from Celsus, or some of his predecessors, than from Nature."—*British Medical Journal*.

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLV.—MEETING II.

6th December 1865.—Dr HANDYSIDE, *Vice-President*, in the Chair.

I. LUPUS OF THE EYELID.

Dr Argyll Robertson exhibited a patient suffering from a rare form of disease, lupus of the eyelid. Lupus seldom affected the eyelids, so much so that some denied that it ever occurred in that situation. In the last May number of the *Dublin Quarterly Journal* a case was recorded by Dr William Stokes, in which both eyelids and eyeballs were affected to such a degree as to require operative interference: a plate showed the appearance of the patient before and after the operation. Dr Robertson's patient had for eight years suffered from an affection of the conjunctiva, redness and watering of the eye, and these symptoms, though relieved, had never entirely disappeared. The present disease had commenced as a pimple on the ala of the nose, which ulcerated, and gradually extended through the nasal cartilage, presenting all the characters of lupus. The patient had, twelve months ago, come under Dr Robertson's care at the Southern Eye Dispensary. More than half of the lower lid was then eaten away by an ulcer, which the patient stated had commenced three months previously, and for which no treatment had been adopted. The ulcer had since spread in one direction, while it had cicatrized at other parts. The chief points of interest in this case were—*First*, That the movements of the upper lid and the eyeball were interfered with a good deal by the cicatrization. The patient could now scarcely raise the lid so as to show the cornea. At first he could raise it so as to display part of the pupil, and to enable him to see, but the eyeball even then was almost entirely fixed. *Second*, Although the ulceration had extended to the eyeball it had only affected the most superficial coats. The cornea, when last examined, seemed unaffected, and was clear and transparent and the sight perfect. This showed the tendency of lupus to extend superficially. The diagnosis had at first been somewhat difficult: the case resembled one of syphilitic ulceration of the eyelids. There was, however, no history of syphilis, and the affection of the nose was distinctly lupus. Under treatment a considerable improvement had taken place, and the ulceration was nearly cicatrized. The treatment had been chiefly constitutional. Arsenic had been repeatedly administered in small doses till the system had been affected. Local

applications had consisted of weak oxide of mercury ointment, solution of nitrate of silver and of sulphate of copper, black-wash, lead, and opium. The chief annoyance had been the glueing together of the eyelids, and the ingrowing of the eyelashes. The eyelashes had been frequently extracted.

II. CANCER OF THE ŒSOPHAGUS.

Dr Grainger Stewart showed a specimen of epithelial cancer of the Œsophagus. The patient had been admitted under his care into the Infirmary last September. Swallowing was at that time interfered with, but a probang could be passed without much difficulty. The operation was not, however, repeated, as the case presented at first a good many of the symptoms of aneurism. The most interesting point in the case was the loss of voice, which came on gradually, and the circumstance that the patient was subject to attacks of great dyspnoea. On account of the latter, tracheotomy was performed by *Dr Alexander Miller* a few days before the patient's death. The operation gave immediate relief, and the patient died rather from inanition than from interference with the respiration. The cause of the dyspnoea was pressure upon both recurrences, which were involved in the growth. The patient had been examined by means of the laryngoscope by *Dr Wyllie* and *Dr Stewart*, and it was found that neither side of the glottis moved readily, and there was a certain degree of thickening of the parts, and that the glottis could not be completely closed.

III. SYPHILITIC DISEASE OF THE LIVER.

Dr Grainger Stewart showed a specimen of syphilitic deposit in the liver: it was not the waxy, but the more ordinary form. On one side of the preparation the disease was in an earlier stage, and had a peculiar translucent fibrous appearance; on the other it was more advanced, and had become cheesy. At other points, syphilitic cicatrices were well marked. Otherwise the liver appeared tolerably natural; but, on microscopic examination, some of the small branches of the hepatic artery were found to be in a state of waxy degeneration.

IV. CALCAREOUS DEGENERATION OF A FIBROUS TUMOUR.

Mr Amundale showed a tumour which had been removed from over the sternum of a patient. From the history the tumour was apparently fibrous; but, on section, it was found that the entire tumour was affected with calcareous degeneration. Such complete degeneration of fibrous growths was rare, except in the case of fibrous tumours of the uterus.

V. CALCULUS IN SUBMAXILLARY DUCT.

Mr Amundale showed a calculus from the submaxillary gland, much larger than was usually found in this situation. When a concretion of this kind was formed in the gland it was sometimes liable to be mistaken for malignant disease of the gland.

VI. CASES OF FRACTURE REQUIRING AMPUTATION.

Mr Amundale showed the bones from two cases of fracture. The first was a case of simple comminuted fracture of the lower end of the tibia and fibula. The patient, a man fifty years of age, fell from a ladder and landed on his feet. There was much comminution of the bones. Suppuration took place in the knee-joint, and, as hectic was setting in, amputation became necessary. The second case was one of simple fracture, but was rendered interesting by an artery having been ruptured, and a sort of false aneurism formed, which pressed upon the main artery. Gangrene was the result, and for this amputation was required.

VII. REMOVAL OF AN INJURED NEURALGIC TESTICLE.

Dr P. H. Watson showed a testicle with the cord as far as the external abdominal ring, which he had removed from a gentleman. Some months

before, while going through a hemp-cleaning factory, the patient got entangled in the machinery. He was seized by the seat of his trousers, one testicle was torn off, and the other was injured. The patient had been married for several years, but after the accident he became quite impotent. When Dr Watson saw him the testicle was rather large, the epididymis was distended, and evidently converted into a cystic dilatation. On examination, it was clear that the vas had been torn through. At this point, the continuity of the canal had been destroyed, and the extremity on the further side of the cicatrix becoming gradually dilated, constituted a cyst about the size of a marble. The patient suffered from intense neuralgic pains, which made his life miserable. Accordingly, Dr Watson agreed to remove the testicle. The operation presented a few peculiarities. A needle was first passed beneath the cord at the site of the external abdominal ring so as to secure the vessels of the cord and prevent bleeding. The testis was then drawn downwards, and a pair of oesophageal forceps having been employed to grasp the cord and textures of the scrotum so that the testicle projected beyond their blades, the oblation of the organ was effected with a single stroke of a bistoury. There was no bleeding from the cord, but a few small vessels of the scrotum were secured upon the face of the cut surface by acupressure. A pad of cork was now placed on each side of the scrotum, and transfixed with needles in fixed handles, so as to carry deep sutures through and through the cut scrotal textures on two levels. In this manner, when the wires were fastened off, the cork pads were squeezed together, and the whole surface of the incision was kept in accurate contact, while the edges were secured by a few points of ordinary interrupted wire suture. The result had been most satisfactory. The entire wound had healed by the first intention, which, after the operation of castration, was admittedly a most unusual occurrence.

VIII. CASES OF EXCISION OF THE KNEE-JOINT.

Dr P. H. Watson exhibited photographs showing the appearance of the limbs in four adult cases after excision of the knee-joint. In all, the limbs were firmly ankylosed, there was but little shortening, and the patients walked with comfort and facility. They had all been treated after the operation by the same apparatus, which upon a previous occasion Dr W. had exhibited to the Society. It consisted of a single iron-rod of the thickness of a No. 10 bougie, extending from the fold of the groin to the end of the toes, applied along the front of the limb, and in close contact with the surface throughout its whole length, except at the knee, where it formed an arch. Behind the limb a Gooch's splint was applied, extending from the fold of the nates to beyond the heel. For four or five inches above its lower extremity the central portion of the splint was cut out so as to avoid pressure upon the calcaneum or tendo Achillis. The splint was also diminished in lateral width for about six inches at the situation of the operation, so as to leave the extremities of the semilunar incision uncovered. The iron-rod and the Gooch's splint were retained in contact with the limb by means of an open wove bandage, which was soaked with a cream of plaster of Paris after it had been applied. Only the incision was left uncovered, so as to provide free exit for the escape of matter. The apparatus thus applied never required to be touched from its first arrangement till consolidation was complete, and all retentive measures could be dispensed with. In no case were the patients kept for more than five weeks in bed, while from the very first day they could sit up in bed and move the limb about, for, as it was slung, and the rod, splint, and plaster bandage adhered closely to the limb, there was no risk of any movement of the pelvis rotating the thigh and thus producing displacement, a result unavoidable by the use of any other apparatus hitherto recommended. It was impossible for any patient to lie continually upon the back for four or five weeks without at least easing the hips from side to side. If measures were employed to secure a constant occupancy of the same posture, bedsores must result; if it was not secured, rotation of the thigh ensued, and the leg would thus be found ultimately to occupy a position of inversion as respects

the axis of the femur, while more or less angulation in an outward direction would occur at the junction of the femur and tibia. It was his early experience of the difficulties met with in securing a completely satisfactory result by means of the various methods hitherto recommended that had led to Dr W.'s devising this plan of after-treatment.

IX. CASE OF ACUTE ATROPHY OF THE KIDNEYS AND LIVER.

Dr Grainger Stewart read a paper on this subject, which will be found at page 633 of this number of the Journal.

Dr Matthews Duncan, alluding to the question whether putridity of the fœtus could have had any influence on the health of the mother, remarked that putridity to the highest degree, both of intra and extra-uterine conceptions, might exist without any apparent injury to the female. Not long ago he had delivered a fœtus from an extra-uterine cyst through the anus, the putridity of which could not have been exceeded, and where the mother had not a bad symptom. The same thing was not uncommon in the case of uterine conceptions. Still it was well known that poisons could reach the mother from the fœtus; that poisons injected into the fœtus could affect her.

Dr Moir confirmed very much what Dr Duncan had said. He did not remember any case where the woman had seemed to suffer from the putrid state of the child. He rather thought that, in Dr Stewart's case, the fœtus had died in consequence of the diseased state of the blood of the mother.

X. ON THE ACTION OF MEDICINAL PREPARATIONS OF IRON ON THE TEETH.

Dr Smith read a paper on this subject, which will be found at page 631 of this number of the Journal.

XI. CASE OF BRONCHOCELE.

Professor Laycock, on the part of Dr M'Intosh, of the Perth District Asylum, read a paper on this subject, which will be found at page 583 of this number of the Journal.

Professor Laycock said that he had seen Dr M'Intosh's case. It was rare to have so large a bronchocele in this country. Dr M'Intosh had accompanied him in a journey through Savoy and Switzerland, where they had seen many cretins; in some the tumours of the thyroid were larger than in this case, but in the great proportion they were smaller. To Dr Laycock, the most interesting point in the case was the extreme anæmia and the entire absence of anæmic murmur. He would suggest to the members of the Society that they should observe such cases, and he thought they would find that the doctrine of anæmic murmurs had been received too easily. From time to time there were, in the clinical wards, cases of great anæmia. At present there was one, a man, dropsical and highly anæmic, and in whom there was no trace of murmur. On the other hand, we met with cases of murmurs of the same character which arose under emotional conditions. For instance, there was a lad of seventeen, hypochondriacal, who, when the class approached to examine him, became excited, and then a loud sawing murmur could be heard at the base of the heart and in the aorta: as the excitement abated the murmur grew fainter, and gradually disappeared. This showed that we had still much to learn as to the relation of murmurs to bronchocele, staring eye, anæmia, and so on. Many of the emotional murmurs were unamenable to ordinary treatment. Iron was sometimes useful, sometimes it did harm. Dr Laycock had seen a case of vascular bronchocele in which iron increased the cardiac irritability. In reference to the pathological anatomy of bronchocele, the Society would recollect that, on a former occasion, he had stated that scrofulous degeneration of the thyroid was rare. An eminent member of the profession had rather doubted this, and said he had seen numerous examples of it. This was, however, quite contrary to his (Dr Laycock's) experience. The thyroid gland was made up of a number of bodies, which easily underwent plastic exudation, and this again might be converted into cartilaginous or osseous matter. In Dr M'Intosh's case a cer-

tain amount of cartilaginous transformation had taken place. It was important to bear in mind that scrofulous matter was not met with in the thyroid gland. This was of some interest as to treatment, for scrofula generally suggested cod-liver oil, iodide of potassium, and the like. The form of disease which actually existed in the thyroid was probably more incurable than scrofula. He had never seen a large bronchocele cured.

Dr Sanders observed that the term "cartilaginous" was sometimes used rather indefinitely to denote any smooth, firm growth. Precision of language was, however, desirable, and he would wish to know whether there were any of the real elements of cartilage in the parts of the tumour described as cartilaginous by *Dr McIntosh*.

At the request of *Dr Laycock*, *Dr Stewart* agreed to make a microscopic examination of the tumour.

SESSION XLV.—MEETING III.

Wednesday, 20th December 1865.—Professor DOUGLAS MACLAGAN, *President*, in the Chair.

I. CASE OF DOUBLE MONSTROSITY.

Some conversation took place regarding Jean Battista Dos Santos, a remarkable case of double monstrosity, who had recently been exhibited in Edinburgh.

II. CASE OF DEATH WHILE UNDER THE INFLUENCE OF CHLOROFORM.

Dr Gillespie read a paper on this subject, which will be found at p. 645 of this number of the Journal.

The President felt sure that all the members of the Society must sympathize with *Dr Gillespie* in the sad occurrence he had narrated to them, and not less in his determination of bringing the subject before them.

Dr Roberts alluded to a similar occurrence having, some years ago, happened in his own practice. He would recommend that, in all cases where patients desired to take chloroform during tooth extraction, they should be accompanied by their ordinary medical attendants.

Dr Haldane corroborated *Dr Gillespie's* account of the post-mortem examination of the body. He had no doubt that death had been due to syncope, and quite agreed in the opinions expressed by *Dr Gillespie*.

III. ADDRESS BY THE RETIRING PRESIDENT.

The President, on leaving the chair, delivered the following valedictory address:—

The time has now come when, in conformity with the laws of the Society, I cease to be your president, and I now beg leave, in conformity with its unwritten law, its established practice, to address you shortly before vacating the chair. I propose to take a brief retrospect of the proceedings of the Society during my term of office,—a wholesome proceeding, which enables us to take stock, as it were, and to consider whether or not we are fulfilling the purposes for which we are associated.

Allow me to express my satisfaction at the prosperous state of the Society, as regards its funds and list of members. We have a tidy little balance in our treasurer's hands; and as time and mortality make gaps in our ranks, we are able to fill up the vacant places with young and able recruits. I hope that the Society will never scruple to use its funds for any purpose which may promote the object of its existence,—the diffusion of professional knowledge among the practitioners of medicine in Edinburgh. I trust, therefore, that the Society will approve of that act of the Council, by which a small sum from the funds was used, to enable the members to inspect the remarkable case of monstrosity they were enabled to see last week.

I trust, however, that I may, with truth, say that our prosperity has not consisted solely in accumulating funds and keeping up the number of our members, but that, during the period of my presidency, it has been the medium of bringing some interesting papers before the profession, and has afforded to its members many valuable opportunities of seeing interesting specimens of morbid anatomy, the surest and best foundation for a correct knowledge of disease, and of sound indications for practice. I attach great importance to this part of our proceedings. It is true that, as these exhibitions of pathological specimens do not take the form of regular communications or dissertations, they do not bulk largely in the eyes of those who know the proceedings of the Society only by its published reports; but I appeal to all those who have been attending our meetings, whether they do not, as a general rule, leave the hall with as much satisfaction when they have witnessed the exhibition of a number of morbid specimens, as they do after listening even to the best of the many interesting papers which have been before us. It would be vain to attempt to specify all the interesting subjects which have thus been brought under our eyes; it would, in fact, almost amount to an enumeration of the specimens exhibited; for what morbid specimen can fail to be of some interest in a meeting of practical men? I cannot, however, refrain from noting, not as the only specimens of importance, but as samples of what have been exhibited, the instances of amyloid degeneration brought before us by Dr Grainger Stewart; an important specimen of disease of the tricuspid valves, shown by Dr Haldane; Mr Spence's case of dissecting aneurism of the iliac artery, shown in illustration of one of his papers; Dr Watson's case of sanguineous apoplexy in a young child; and a host of morbid growths, the produce of operations by various surgical brethren. I would venture before quitting this subject to remark that it is not at all requisite that a morbid product should be rare or novel. Those of the Society who are not connected with the Hospitals have comparatively few opportunities of refreshing their minds with the inspection of pathological specimens; and all of you will, I am sure, cordially welcome any good example, even of a common form of disease, perhaps none the less that it is more likely to be of practical use to you than the rarer instances can be. I hope, therefore, that this department of the Society's work will continue to be diligently cultivated and cordially supported, and that specimens will not be kept back merely because they are not curious from novelty or rarity.

The great bulk of our recorded proceedings, however, have consisted of the papers read before us, and of the discussions to which they have from time to time given rise. These naturally divide themselves into the departments of Medicine, Psychological Medicine, Surgery, Obstetrics, and a department which I shall venture to call Miscellaneous, and to which I refer papers not included under any of the above divisions. I do not propose to enumerate, much less comment on, all the papers which have been read before us; but I would wish to recall to your recollection one or two of them, and to offer some imperfect observations which they have suggested to my mind. I protest, however, *in limine*, that I do not make this selection upon any principle of superior merit in the papers which I specially mention. I need hardly say that nothing would be more unbecoming in a President than to make invidious distinctions among those of his fellow-members who have contributed to the Society's transactions during his incumbency. I cannot deal with all the papers which have been read. I wish I could; but, fortunately for your patience, my own time has not permitted me to make so flagrant an attempt at putting it to the proof. I am constrained, therefore, to confine my remarks to one or two, and I would have the authors of them to understand that it is the interest of the subjects, and not the special merits of the papers, which have led to this selection.

In the department of Medicine, one of the earliest papers which I had the pleasure of listening to in the chair, was the account of tubercular leprosy, in Madagascar, by Dr Andrew Davidson. There was, in truth, a double interest attached to this paper,—first, on account of the subject, which was one in which none of us had any practical experience; and, secondly, on account of

the author being one of the gentlemen educated at Edinburgh under the auspices of the Medical Missionary Society. The very fact of Dr Davidson communicating such a paper to us, furnishes evidence, if such were needed, that the character of the men who labour in the field of medical missions, often singularly misunderstood in many quarters, is not that of missionaries who have added the study of medicine to that of theology, but that of duly qualified medical practitioners, who, with zeal and ability for treating the ills that flesh is heir to, combine a zeal and ability for conveying to others a knowledge of gospel truth.

You will probably remember that, in that paper, Dr Davidson describes the disease under its three stages,—of, 1st, the appearance of spots on the skin; 2d, the formation of tuberosities; and, 3d, the occurrence of ulceration, with falling off of portions of the limbs, such as fingers and toes. These various stages were described at length, and we had set before us, shortly but graphically, the full portrait of the confirmed case, with that loathsome combination of its tuberculated face, its foetal exhalations, its ulcerated limbs, with the fingers dropping off, and its fatal termination by exhaustion of the digestive, or obstruction of the respiratory function. The point to which Dr Davidson specially addressed himself was the hereditary nature of the disease; he laid before us the facts observed at the Dispensary of Antananarivo, on which his opinions were founded, and he gave us his conclusions, “that leprosy seems to be a disease *sui generis*, distinct altogether from syphilis and yaws, propagating itself by hereditary transmission, and possibly by inoculation. It is probably seldom produced immediately and at once in any constitution from the causes originating it, but gradually, by the persistence of the causes operating through successive generations.” Lastly, on the subject of treatment, Dr Davidson only confirms what is, I fear, the experience of all who have encountered this hideous malady, that medicine may palliate, but cannot cure the disease.

I may be permitted to observe that this is not the only paper on leprosy which, during late years, this Society has been the medium of communicating to the profession, a very excellent memoir on it as it appears in Jamaica, having been read before us in March 1857, by Dr Fiddes, of Kingston. Dr Fiddes, like Erasmus Wilson and the Swedish Commission on the leprosy, makes more of the distinctions into the tubercular and anæsthetic varieties than Dr Davidson is inclined to do, but in many respects the conclusions come to by the two authors are accordant.

The Society will not have forgotten the interest with which it listened to Dr Joseph Bell's narrative of a case of paralysis after diphtheria,—a subject at once important practically and obscure pathologically. In a collection of similar cases, given by Dr Cummins, of Cork, in the November number of the Dublin Quarterly Journal, there is one which resembles that of Dr Bell, in respect that sufferer and narrator are one and the same. We congratulate both writers on their recovery from so serious a malady as diphtheria with paralysis.

We have been indebted to Dr Haldane, besides many important pathological specimens, for two papers of interest. In one of these, read in February last, he laid before us five cases of pneumonia, two of which were fatal, one by gangrene of the lung, the other by a combination of asphyxia and asthenia. The two fatal were exceptionally unfavourable; one being in a man of debilitated constitution, the other having been neglected for eighteen days, and being in a state bordering on hopelessness when admitted. Dr Haldane took occasion to hang upon these cases a remark as to the illustration which, so far as they go, they afford, of the danger of drawing from limited statistics conclusions as to methods of treatment. There certainly would be a vast difference in the value of any conclusions which we might draw from the statistics of five cases and those of the 3200 which Dr Barclay tells us we ought to have in order to guard us against medical errors. The value of the numerical method in practical medicine is a great problem, and one upon which, as the phrase is, much may be said on both sides. I confess myself inclined to agree with those who

hold that no amount of statistics can entitle us to draw from them absolute rules of practice. To requote from Claude Bernard the words of a mathematician quoted by him,—“La loi des grands nombres est toujours vraie en général et fausse en particulier.” Besides the differences in extent of the morbid action, strength and force of circulation, and other varying factors which possibly we might be enabled to set down with some approximation to accuracy, there are the never-ending variations of that indefinite something which we call power of constitution, which involves tolerance of disease and tolerance of remedies,—which never can be expressed in any statistical data,—and which, perhaps, above all other circumstances, influence the proportionate mortality in any given number of cases.

It is possible that some of you may agree with me in thinking that, considering the sources of error which surround it, we have had enough of the statistics of pneumonia. Pneumonia has had some severe galloping in its capacity of *chêval de bataille* in the numerous scientific combats as to the value of statistics and the change of type in disease. I own I shall not be sorry if I hear that this aged horse has been withdrawn for a while from this active service, has been allowed to cool, and been turned out to grass. I fear, however, that there is no such quiet time in store for the old charger; and all I can say is, that I hope whoever mounts him again will have him well in hand, and not allow himself to be run away with.

Dr Haldane's other paper was on uræmic convulsions, in which he supported the views of those who dissent from Frerichs' doctrine that uræmic phenomena are due to carbonate of ammonia, formed from urea in the blood. I believe we may hold this theory as being now essentially abandoned. The most recent views on the subject seem to be, that uræmia is not due to urea at all,—that it is the accumulation in the blood of creatine and other extractive matters which causes the symptoms,—that urea and uric acid are formed in the kidneys,—and that any traces of them found in the blood are due to resorption from the urinary apparatus. It may be that, in its turn, this view of uræmia being due to creatine, etc., has been given up; if so, my reading has not carried me so far. Certainly the whole subject of uræmia is one which admits of a great deal of elucidation.

None of those who were present on the occasion will have forgotten the interesting paper by Dr Sanders on a rare form of nervous disease, for which he proposes the name of *Dystaxia agitans*,—and which he illustrated to us by the exhibition of the patient himself. The muscular disorder in this case was the consequence of two falls which the patient had sustained. He was quite quiet when at rest or during sleep, but when he was moved or agitated, he was the subject of violent oscillating movements of all the muscles of the body, except those of the face. There was no paralysis of voluntary muscular motion, no loss of muscular power, no impairment of the sensory function. In his very able commentary upon the case, Dr Sanders pointed out the particulars in which it differed from ordinary paralysis agitans and chorea, and expressed a confident opinion that the affection was functional only. It will interest the Society to know that this opinion was well founded, the man being a good deal better now. It appeared, I presume, to most of you that Dr Sanders was right in holding that he had described a new form of disease. My attention has been directed to the fact, of which Dr Sanders is aware, that Mason Good had described a somewhat similar case, under the specific nosological designation of *synchonus tremor*. It is true, that in Good's case the muscular affection was not so universal, as the patient could carry, even in the midst of one of her violent shaking fits, a cup of tea to her mouth; but this seems to have been after, by force of habit, she had acquired some control over the irregularly-acting muscles. I do not know the exact present condition of Dr Sanders's patient, but I am sure that the Society will be anxious at some fitting time to hear from him some further account of this very interesting case.

In the department of Psychological Medicine, we had two able communications from Dr Francis Skae on climacteric insanity, as observed in 200

cases in the female, and 60 cases in the male sex; and it is satisfactory to learn from him that the general result of these cases is good. We have also had a paper of much value, from Dr Tuke, on puerperal insanity; cases of which he showed ought to be classified under three heads, according as they occur during pregnancy, at or about the period of delivery, and during lactation. The facts laid before us consisted of a series of 155 well-recorded cases which have occurred during the last eighteen years in the Royal Asylum. It is highly satisfactory to get from the experience of that valuable institution, on the authority of Dr Tuke, a confirmation of the generally-entertained belief that, under proper and prompt management, the treatment of puerperal insanity is successful in a large proportion of cases.

I must, however, pass from the department of Medicine to that of Surgery, in which we had many interesting communications.

I should like to make some observations on several of those papers, but time does not permit. Besides some important individual cases, pleasant to hear as records of successful surgery, I would call to your remembrance, as of much practical value, the paper of Mr Annandale on cleft palate, and that of Professor Spence on tracheotomy in diphtheria and croup, which commended itself to me as one of the most important practical memoirs that has been read before the Society for some time, and as one the interest of which was not in any way confined to those who are *ex professo* operating surgeons.

It is now so rare to find cases of pure inflammatory croup uncomplicated with diphtheria, that the paper of Mr Spence is possessed of even greater interest than that which he published in 1860, giving the results of his experience of the operation in cases of pure croup. The favourable opinion which Mr Spence expressed in his first paper has been corroborated by his subsequent experience; for at the time when he read the second paper, he had operated on 54 cases of croup and diphtheritic croup, with the result of 19 recoveries, or 1 in 3 cases. Of these 54 cases, 18 were examples of true diphtheritic croup, of which 7 recovered. Under reserve of due caution in reasoning from a few cases, it is curious to observe that the mortality in the cases of diphtheritic croup was actually less than in the total number of cases of croup and diphtheria. The cases operated on by Mr Spence were not slight, but, on the contrary, in every case the operation was had recourse to as a last resort, to obviate the immediate urgency of suffocative laryngeal symptoms. Besides affording the encouragement which these facts give, Mr Spence's paper was of importance as setting before us the conclusions to which experience had led him, of which the following are the chief:—The cases of diphtheritic croup which may require operative interference are divisible into two groups, according as the laryngeal symptoms or the constitutional phenomena predominate. Whilst the cases of the first set are best suited for tracheotomy, those of the second set are so ill suited for it that the surgeon should rarely perform it, except with the hope of relieving suffering, and with the knowledge that the chance of recovery is extremely small; cases in which the diphtheritic affection has supervened on scarlatina, measles, or gastric fever, or where there is marked submaxillary swelling, are so unfavourable that Mr Spence had not met with any success where these conditions existed.¹ Another important point in this paper was, the pointing out that cases where there is paralytic dysphagia are unfavourable, from the food being apt to pass into the larynx, and from the difficulty that exists in nourishing the patient after the operation. Mr Spence did not advocate the performance of tracheotomy at any early stage of diphtheria. It is satisfactory to find, as the result of Mr Spence's ample experience, that when undertaken even under very adverse circumstances, it affords considerable chances of recovery.

Among the individual surgical cases presented to us, the rarest was the successful ligature of the common iliac by Dr Mackinlay, of Paisley; and we had

¹ Since Mr Spence's paper was published he has had more than one successful case of tracheotomy even in such cases, and has modified his unfavourable opinion of the operation under these circumstances, which he at that time expressed.—*Ed. Edin. Med. Jour.*

also an interesting case of gunshot wound, and one of removal of a portion of the humerus by Dr Brotherston, of Alloa. We gladly receive these communications from our provincial brethren, as evidence that successful operative surgery is not confined to our great centres of population or seats of hospital schools.

The great cause of Needle *v.* Ligature, which had been left as a "ganging plea" at the time of the election of my predecessor, has been since I occupied this chair more than once before this court for debate, and has been ably and temperately pleaded by the advocates of the respective parties. It must, as in all practical questions, remain for the jury of the profession to return a verdict on the merits when the evidence has been placed fully before them, but this, I humbly submit, has not been quite done. I am not entitled to express any opinion upon the subject, because it does not lie in my line to deal with it practically. It is undeniable that acupressure in such surgical wounds as those of amputation and excision of tumours is capable of completely arresting hæmorrhage, but I am bound to say that the impression on my mind from what I have read is, that the needle has not yet made out a sufficiently strong case against the ligature to entitle it to an exclusive—or, if you will, occlusive—patent right to stop bleeding; and this would certainly require to be fully done, when we remember how many millions of bleeding vessels of all sizes have been successfully treated by deligation. What we most want yet to know, is, whether, under acupressure, the frequency of union by first intention in surgical wounds,—or rather, putting aside the question as to whether or not a little pus may be formed, whether the time required to form good stumps and firm cicatrices, is such as to warrant the conclusion that the ligature is so much the impediment to this as its adversaries maintain. I think, however, whatever be the ultimate conclusion that is come to as to the best way of securing arteries, that we shall take a very limited view of the question if we regard it merely as a contest between a string on the one hand, and a needle and wire on the other, as the mechanical means of stopping vessels. It involves a great principle whose bearings extend beyond that of mere operations with the knife. The larger question is, to what extent, and under what variety of circumstances, may the temporary obstruction of a vessel, without any interference with its internal coats, lead to its permanent obliteration. When we look at this, as illustrated by acupressure, in connexion with modern doctrines and practices as to the cure of aneurisms, and what we know of thrombosis, we must see that, to put it no higher than a physiological experiment, acupressure may have important and as yet unknown relations, not merely to surgical practice, but to questions of internal pathology.

In the Obstetrical Department, besides Dr Matthews Duncan's curious statistical-physiological papers on the relation of the size and weight of the child to the age of the mother, and on the production of twins, we have again had before us ovariectomy, which is fairly entitled to be called one of the victories of modern British surgery. Dr Simpson communicated to us one of his successful cases, and we had a continuation of the reports of his practice in this operation by Dr Thomas Keith. Dr Keith's cases up to the present time are thirty-nine in number (besides one operated on the day before yesterday); of these nine have died and twenty-eight recovered. The last eight cases in succession have done well; of the last twenty-one, three only have died, and all the recovered cases are now in perfect health. Such results are full of promise and encouragement, and are not the less interesting to this Society that through it most of the cases have been communicated to the profession.

In the department which I have called Miscellaneous, we had four papers.—Dr Andrew Wood's on medical education; Dr Craig MacLagan's on arsenic eating in Styria; Mr Turner's on malformation of the organs of generation; and Dr J. Smith's on certain positions of the head as a cause of syncope.

It remains for me now to call to your remembrance those members whom the Society has lost by death during the period of my presidency. These

were Dr Newbigging, Professor Miller, Dr David MacLagan, and Dr John Gillespie.

Of all these gentlemen there exist in our Medical Journal, and there appeared in the newspapers of the day, biographical notices so suitable and complete, and, moreover, they were so well known to the members of this Society, that it would be an unwarrantable intrusion on my part to attempt now to repeat at length what has already been done by pens more able than mine. In refraining from this I deny myself a personal gratification; I should have liked fully to express my pleasant recollections of them all.

In the case of my friend and next-door neighbour, Dr Newbigging, I should recall to your remembrance his love for the profession of medicine, and the high sense which he entertained of the honourable nature of the office of a medical man. I should speak of the zeal with which he practised it, and the courage with which he bore its fatigues, even under the painful disease which carried him off. I should note, as his personal peculiarity, his reticence on the subject of his illness, the nature of which was unknown to his many friends, save to him who was the friend of his life, his medical adviser and his biographer. I should mention how much his friendship was valued by those who had been privileged to make his acquaintance during temporary residence here, for there was no one not connected with the active teaching in our medical school to whom the Edinburgh men, when one met them at a distance, more frequently sent kind remembrances than to Patrick Newbigging. I should state how successfully, yet unostentatiously, he discharged the duties of President of the Royal College of Surgeons; and I think that I should not be far wrong if I summed up all by saying that his success, professionally and officially, and the esteem in which he was held personally, were all connected with this, that throughout life he consistently maintained the character of a well-bred, upright, courteous, and Christian gentleman.

Dr Newbigging was born in November 1813, and died on 16th January 1864. He was more than once a member of the council of this Society, but had not occupied the presidential chair.

Of Professor Miller, I should have to speak to you as of one who held a high place in the teaching, as well as practising, section of our profession in Edinburgh. I should remind you of his handsome person, his fine voice, his copious command of language, and his play of fancy and humour, all of which contributed to make him pre-eminently successful as a lecturer and public speaker. I should have to mention the success of his writings and the extent of his professional practice, and his manual dexterity as an operator conjoined with that free use of constitutional treatment which made much of his surgery truly conservative. Then turning from the professional view, I should regard him as a man and a member of society, and should find a ready theme for eulogium in his earnest and uncompromising advocacy of all that he believed to be conducive to the social, moral, and religious well-being of his fellow-men,—advocacy which rendered him a man of mark, and commanded for him the respect and esteem of others, even though they did not share his opinions, and did not appear on the same platforms, or sit in the same ecclesiastical assembly with him. I should sum up by saying that he has left behind him the pleasant memory of a genial companion, a useful citizen, and an earnest, God-fearing man, who carried his religion in his heart, as well as in his outward life and conversation.

Mr Miller was born in April 1812, and died on 17th June 1864. He was one of my predecessors in this chair, and discharged the duty that I now perform by giving his valedictory address on 15th December 1858.

Of Dr MacLagan, I should have to speak as one whose earlier professional lot was cast in more stirring times than those of the two friends whom I have already noticed. I should have to mention his services as an army medical officer in the pestilential swamps of Walcheren and the bloody fields of the Peninsula, and his subsequent career in civil life, where he gained the esteem, not only of his patients but of his professional brethren, as was evidenced by

his having the rare honour of being President both of the College of Surgeons and of the College of Physicians of his native city. I should have to speak of his warm interest in every scheme devised to do good to his countrymen, and of the earnestness with which he entered on the field of political and municipal action, without, however, ever alienating a friend or saying an ungenerous word of an opponent. But whilst my words would necessarily refer to the wider area of public service, professional work, and duties of citizenship, my thoughts would concentrate themselves within the narrower precincts of domestic life, where we, who were his own, could best know, and, knowing, could appreciate, the purity of his character, the simple genuineness of his piety, the unvarying sweetness of his temper, and the warmth of his heart. I am persuaded that you would, with sympathizing kindness, follow me into this inner circle were I to open it up to you; but this must not be. This is not the time or place to offer a full oblation on the altar of filial regard. I may, however, perhaps be pardoned if, speaking as I now do to a professional audience of him for the first time since his death, I should say, as I do in all sincerity, that whatever measure of success I have met with in life, I owe it, under God, to his precept, which was good, and to his example, which was better.

At hoc nunc
 Illi laus debetur, et a me gratia major
 Nil me poeniteat sanum patris hujus.

Dr MacLagan was born in February 1785 and died on 6th June 1865. He also was one of my predecessors in the chair, having been President of the Society from 1840 to 1842.

Dr John Gillespie, after assisting his uncle, Mr Marshall, during the period of his medical studies, obtained the diploma of the College of Surgeons, and the degree of M.D. at St Andrews, in 1845. He was soon after this appointed House-Surgeon to the Leith Hospital, which post he filled with credit and honour for nearly two years. He then entered upon private practice, was soon appointed parochial surgeon for North Leith, and continued in this office till his death. His services, both to the Parochial Board and the community of the district, as well as among his professional brethren, were highly valued. He was a general favourite, and he was a changeless friend. Dr Gillespie became a Fellow of the Royal College of Surgeons in 1864. He was a member of the Medico-Chirurgical and Obstetric Societies.

He was born in December 1822, and died of rheumatic affection of the heart in February 1865.

Before I quit the chair, I have, as a final duty, to reiterate the thanks which I verbally expressed to you two years ago for the honour you had done me in calling me to preside over you. There is nothing of which a professional man may more justly be proud, or for which he should be more grateful, than the receiving of any honour at the hands of his brother practitioners. Such thanks I now tender as being all the more called for that you have listened so patiently to this too lengthy address.

IV. ELECTION OF OFFICE-BEARERS.

The following gentlemen were elected office-bearers of the Society for the ensuing year:—*President*—John Moir, F.R.C.P. *Vice-Presidents*—J. W. Begbie, M.D., D. R. Haldane, M.D., J. D. Gillespie, M.D. *Councillors*—W. Sanders, M.D., James Cattie, M.D., T. Grainger Stewart, M.D., J. Matthews Duncan, M.D., Arthur Mitchell, M.D., Robert P. Ritchie, M.D., D. Argyll Robertson, M.D., W. Stephenson, M.D. *Treasurer*—G. W. Balfour, M.D. *Secretaries*—Patrick H. Watson, M.D., Charles Dyce, M.D.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXV.—MEETING I.

15th November 1865.—Dr PATTISON, *Vice-President*, in the Chair.

I. ON THE INFLUENCE OF CHLORATE OF POTASH ON THE FŒTUS.

Dr Bruce read the following paper:—It is within the experience of every one engaged in midwifery practice that some women are unable to carry their offspring alive to the full time,—that they again and again bring forth still-born children, often before the termination of the natural period of intra-uterine life. The cause of this is not unfrequently owing to some imperfection in the placenta, from fatty degeneration or otherwise, or it may be from some taint in the mother's system, which allows of foetal life being maintained only for a limited period.

It has been thought that, in some of these cases, by means of certain methods of treatment we might be enabled to prolong the existence of the fœtus, if not to the full time, at least so long as to bring it within the viable period, and then by the induction of premature labour, if necessary, save the child, which would otherwise have perished. One of the medicines recommended with this view is the chlorate of potash, on account of the quantity of oxygen which it contains; and my object is to relate a few cases where it was employed for the purpose.

CASE I.—Mrs A. came under my care towards the end of 1863, with the following history:—She had been pregnant three times previously, each of the deliveries being premature, and the children still-born. The first was a sixth month child, said to have been dead three weeks; the second, an eighth month child, also three weeks dead; and the third, an eighth month child, said to have been eight days dead. For a month previous to her last delivery, she had been taking some powders prescribed for her by her medical attendant. I saw her in her fourth pregnancy, when about six and a-half months gone, and at once put her upon the chlorate of potash, in one drachm doses daily, and kept her closely under observation, intending to bring on premature labour, should this be thought necessary to preserve the life of the fœtus; however it was not required, as auscultation, frequently practised, always revealed sufficient activity in its circulation, and delivery took place at the full time, on 6th March 1864, the child being strong and healthy in appearance.

CASE II.—Mrs G. consulted me about the same time as the last case, and gave me the following particulars. She had been pregnant five times previously, and never had a living child except the first; the other four all died about the fifth month. On January 4, 1864, I gave her the chlorate of potash, one drachm daily, to be continued regularly, and directed her to let me know if she felt any symptoms of stirrage becoming weaker. She continued to feel movements distinctly until the eighth month at least, but did not send for me until 28th March,—four or five days after all symptoms of life in the fœtus had ceased, of course just a few days too late.

I satisfied myself that the child was dead, and found that pains had begun already, and that labour was apparently about to commence; the pains were lingering and inefficient, and I delivered by means of the forceps. The child had evidently been dead for some days, in all likelihood about the time she ceased to feel stirrage.

On delivery, it was found that the abdomen was much enlarged from ascites, and that the child would probably not have lived at any rate; so there was the less regret that its mother neglected to send for me sooner. There remained the fact, however, that the child had lived until the eighth month, instead of the fifth only, as in her last four cases, and that, had she let me know in time, labour might have been induced at a period in which the child would have been viable. In this case the placenta was large, and of a very fatty nature.

CASE III.—Mrs J. placed herself under my care in January 1864. History:

She is now pregnant for the fourth time, and is about six months advanced. She has had three children before; the first at six and a-half months, the second at five months, and the third at six and a-half months, all still-born, except one, which lived about eight hours.

I at once put her upon the chlorate of potash, and kept her under observation during the remainder of her pregnancy, visiting her from time to time, and desiring her to let me know if at any time she should notice the movements of the child becoming weaker. She was extremely apprehensive from having been so unfortunate before, and sent for me several times, being afraid that the child was not lively enough; but auscultation always satisfied me that it was unnecessary to interfere further than by sometimes increasing the quantity of the chlorate. Labour came on naturally on the 2d of April, 265 days after the cessation of her last menstruation, and therefore a little prematurely. She was delivered of twins, the first alive, and of tolerable size; the pains being inefficient afterwards, I had to deliver the second child by means of the forceps. It was, however, still-born. The first child only survived about three days.

CASE IV.—Mrs R. consulted me about the beginning of February last, and gave the following particulars:—She is now pregnant for the fourth time, and has never gone the full period in any of her former pregnancies. The first child was said to be fully eight months, and hydrocephalic; the second, between seven and eight months; the third at the sixth month, about a week dead; the last two were said to have been slightly hydrocephalic. None of these children were born alive. This was perhaps not a very promising case, from the cause of the still-births being probably on the part of the child; however I put the patient upon the chlorate of potash, and desired her to let me know when she felt any diminution in the movements of the fœtus. She called upon me on February 16th to say that she had felt no stirrage all day, and very little the day before. On examining stethoscopically, the sounds of the fetal heart appeared to be weak, and I advised the induction of premature labour, as she was now about seven and a-half months gone. She consented, and on examination I found the os uteri open, and the bag of membranes within reach and much distended, although she had had no pains previously. I at once ruptured the membranes, and a large quantity of liquor amnii escaped, but the pains did not begin until the next day, thirty-four hours after rupturing the membranes. The labour was short, and, on delivery, the child gasped a little, and its heart was beating slowly; the abdomen was much enlarged, and felt very tight. By persevering for about half-an hour in the usual methods for re-suscitating, the child began to breathe of its own accord, but laboriously, and it only lived about four hours afterwards.

The cause of the enlargement of the abdomen was found to be the extreme size of the liver. This was the first time that Mrs R. had the gratification of bearing a living child, though it did not long survive.

CASE V.—Mrs J., the same patient as in the third case, now pregnant for the fifth time. I again recommended the chlorate of potash in her case, and watched her progress until the full time, when she was delivered of a large and healthy-looking child on the 3d of June last.

CASE VI.—Mrs C. History: She had been pregnant four times previously, but never had a living child at the full time. Her first child was an eighth month, which lived an hour or two; her second an eighth month also, which lived eight days; the third, 7½ months, still-born; the fourth, 7½ months, also still-born. I attended only in the last of these confinements, and found the placenta to be fatty. The chlorate had been given, but only for a short time. There were some peculiarities about the child. It was very narrow at the shoulders, the lower part of the chest projected very much outwards, and the under jaw was fractured.

In this, her fifth pregnancy, I gave the chlorate of potash from an early period, and watched her carefully, giving also particular directions about sending for me should she find the movements of the child to become weaker. She sent for me on several occasions, but I always found the fetal circulation

pretty good, although sometimes not so strong as at others, and never considered it necessary to interfere further than to increase the chlorate from a drachm to 2 drachms daily. She carried this child to the full time, and was delivered on the 11th August last of a strong and healthy-looking boy, to her very great satisfaction, after so many disappointments.

The foregoing are all the cases I can refer to at present, and they are much too few to draw any satisfactory conclusions from; but I think they are interesting, in so far as some of them, under the use of the chlorate of potash, ended so much more satisfactorily than they had ever done before, where some had become mothers of healthy children, which a succession of previous miscarriages had well-nigh made them despair of. How far these cases may have been influenced by the remedy employed I will not venture to say, but the result of the practice appears to me to be of an encouraging nature; and in any other cases of the kind which may come in my way, I shall feel strongly inclined to follow out a similar treatment.

Dr Pattison mentioned a case where a mother had had nine premature or still-born children, but the chlorate of potash had not been used. As the case was interesting, he might mention the particulars of her confinements. *1st*, In September 1853, she was delivered of a girl at seven months, which lived two weeks; *2d*, In July 1854, she had a premature dead child, when she was attended by *Dr Pattison*; *3d*, In August 1855, she had a premature dead child,—*Dr R.*; *4th*, In November 1856, she had a male child at seven and a-half months, which lived two hours,—was attended by a midwife; *5th*, In March 1857, she was delivered naturally of a male child at full time, still-born,—*Dr S.*; *6th*, In March 1858, she was delivered instrumentally of a female child at full time, still-born,—*Drs S. and S.*; *7th*, In August 1861, she had a female child, delivered by version at full time, still alive,—*Drs R. and K.*; *8th*, In October 1863, she had a miscarriage,—*Dr M.*; *9th*, In October 1865, she had a female child at full time, delivered by version, living,—*Drs P. and R.*

Dr And. Inglis said he had never seen the chlorate of potash fail. Even in one case, where, after sixteen still-born children, the seventeenth was born alive.

Dr Sidey thought it was always useful.

Dr Cairns mentioned a case where a woman had aborted five times, but after the use of this medicine she carried her sixth child to the eighth month.

Dr Moir stated that he had used chlorate of potash in very many cases, and thought it did good.

Dr Young preferred the iodide of mercury.

Dr Keiller remarked that he had very frequently administered chlorate of potash, for the purpose of preserving the life of the child in utero until the period of its viability, if not to the full time, and his experience led him to continue the practice. It was a matter very difficult to determine whether any undoubted evidence existed to prove that the mother's blood was oxygenated by the use of the chlorate of potash to the extent of acting on the placental structures, or their circulating fluids, so as to influence in any degree the vitality of the child. It might be as he (*Dr K.*) had, on former occasions when this matter was before the Society, hinted at, that the health of the mother happened to be in better condition on the particular occasion when a living child was born, or that the patient's health was benefited by the alterative or tonic influence of the salt referred to, without the supposed directly oxygenating effect of the remedy on the blood of the child. The condition of the mother's health was not specially mentioned in the cases cited by *Dr Bruce*; but he (*Dr K.*) thought it important to take this into consideration while estimating the value to be attached to the use of the chlorate. *Dr Keiller* had often noticed that the health and spirits of females treated in this way improved very materially; this might be partly brought about by the taking of a remedy likely to promote the chance of ultimately getting a living child, for we all know the wonderfully tonic power of lively hope on the one hand, and the depressing influence of mental anxiety and dread on the other. The mere

giving of a something, and the encouraging the idea of its remedial power, often acted like homœopathic charms, the patient being induced to go more about, and thus, also, favour the chances of the general condition of her health being improved. Dr K. had at present a case in hand, the result of which he would afterwards communicate to the Society. The patient had lost seven children, and was now between six and seven months in her eighth pregnancy. She had, for the second time, come from Australia to undergo treatment likely to lead to the desired end of having a viable child. This patient was now taking the chlorate of potash in much larger doses than those spoken of by Dr Bruce. For some time past he (Dr K.) had been in the practice of administering the chlorate to the extent of several drachms daily in a very diluted form, the patient using it freely as a common drink.

It was very important, while watching the progress of cases, to beware of being misled by the *active movements* of the child to which Dr Bruce had referred, as if this amounted to unfailing evidence of its safety. It often happened that immediately preceding death the foetal movements became convulsive, or unusually strong, which, indeed, in many cases was a suspicious sign. This ought studiously to be kept in mind, for although gradual feebleness or loss of movement was usually the most noticeable sign of the child's dying, extreme or convulsive plunging frequently indicated the advent of death.

The condition, not only of the child, but of the uterine circulation, as ascertained by the use of the stethoscope, ought to be frequently seen to, and the general or constitutional evidence of foetal danger or death should in such cases be observed.

The frequency of syphilitic influence as a common cause of foetal death demanded our attention and remedial care. At the same time, the chlorate of potash, if it did not do away with the necessity of administering other or more special syphilitic remedies, did not interfere with their use in cases where constitutional taint from syphilis was found or supposed to exist.

Drs Young and Pattison spoke in favour of chlorate of potash in cases of malignant scarlet fever.

Dr Bryce said he had prescribed chlorate of potash in many cases where the placenta had been diseased, and with much benefit.

Dr Balfour spoke in favour of its use, and remarked that, however the chlorate of potash acted, it was not, as formerly supposed, by its parting with any of its oxygen, for that it was eliminated unchanged. He was inclined to think that it so altered the saline constituents of the blood as to enable that fluid to take up more oxygen, in a similar way that sea-water suspended more oxygen than common water.

Dr Cochrane agreed with Dr Young in his preference for some mild alternative.

Dr Zeigler closed the discussion with some remarks as to the necessity of finding out the real pathological condition of what was called a diseased placenta, before ascribing any virtue to any particular form of treatment.

II. THE USE OF THE WIRE-RIBBON IN SOME CASES OF DIFFICULT TURNING.

BY DR VAL. HEYERDAHL, BERGEN.

Dr David Simpson read the following paper:—The application of a running noose of tape around the ankle of the child, in order to facilitate version, is easily made at the outlet of the pelvis, without any special contrivance for its performance. But when the feet are in the brim or pelvic cavity, this little manoeuvre may be attended with great difficulty, and must sometimes be dispensed with altogether (as impossible).

To remedy this difficulty, several more or less useful instruments have been constructed, and in some text-books of midwifery students are recommended always to carry a "proe-noud" in their obstetrical armamentarium. I will only cite the names of some inventors, Pugh Neverman, Gerner, Braun, Tre-

furt van Huevel, Wasseige, etc.,¹ to show that their number is not so limited. Of modern writers upon the subject I will mention a most valuable treatise by Dr Hyernaux, of Brussels.² This distinguished accoucheur points out most impressively that, in some cases of difficult turning, an instrument to apply the tape is indispensable; at least, he has succeeded by its aid where others have completely failed in accomplishing version. He pretends that he, by his instrument for applying a running noose of tape high up in the vagina, has been able to turn in cases where embryotomy apparently was the only method practicable. Dr Hyernaux had the kindness, during my stay in Brussels two years ago, to show me his instrument and explain the manner of using it. It is a staff of German silver, with a horse-shoe shaped bi-valvular tube, with open bevelled extremities fixed to its top. At the handle is a spring, by means of which the horse-shoe shaped tube can be opened into two semi-circular grooved valves. A loop of the running noose of tape is put into, and fastened in, the bi-valvular tube. The instrument is then introduced, with the horse-shoe along the arm and hand of the accoucheur, up to the foot, which is then slipped into the circle formed by the tube and the free part of the loop, between the two extremities of the horse-shoe. By pressing on the spring the tube readily opens, and lets the loop of the running noose free, surrounding the child's ankle. It is then tightened by pulling on the free ends of the noose.

I found the instrument rather ingenious, and easily applicable, and, as far as I could judge without putting it to test, the very best of the many invented for the same purpose, by other accoucheurs, from the time of Smellie and Siegemundin up to the present day.

I agreed with Dr Hyernaux in his views. I had myself sometimes found it exceedingly difficult to get a running noose of tape up to the foot, and in such an emergency much missed the aid of a proper instrument. I therefore procured one of Hyernaux's, with the view of trying it on the first opportunity.

But at the very first case of difficult version I met with on my return to Norway, I had forgotten to carry the instrument along with me, which necessitated the employment of a substitute for it, which I am now going to describe.

It was a case of transverse presentation, in the country, some miles from Bergen, in which the diagnosis had not been made out, and the labour, therefore, had gone on a considerable time unrelieved,—the waters having flowed away about two days before my arrival. The pains had been rather energetic for the last twelve hours, with strong bearing down. I found the left shoulder presenting low down in the vagina, and considerably swollen; the uterus and os firmly embracing the fetus. The foetal heart was heard indistinctly about the umbilicus. I put the woman—a strong primipara at the term of gestation—under the influence of chloroform, and proceeded to turn. It was impossible to push the bulk of my hand into the uterine cavity, although the bearing down pains were very much lessened; but with two fingers within the os I reached and got hold of one foot, aiding this purchase by forcing the pelvic extremity of the fetus lower down with the other hand. So far I succeeded; but I soon found that I wanted strength enough in my finger to bring the foot farther down than to the brim, the foot being in such a degree slippery that I lost my purchase every time I tried to pull down. At the same time it was impossible to raise the presenting shoulder up to get a little more room. I tried in vain to bring up a tape round the ankle. I failed repeatedly on account of the looseness of the tape, moistened in the discharges from the vagina, and I regretted very much that I had not the instrument of Hyernaux at hand, while I was convinced of its usefulness and importance in this case.

It then struck me to try another material for a running noose, with more stiffness and not so liable to slacken in moisture as common tape. I found amongst a large stock of ribbons, tapes of different kinds, etc., a peculiar sort

¹ Vide Professor Kilian, of Bonn,—his Atlas of Obstetrical Instruments.

² Du Baes et d'un nouveau Port-nœud sur le Pied de l'Enfant dans la Terminaison des Accouchements laborieux.

of tape with thin wires interwoven, commonly used by milliners to give bonnets their different shape and fashion, called wire-ribbon.

I took about a yard of wire-ribbon, half-an-inch broad, doubled it up, and formed it into a running noose. I made the loop a little elongated, and large enough to slip a foot through, and twisted the free ends a little to give them more stiffness. I now succeeded in sliding the well-oiled running noose of wire-ribbon up to the foot, and to get a firm hold of it, and then, by moderately pulling on the foot, securely grasped in this manner, and at the same time pushing the presenting shoulder upwards with the other hand introduced into the vagina, I was fortunate enough to turn in no time. Soon after, the child was born by nature alone, with the exception of a little manual force to extract the head. The child was dead; but the patient did well, according to the report given me two days thereafter.

This case is, I think, of some interest for the practitioner, because it proves that the difficulty of grasping the foot high up in the vagina by a running noose of tape—sometimes baffling our best endeavours—may be overcome without instrument in so simple a manner,—namely, only by substituting the material commonly used—tape—by another equally simple material—wire-ribbon, to be procured wherever bonnets are worn.

Of course the method of applying wire-ribbon may be modified in different ways; for instance, by doubling the wire-ribbon, then twisting it in its whole length, only the uppermost portion being left untwisted and forming a loop. To the other end is fixed a handle, made of a small piece of wood. This little apparatus is used in the same manner; the only difference is, that the loop is tightened by augmenting the number of twistings by turning the handle. The material possesses this quality, that the twistings, once made, do not untwist again, and thus the foot is securely and firmly grasped.

But I am inclined to think that the method put into execution in my case is perhaps the simplest, and, therefore, also the best.

Should the stem of the running noose give too much during its introduction, this inconvenience may be remedied by using a small surgical forceps to introduce it with.

There is one drawback to its use: wire-ribbon is sharper and more apt to cut than common tape, and I was obliged to protect my hand by means of a handkerchief. Around the ankle of the child was left a deep furrow, but it had not cut through the skin.

By applying the wire-ribbon running noose to the feet of the dead body of the child, I was not able to cut through, although I purposely used much more force, and for longer time, than advisable in any circumstance connected with the operation of turning a living child.

If instruments can be dispensed with, it may be a step in advance in the technosis of turning, and worthy the notice of the profession.

I take the liberty to recommend the use of wire-ribbon to the attention of the members of the Obstetrical Society, that it may be put to a fair trial.

Hints.—Wire-ribbon may be useful in dislocated finger; may substitute the fillet; may be formed into pessaries.

Dr Young stated that, in his experience, it was most difficult in many cases to get down the feet, and suggested the use of India-rubber sheaths on the first phalanx of the index and middle fingers, to prevent slipping.

Dr Pattison moved a vote of thanks to *Dr Hyerdahl* for his most interesting communication.

QUESTIONS AT THE SECOND EXAMINATION FOR THE DEGREE OF BACHELOR OF MEDICINE,—OXFORD, December 1865.

I. PATHOLOGY.

1. Describe the general appearance and the microscopical structure of a lung affected by pneumonia, discriminating between the several stages of the disease.

2. State the pathology of diabetes mellitus, and mention other forms of disease with which it is apt to be associated.

3. What are the varieties of softening which are found in the cerebro-spinal axis, and what are the most usual causes of the disease in the several parts of that structure?

4. Mention the best known varieties of pityriasis, and describe their microscopical characters.

5. Give a sketch of the natural history, and shortly describe the external appearance, of the entozoa found in the human abdominal viscera in this country.

6. State the chemical composition of biliary calculi.

7. What are the causes of the condition described as angina pectoris?

8. Give the differential diagnosis between syphilis in the infant, varicella, varioloid, and variola, as usually described.

II. THERAPEUTICS.

1. How would you treat a case of rheumatic fever and its most common complications?

2. What treatment would you ordinarily pursue in the delirium of typhus, and in the severe diarrhoea of typhoid fever?

3. When are topical applications to the pharynx, or larynx, desirable? How do you apply them, and with what object? What drugs are used for the purpose, and in what doses?

4. State the most usual modes of treating syphilis in its several stages or varieties.

5. What indications for treatment do you gather from microscopical examination of the urine?

6. Enumerate the counter-irritants contained in the British Pharmacopœia, and state under what circumstances you would have recourse to the use of each.

7. Describe the various modes in which water may be used externally as a therapeutical agent, and name the cases to which each is suited.

8. What are the doses of the Pharmacopœial preparations of arsenic, copper, opium, belladonna, strychnia, and cinchona?

III. DISEASES OF WOMEN AND CHILDREN, AND SURGERY.

1. Enumerate the sources of hæmorrhage from the organs of generation of the female, and state the mode of treatment in each case.

2. State the signs of pregnancy.

3. With what diseases may scarlet fever and croup be confounded, and how would you distinguish them from those diseases?

4. Describe muguet—its causes, nature, and treatment.

5. How would you perform the operation of tapping the abdomen and the thorax, and in what pathological states may it be expedient to resort to it?

6. What are the circumstances which determine the selection of lithotomy or lithotripsy in the treatment of stone?

7. What are the chances of the return of cancer after its removal from the breast.

IV. FORENSIC MEDICINE AND HYGIENE.

1. State the symptoms of poisoning by sulphuric acid, phosphorus, and strychnia.

2. What are the symptoms of poisoning by carbonic acid, under what circumstances does it usually occur, and how would you prevent them?

3. How would you ascertain whether the dead body of a child found in the water had ever breathed?

4. What amount of cubic space is required for each bed in a hospital ward? How do you ascertain the rate of change in the air in a ward? and state the ordinary impurities which may exist in vitiated hospital air and the methods in use for examining them.

5. What are the diseases which are or are supposed to be caused by impure water?

6. What are the best disinfectants for a sick room? How far are disinfectants of use? and state the principles on which those act which you enumerate.

V. CLINICAL EXAMINATION.

1. Examine ——— in ——— Ward, and ——— in ——— Ward, and report on their cases.

2. Describe minutely on paper the pathological state of ——— in ——— Ward; giving the history in full, and your opinion as to the probable effect of remedies in such a case.

3. Examine and write an account of the urine marked A and the fluid marked B in the Clinical Room.

4. What is the temperature of ———, and how far does it deviate from the normal state?

VI.

Passages from Celsus and Aretæus to be translated and criticized.

THE NEW MEAT-PRESERVING PROCESS.

IF the wholesale destruction of cattle by plague and pole-axe continues, we must look out for a supply of meat. This is in a great measure found in the importation of cattle and carcasses, and with the high prices now obtained a pretty liberal supply may be anticipated from these sources. But to this there is a limit. Live oxen or sheep can only be brought such distances as that the market price obtained for them will more than cover their prime cost, plus expenses of transport; while the trade in carcass meat will have to contend against the liability to putrescence increased by length of transit, conditions of atmosphere, close packing, etc. Whatever may be the true cause of the present high prices of butcher's meat, it is a question of great moment, in a sanitary point of view; for if the masses be ill-fed, they will fall victims easily to the first epidemic. We have been led into these reflections in consequence of having visited the factory of the Import and Export Fresh Provision Company, near Rainham, in Essex. The object of the Company is the preservation of fresh raw meat by a most ingenious and effective process, founded on principles in true accordance with Nature's laws, and obeying those laws in many of its details. The directors had invited a few scientific gentlemen to inspect the building, outhouses, etc., and view the means for carrying on all the minutiae of the special process. Among those invited we noticed W. Hawes, Esq., the Chairman, and P. Le Neve Foster, Esq., the Secretary of the Society of Arts; Drs Ed. Smith, Markham, and Seaton; H. Lee, etc. The first aim of the Company is to obtain the best meat; this is procured, not as is usually done—by purchasing at a meat market the carcasses of oxen, sheep, etc.—but they go to the live market, and there make their purchases. It will be evident, therefore, that any symptom of disease, rinderpest or other, will be more easily detected than when the mere carcass is bought. The cattle are then conveyed by rail—a siding being close adjoining—to the factory, which is situated on a farm of sixteen acres' pasturage, the property of the Company, where they remain for a time until they have recovered from the effects of driving, carriage, etc., and are then in the fittest condition for food. They are subsequently slaughtered in a capacious and well-appointed abattoir adjoining the main building. At a convenient time the carcass is cut up into the customary joints and conveyed into the factory, where each piece is weighed and put into tins before being submitted to the raw meat process. It

may be premised that only the best joints—such as legs, loins, shoulders, ribs of mutton, etc., sirloins, ribs, rumps of beef—are thus treated, the coarser or second rate pieces are subjected to the cooking process now so well known, and which is also carried on in the same building. It will be seen that the whole of the animal is thus either preserved or utilised—the skin, offal, and blood after slaughtering, the bones after the cooking process and subsequent digesting to obtain from them the large amount of nutriment they contain, with the superfluous fat, are all collected, and become a source of profit by sale to different manufacturers.

To resume, the raw meat in joints having been placed in tins prepared to receive them, and the lids securely soldered down, are conveyed to the upper story of the building, where is situated a large iron chest with a strong lid; the tins of meat, which have each a short metal tube at the bottom and another at the top, are placed in the chest, the bottom tube of each tin being fastened to an elastic tube, which by its other extremity is attached to a pipe running along the floor of the chest; the upper tube is similarly attached to elastic tubes in relation with a pipe extending along the upper portion of the chest. By supplies from a tank overhead, the chest and tins of meat are filled with water; the lid is then lowered and screwed down so as to be air-tight. At this stage the chest and tins are both completely filled with water, which is seen flowing from taps corresponding to the upper parts of each. The air they before contained has given place to the fluid, so that one object is gained,—viz., the abstraction of oxygen; if not wholly, still in a very great measure. All the taps are now closed, and the first stage of the process is complete. The iron chest has extending from its floor two perpendicular pipes, passing to a well in the basement, a distance of upwards of thirty-five feet. These are connected, one with the cavity of the chest, the other, by means of the pipe which ran along its floor, with the cavity of the tins. By means of taps, communication is opened between the descending tube and the chest on the one hand, and the other descending tube and the tins on the other, each series forming a water barometer. When it is ascertained that the tins have become emptied of their fluid contents, and a vacuum established in them, as ascertained by vacuum gauges, the taps are closed. Deutoxide of nitrogen is then supplied carefully and simultaneously to the tins and chest. This will have the effect of neutralizing any remaining oxygen in the meat or tins. Both cases and tins are again refilled with water, the deutoxide is driven off into a spare gasholder, a vacuum is again created in the tins, and they are then supplied with a mixture of nitrogen gas, with a small proportion of deutoxide. This may be said to complete the second stage. The lid is now lifted; each tin is carefully raised, in order that its lower tube may be secured by first pinching it close and then soldering. This being done, a quantity of sulphurous acid gas, proportionate to the weight of meat, is forcibly injected, and the remaining tube is closed and soldered. Each tin is subsequently tested, at a temperature of about 90°, for several days. They are then ready to be japanned, and are fit for use. The means used for preserving poultry and game is substantially the same.

It will be seen that this process, as we have said, depends on certain chemical and physical laws, which are applied to a most useful and beneficial end, for by this means it is proposed to import from cheap meat countries, Australia and South America for example, supplies of meat for the English market. Such a plan is in progress, an agency or local board having been established in Queensland, and it is hoped that soon we may be able to regale ourselves with beef and mutton grown and fattened under the Southern Cross. A hot luncheon was provided for the visitors, consisting of legs and shoulders of mutton, sirloin, fowls, and ham. It was amusing to hear the doubtful and wavering opinions of the guests as they discussed each dish, as to which was preserved and which fresh meat. It certainly was a difficult matter for even a daintily-fed Englishman to distinguish, but when it was declared that everything placed on the table had been put up two or three months ago, all expressed their surprise. Mr Foster, however, capped all that could be said by narrating how he had

kept a tin of meat under his own care for quite eight months, through the late hot summer. It was opened the other day; he had the joint cooked at home, his family and visitors were helped to it more than once, and when told the length of time he had had it in his possession they could scarcely credit the statement. It turned out that the same joint had been put up about three months before it was sent to Mr Forster. We wish the Company every success in their undertaking. Tins of meat have been sent to the Admiralty, and we shall soon have some account of them. Several testimonials were laid before the guests, which one and all spoke highly of the results, the meat having been eaten in almost all latitudes. For her Majesty's and mercantile marine the compact way in which the meat is preserved must be of great moment, if only the matter of space is considered, when compared with the room occupied by living cattle and fodder, and water required for their sustenance, but of much greater account if considered as replacing by fresh provisions the salt food necessary for crew and passengers. It would thus seem that the Company will reap advantages both on an import and export trade. We shall wait for and be glad to hear of further results.—*Medical Times and Gazette*.

ROYAL COLLEGE OF PHYSICIANS OF EDINBURGH.

At the Annual Election Meeting of the Royal College of Physicians of Edinburgh, held on St Andrew's day, 30th November, the following office-bearers were elected for the ensuing year:—*President*—Dr John Smith. *Vice-President*—Dr John Graham Macdonald Burt. *Council*—Dr John Smith, Dr William Sellar, Dr John Moir, Dr Robert Bowes Malcolm, Dr Alexander Wood, Dr John Graham Macdonald Burt, Dr Robert Paterson. *Examiners*—The President, Drs Craigie, Wood, Douglas, Burt, R. Paterson, Wright, Keiller, Pattison, Cumming, Duncan, J. W. Begbie, Haldane, Sanders. *Treasurer*—Dr Samuel Somerville. *Secretary*—Dr Rutherford Haldane. *Librarian*—Dr Matthews Duncan. *Curator of Museum*—Dr William Henry Lowe. *Clerk*—Mr Christopher Douglas, W.S. *Auditor*—Mr Kenneth Mackenzie, C.A. *Under-Librarian*—Mr John Small. *Officer*—Thomas Marshall.

CONVERSAZIONE OF THE ROYAL COLLEGE OF SURGEONS.

On Thursday the 14th December, a Conversazione of the Royal College of Surgeons was held in their hall, Nicolson Street, when Dr Stevenson Macadam delivered a Lecture on "The Recent Progress of Chemistry," which we hope to publish in the next number of this Journal. The Lecture was illustrated by numerous interesting experiments. On the motion of the Lord Provost, seconded by Professor Christison, a vote of thanks was accorded to Dr Macadam.

PUBLICATIONS RECEIVED.

- Anatomical Remembrancer. Sixth Edition. London, 1866.
 Beasley,—Pocket Formulary and Synopsis of the British and Foreign Pharmacopœias. By Henry Beasley. Eighth Edition. London, 1865.
 Cooke,—Cancer: its Allies and Counterfeits. By Thomas Weeden Cooke. London, 1865.
 Cockle,—Intra-Thoracic Cancer. Part II. By John Cockle, M.D., etc. London, 1865.
 Fox,—Cholera Prospects. By Tilbury Fox, M.D. London, 1865.
 Gairdner,—Plea for an Extension and Alteration in the Curriculum of Arts in the University of Glasgow. By Prof. W. T. Gairdner, M.D.
 Jones,—The Principles and Practice of Ophthalmic Medicine and Surgery. By T. Wharton Jones, F.R.S. Third Edition. London, 1865.
 Pathological Society of London, Transactions of the. Vol. 16.
 Silver,—Outlines of Elementary Botany. By Alexander Silver, M.D. London, 1866.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On Insanity caused by Injuries to the Head and by Sunstroke.* By FRANCIS SKAE, M.D. Edin.

(Read before the Medico-Chirurgical Society, 3d January 1866.)

THE following cases and the remarks which accompany them are intended to illustrate the characteristic features, and to describe the usual course and termination of insanity caused by sunstroke, and by injuries to the head.

From personal observation of a considerable number of such cases in the Royal Edinburgh Asylum, and from a careful study of the records of many more in the case books of the institution, I am led to the conclusion, that insanity caused by sunstroke, and insanity caused by a fall or blow on the head, closely resemble each other, and possess some important features in common.

There are also some important points common to both of them which cause them to differ from the forms of insanity resulting from other physical causes.

The pathology of insanity resulting from severe injury to the head is the same, probably, as the pathology of insanity from sunstroke,—to wit, chronic hyperæmia of the brain and its membranes.

I will therefore consider and describe them together as forming a *natural class* or *group*, to which, for the sake of convenience, I will apply the term used by Dr Skae in his classification, *Traumatic Insanity*.

It will be seen from the following cases, that insanity is sometimes the *immediate* result of a severe injury to the head, but that more frequently it is a remote consequence: *months*, or in some cases *years*, elapsing between the receipt of the injury and the appearance of the insanity. In such cases, the connexion between the injury and the insanity is generally indicated by the fact, that a slight change in the character, habits, or disposition is observed immediately after the injury,—this change becoming more and more marked until it culminates in insanity. Injuries to the head have long been universally recognised as a cause of insanity; with the exception, however, of Esquirol and Prichard, no authors seem to have made more than passing allusion to this physical cause of insanity.

Esquirol, in his tables, gives 710 cases of insanity due to physical causes, of which 18 resulted from blows or falls on the head;¹ he also relates² 2 cases illustrative of the time which may elapse between the receipt of the injury and the development of insanity; the interval in 1 case was some months, in the other, fourteen years.

Prichard,³ in his treatise on insanity, states that in his experience he has met with similar cases. He also mentions cases where change of character, not amounting to insanity, has followed severe injury to the head, and continued through life.

With the exception of the above somewhat meagre description, no author with whose writings I am acquainted has given any account of the symptoms or general character of such cases.

I trust, therefore, that the history of the following cases of insanity from injury may be of interest.

I have selected such as will best illustrate the peculiar features which I think are possessed in *common* by insanity caused by injury to the head and insanity caused by sunstroke.

CASE 1.—R. H., æt. 39, a carpenter, married, of sober and industrious habits. Three months previous to admission he fell from a scaffolding; he fell upon his back, the hinder part of his head sustaining a severe contusion, without fracture. Phrenitis supervened soon after the fall, and during his convalescence from the phrenitis, his present malady developed itself, and has now continued for nearly a month. His insanity, therefore, was first apparent two months after the fall. The symptoms of his insanity are monomania of pride, extreme optimism, and delusions of exaltation, such as "that he is a master-joiner, that he is so successful at his trade that he expects he will soon earn upwards of £150 a-week." His bodily condition is weak; he has a slight unsteadiness in his gait, noticed ever since the fall. Such was his state on admission. It was further stated by his friends that no hereditary predisposition existed, and that he was neither epileptic, suicidal, nor dangerous.

For three weeks after admission no change for the better took place; he then began slowly to improve, first in bodily health, then mentally; he became stronger, the unsteadiness in his gait was much less apparent, and he worked daily in the carpenter's shop; at the same time he ceased to express his delusions, and conversed pretty rationally. He continued in the same condition a fortnight longer, and was then *removed as relieved*, in so far that he had ceased to express extravagant delusions, although his manner and speech indicated a mind much weakened.

I regret that his subsequent history cannot be traced, and his case is, therefore, very imperfect; but I think it probable, that as the improvement in his mental state was but slight,—as it did not seem to be progressive, as it seemed indeed almost at a standstill,—

¹ *Maladies Mentales*, tome i. p. 64.

² *Ibid.*, p. 68.

³ *Treatise on Insanity*, p. 202.

that it went but little further, and that this man would always be weakminded, indolent, incapable of doing much for himself.

Remarks.—This case, although by no means a typical traumatic one, affords an illustration of insanity following immediately on acute inflammation of the brain: it is also an illustration of a case of traumatic insanity simulating general paralysis,—the unsteady gait, the optimism, and the extravagant delusions; but differing from it in the absence of other physical signs, in the fact, that the unsteady gait was due to injury of the spine, and was seen immediately after the fall, and was apparently unconnected with the insanity. This man differed from the majority of traumatic cases I have seen, in never showing the slightest irritation or violence of manner; he seemed perfectly harmless, and his case, in this respect, is somewhat exceptional.

The following case is in some respects a contrast to the above.

CASE 2.—J. E., æt. 15, the son of a farmer. His father's statement was as follows:—He was a very intelligent boy until he was five years of age, and at that time he was struck immediately behind the ear by his schoolmaster with a ruler. The blow caused swelling and ecchymosis—no fracture. The same night he screamed out in his sleep; in the morning a change in his manner and appearance was noticed; he seemed stupid and silent; in the course of a few days he ceased to speak entirely, and he remained silent for six weeks; when he began to speak again he talked nonsense; he has never spoken sensibly since, and it is now ten years since he received the blow; he is often very noisy at nights, and he swears a great deal.

His father managed him at home till he brought him to this asylum on account of his violence and his dirty habits. He never had any fits nor any symptom of paralysis. It was also stated that the lad's grandmother, on the father's side, had been insane for several years. On admission, 3d of March, he was a tall delicately-formed lad, of very fair complexion. Mentally, he seemed perfectly imbecile, and his bodily condition was weak. He was very noisy at night, swearing profusely, and he was very dirty in his habits; he was quite harmless, and appeared incapable of understanding anything that was said to him, or of answering any question, however simple.

He was put among the noisy patients, and continued in that ward; little mental improvement ever showed itself in his case; his bodily health rapidly improved, and he became much stouter. For a year and three months after his admission he continued unchanged. On the 16th of May, he had a succession of very severe epileptic fits for the first time since the infliction of the blow which had caused his disease. On the 19th, the fits became more frequent, until they were almost continuous, and he died comatose on the evening of that day. The post-mortem examination was made fifty-six hours after death, and the following appearances were seen:—

Post-mortem Examination.—1. The scalp, calvarium, and dura mater appeared healthy.

2. There was slight opacity of the arachnoid over the superior part of the cerebrum. There was a small quantity of serum in the sac of the arachnoid.

3. The lateral ventricles contained 3iv. of serum.

No morbid appearances were discovered in the brain or cerebellum.

The entire encephalon weighed 46½ oz.

Remarks.—The peculiarities of this case are, the immediate appearance of insanity characterized by almost total fatuity immediately after the infliction of the injury.

The accession, without any premonitory symptoms, and after a lapse of ten years since the injury, of epilepsy proving rapidly fatal.¹

The comparatively slight nature of the injury which was followed by such grave results.

CASE 3.—Mr D. The following is an instance of epilepsy with maniacal paroxysms, the result of severe injury to the head.

D. was brought to the asylum in a very violent paroxysm of acute mania, which had lasted two days, and had commenced immediately after a series of very severe epileptic fits.

History.—He is 24 years of age, single, an officer in the army, a young man of strictly temperate habits. Seven years ago, while in India, he was thrown from a carriage-palanquin, and fell with violence on the left side of his head; there is no account of his condition then, nor is it quite certain whether he had any epileptic fits in India, but it is not likely that he had, as he remained with his regiment for five and a-half years after the accident; he then came home on leave. When he came home, he appeared quite well, and continued so for a year; he then had his first attack, consisting of many severe epileptic fits, followed by a paroxysm of excitement. This was exactly six months before his admission into the asylum. During the six months between his first attack and his admission, he had regular monthly attacks, consisting of a series of fits, followed by great excitement, lasting three or four days.

On admission, he was so violent that two attendants could not restrain him, and he was sent to a padded room to prevent his injuring himself by the furious manner in which he dashed himself about.

His face was puffed, conjunctivæ congested, pupils moderately dilated, pulse full and rapid, skin hot, bowels obstinately confined.

For some days after admission he continued in almost incessant *rotatory motion* from left to right, this he did whether sitting or lying; his eyes were usually open, fixed in a glassy stare; his

¹ The constitutional changes associated with the period of puberty may partly account for the appearance of the epilepsy at this time.

movements were all automatic ; the hand of another was grasped if placed in his ; he moved away if one stood in front of him ; every night at ten o'clock he took some food, but he manifested no consciousness whatever ; he continued for ten days in exactly the same condition, and then slowly and gradually recovered consciousness ; the regular rotatory movements ceased ; for some days he was unable to get up owing to the severe bruises he had sustained in the course of his violent movements ; in a few days he was to all appearance perfectly well ; he complained a good deal of uneasiness on the left side of the head, at the *site of the injury*. Two months after this, having continued quite well in the interval, he had *seven* severe epileptic fits, followed by a maniacal paroxysm, less violent than the former one, but again necessitating his being placed in a padded room to prevent injury to himself ; the attack subsided in a week, when he was again apparently quite well.

It was noticed that pressure on a certain part of the left side of the head gave pain, and induced involuntary contraction of the muscles of the arm of the opposite side. The history of an injury whose position was indicated by a cicatrix, the pain or at least uneasiness *constantly* felt and aggravated before a paroxysm, the peculiarity of the movements in the maniacal state, the pain and twitching in the right arm produced by pressure on the site of the injury, pointed to the existence of some local structural change, probably seated in the bone or subjacent membranes, and to the *possibility* of relieving a probably hopeless case by trephining over the site of the injury. At the request of the patient's friends and medical attendants, Mr Syme performed the operation ; he trephined on the spot indicated by the patient himself ; the operation was performed without chloroform, and the patient bore it with great courage ; he was at the time quite free from excitement, and perfectly rational. On removing a portion of the bone, a very questionable alteration was noticeable. One half of the circular portion removed seemed slightly thicker than the other, and the vascular grooves on its under surface shallower. No alteration was detected on microscopic examination. For four days after the operation, the patient continued to do well ; he then became furiously excited, struggling violently, shouting at the top of his voice, and requiring to be restrained by means of sheets to prevent his injuring the wound ; his pulse at this time was 110, full and strong ; he became a little quieter every evening, and took some food. The excitement continued for twelve days, and then gradually subsided ; he soon went about again, apparently quite well in mind, with his bodily health and strength little if at all impaired by what he had undergone.

He continued well for three months, during which time he had no fits, and was perfectly sane. At this time his friends removed him on trial.

Thirteen months afterwards this patient was readmitted, showing all the symptoms of his previous attacks. During the year of his

residence at home, he had suffered from occasional slight attacks of excitement, and from epileptic fits, at regular intervals. As the detailed history of his case would be mere repetition, I may say, briefly, that since the time of his second admission, in July 1858, just described, he has remained voluntarily in the asylum.

For four years after his readmission he took on an average four epileptic fits a-month; but during these four years he had only five attacks of excitement, the two last of them being of a comparatively slight character.

For the last three years he has suffered less frequently from epileptic fits, and has had *no maniacal* attacks at all. On one or two occasions, when exposed to annoyance or excitement, he showed an uncontrollable impulse to *strike* or commit some violent act, and this excited condition, during which he was apparently only semi-conscious, lasted but a few minutes. With these exceptions, he is perfectly *sane*.

During the last four months he has been taking bromide of potassium (in ten-grain doses, thrice daily), with the effect of reducing the number and the severity of the fits in a very marked degree, and in lengthening the interval between their occurrence to such an extent, that from having two and three fits every month, their number has been reduced to five within the four months; otherwise, he is decidedly improved, his bodily health is excellent, and he shows no indication whatever of insanity.

Remarks.—Notwithstanding the very long time which elapsed between the occurrence of the injury and the development of the insanity and epilepsy in this case, there are several considerations which lead me to believe that both the insanity and the epilepsy were the result of morbid action established by the injury, and to describe the case as a traumatic one. These are, *1st*, the *uneasiness* felt at the site of the injury immediately before a paroxysm; *2d*, the *pain* which pressure on that part occasioned, the *involuntary contractions* of the muscles of the arm of the *opposite* side produced by pressure on the part, and the *giddiness* produced by pressure on the part; and, *3d*, the fact that one half of the circular piece of bone removed by the trephine was thicker than the other, and the grooves on its under surface shallower.

The prospect of the surgical operation being successful was greatly lessened by the difficulty of exactly localizing the spot where thickening was likely to have taken place. The operation, however, was so far successful, that a piece of thickened bone was removed, and pressure on the subjacent parts consequently lessened.

The result of the operation may shortly be stated thus,—that while at the time it was performed, the patient was subject to a regular monthly recurrence of a series of severe epileptic fits, accompanied by a maniacal attack of great violence, since that time, although the epilepsy still continues, the maniacal attacks have

become less frequent and less violent every succeeding year, and during the last three years there has been no maniacal attack at all.

CASE 4.—The following case has peculiar features :—A Russian officer, who had commanded a battery at Sebastopol, and who there received eleven wounds, one of which, a very severe one, was on the back of the head and neck, arrived in Leith, commissioned by his government to purchase some vessels there. While in Edinburgh, he one day, in the Exhibition of Paintings, exposed his person to some ladies; for this offence he was imprisoned, but liberated in consequence of a medical representation as to his mental state. On the day after his liberation, he went about Leith in his dressing-gown, trying to purchase pistols, and was in consequence sent into Morningside Asylum. He stated himself, that after the wound in his head and neck he had suffered from *tetanus*, and subsequently from *epilepsy*, and that since that time he had been irritable and easily excited.

On admission he was confused, alarmed, and suspicious; the morning after admission he asserted that his milk was poisoned. The scars of his wounds were all visible, the principal one being the cicatrix of a rocket wound, which extended over the occipital bone, and a considerable way down the back of the neck. In the course of the next ten days he became composed and reconciled to his stay in the asylum; he seemed rational and agreeable, but irritable and touchy; he was permitted to walk about the grounds on his parole, and his bodily health and appearance rapidly improved. Three months after admission, he displayed a repetition of the habit for which he had been imprisoned, and which he very much deplored himself, although he appeared unable to resist it.

For a month after this he remained in the asylum, perfectly well conducted, and apparently rational; he had no epileptic fits during the four months of his residence in the asylum. He complained at times very much of his head. Although quiet and agreeable, he had the appearance of a man apt to be very easily dangerously excited. In this condition he was removed to his own country, and with his subsequent history I am unacquainted.

Remarks.—The peculiar feature of this case is the curious sequence of effects produced by his wounds. First tetanus, then epilepsy, from which he also recovered; and then this peculiar phase of insanity, liability to dangerous excitement, liability probably to homicidal impulse, as was evinced after his liberation from prison, by his attempting to procure pistols, and liability to a sudden and uncontrollable desire to expose his person to females.

The traumatic *tetanus* was probably the result of one or more of the other ten wounds which he received on different parts of the *trunk* and *limbs*; but the *epilepsy*, from which he recovered, and the subsequent insanity, from which he did not entirely recover,

appear, like the *tetanus*, to have been *traumatic*, and to have been both caused by the same injury, viz., the wound on the back part of the head.

CASE 5.—The following is the case of a man who was also wounded before Sebastopol. I relate it very briefly:—He was admitted in 1857; his skull had been fractured by a stone from a blast at the siege. He was discharged from the army three months afterwards. The first unmistakable evidence which he gave of his insanity was four weeks previous to his admission here. While making hay, he threatened to stick the hay-fork into his father, “as the war had begun.” At this time he was described as a good deal excited, and in the habit of wandering about at night. Four days after this, he again attacked his father, knocking him off his chair; he had a knife in his hand at the time. On this occasion he was taken into custody; he was very excited that night. On admission the next day, he was very quiet, courteous, and polite, and described the way in which he received his wound; he did not appear to have any delusions. The wound was situated near the middle of the left parietal bone. It consisted in the scar of a scalp wound, three inches long; in the centre of the scar a depression half an inch deep, and large enough to receive easily the top of the thumb.

The man himself says that no bone was removed from the wound, and that no operation, besides probing the wound, was performed. He continued in Morningside Asylum for five months; during that time he was *quiet*, and expressed no delusions; his mind appeared weakened, and he seemed to be lapsing gradually into dementia. He was very sullen and obstinate, slovenly and dirty in his manner of eating, and during the latter part of his stay he refused to work at all. He was removed unimproved to another asylum.

CASE 6.—F., a seafaring man, æt. 32, single. Three years previous to admission had received a violent blow on the head, followed by several attacks of insanity, both at sea and on shore.

A few days previous to his admission here he had followed a gentleman about the streets with a knife, and threatened to stab him.

He expressed the following delusions: that he was a descendant of the royal family; that another man's wife was his wife; that the souls of other men took possession of him, and his soul possessed them.

He was brought to the asylum with difficulty, but on admission he was quiet and contented, only expressing a suspicion that some toddy, which was given him, was poisoned; his bodily health was good. For three or four months after admission he continued contented and perfectly tranquil, lounging about smoking, talking now and then very mysteriously of friends in America; he said he was a

Yankee, and commenced to speak like one; he had persistent hallucinations of hearing. Subsequently he became much more incoherent, using unintelligible metaphors in his conversation and letters, — writing long letters, almost quite incoherent, with the words, “No connexion with Kossuth,” on the envelope.

He never made any progress towards recovery, but became even more demented and taciturn. He was eventually removed to an English asylum.

Remarks.—The earlier history of his case affords an illustration of two not uncommon features in insanity, resulting from injury and from sunstroke.

One is homicidal impulse, and the other is the fixed delusion of the possession of high rank.

The incurable, hopelessly, non-progressive nature of the case is also characteristic of *traumatic* insanity, whether resulting from *injury* or *sunstroke*.

The foregoing cases illustrate some of the most prominent features of traumatic insanity resulting from blows or falls on the head. It is needless to read cases of general paralysis of the insane, or of simple dementia following such injuries, as they are not of such general interest as to justify my occupying your time. I will content myself by alluding to the occurrence of such cases, and will now consider *insanity* from *sunstroke*.

Like injury to the head, sunstroke has always been recognised as a physical cause of insanity, and as such is alluded to in most psychological works. Some have given this cause more than a passing notice, and of these I may mention Battie, who, in his treatise on madness, 1758, relates the case of a sailor who became, according to Dr Battie, “raving mad in a moment, while the sunbeams darted perpendicularly on his head.”¹ Arnold (1786) also particularly refers to it (vol. ii. p. 66). Esquirol hardly alludes to it; but of his 710 cases due to physical causes, sunstroke is the assigned cause in 16.² Prichard considers that insanity from this cause is due to inflammation or a state bordering on inflammation in the encephalon, and observes that cooks and others exposed to great heat, are liable to attacks of mania.³ Feuchtersleben (1845) mentions sunstroke as one of the causes of furious mania.⁴ Sir A. Morrison, in his lectures, states that the periodic mania of Charles VI. of France was due to *coup de soleil*.

From the reports of Bethlehem Hospital it is seen that from 1846 to 1860, inclusive, there were admitted 13 cases of insanity caused by sunstroke, 7 of which are said to have recovered. In speaking of these cases, Dr Hood takes the same view of the manner in which sunstroke acts as does Prichard, viz., that it excites “inflammation or a state akin to inflammation, in the encephalon.” He confirms

¹ Battie on Madness, p. 47.

² Op. cit., p. 64.

³ Op. cit., p. 203.

⁴ Med. Psychology, p. 265.

the observation of Prichard as to the liability of cooks and persons exposed to great heat from fires or furnaces to insanity.¹

The latest observations in connexion with the subject are those of M. Brunet, published in the November number of the *Annales Medico-Psychologiques*, on the effects of insolation on the insane.² His paper is a description of the *pellagra* of Italian authors, as observed by him in the patients of the Nîort Lunatic Asylum, and has no direct bearing on the subject of this paper.

The above references are the only ones of importance with which I am acquainted.

It is not necessary that I should here describe the various immediate effects of sunstroke, as I could only do so by reproducing the remarks of others. From the rarity of the accident in this country, its immediate results are seldom seen; and the patients admitted into our asylums have generally, if not always, been insane for some months. The accounts which they bring along with them from abroad of the manner in which they were first affected by sunstroke are invariably imperfect. I will confine myself entirely, therefore, to the description of their insanity.

The following cases are intended to illustrate the most prominent characteristics of insanity due to sunstroke, and the similarity which exists between it and insanity due to injury of the head.

CASE 1.—The following case was admitted some years ago. Mr F., æt. 29, single, an officer in the Indian Army, stated to have been of rather unsteady habits. His mother had a sunstroke followed by temporary insanity.

Two years before coming to this country he suffered from sunstroke; the manner in which the sunstroke affected him at the time of its occurrence is not stated. Shortly after the sunstroke he lost the power of his legs (which were still weak on admission); he was sent home in charge of a sergeant, who left him on landing in this country.

He made his way to the house of a relative; he then appears to have been drinking a good deal,—to have been excited and violent, sleepless at night, sometimes sleeping all day. He declared that he was the Earl of Dalhousie, and talked much of his wealth and importance, his exalted rank, etc.

On being admitted he was very impatient, moody, and discontented; talked with imbecility about his rank in the army and the cruelty of his family. His gait was slightly affected.

He is subject to hallucinations of hearing, especially at night.

He declares his name is John, which is the name of his half-brother, whom he denounces and curses in no measured terms.

He remained some years in the asylum, little or no change taking place in his mental condition; he was occasionally cheerful and

¹ Statistics of Insanity, p. 67.

² Des Effets de l'Insolation chez les Aliénés (pellagre).

agreeable, but more generally moody, suspicious, and irritable. He was continually writing letters to his agent and his friends; some of these he signed "Dalhousie," all the others he signed with the name of his half-brother. His bodily health rapidly improved, and the weakness in his legs entirely disappeared.

He is still in the asylum. For the last two years he has been demented, his bodily health continuing excellent.

His case is perfectly hopeless, and its progress has nothing in it requiring further remark.

Remarks.—His case illustrates the similarity which insanity caused by sunstroke, at its *first development*, bears to general paralysis. I allude to the *unsteady gait*, the *optimism*, and the *delusions of exaltation*.

His case is also an example of the remarkably *chronic non-progressive* insanity which often follows injury or sunstroke, characterized by sudden suspicions, reserve, one or two *fixed* delusions, and irritability of temper.

CASE 2.—R. L., æt. 42; admitted February 20, 1864; single; a coach-builder.

History.—Nineteen years ago, when 23 years of age, he was resident in Calcutta; he suffered from sunstroke at that time, and is stated to have been continuously insane ever since. There is no hereditary predisposition. He is not epileptic nor suicidal, but is dangerous, and is subject to paroxysms of violence at irregular intervals.

On account of his having lately become more violent than usual, he was sent to this asylum. He seems to have been kept at home as long as possible, until his outbreaks and his dirty habits rendered his removal necessary.

On admission he was taciturn, expressed no delusions, and showed no tendency to violence or excitement. He appeared to be demented; his bodily health was very feeble. For one month after admission he improved in bodily health, but continued unchanged mentally. Four months after admission, his bodily health having gradually failed, and his mental state being quite unchanged, he sunk and *died*.

The following was observed at the post-mortem, thirty-four hours after death:—

1. The calvarium was nearly eaten through by one or two pacchionian bodies.
2. The dura mater was firmly adherent in one small patch to the posterior part of the longitudinal sinus.
3. There was a mass of calcareous deposit the size of a sixpence, and of irregular shape, on the anterior part of the dura mater, causing a corresponding depression on the under surface of the calvarium.
4. The brain substance throughout was much softened; the septum lucidum was thickened; the *pineal* body was twice its normal size.

5. The grey matter of the *cerebellum* was markedly divided into two layers.

The encephalon weighed 58 oz. The cerebellum and pons, $6\frac{1}{2}$ oz.

Remarks.—I have given this case more as an instance of the manner in which those very chronic non-progressive cases, often seen in traumatic insanity, terminate, than for any particular feature of interest the case itself possesses.

CASE 3.—J. B., æt. 21; admitted in 1857. Served in the army two years. In the Crimea he suffered from sunstroke; after which, he was four months in hospital, and was discharged and went home. For the twelve months preceding his admission, with the exception of his becoming very easily and very strongly excited by drink, he remained well enough.

The attack for which he was sent to Morningside is described by his friends as follows:—"This is the first attack of insanity; it has lasted four weeks, but he has never been quite right since he left the army. The first symptoms were his *jumping* on the floor and *tearing his clothes*, going out of doors and running for miles, refusing to work, and being very insolent to his parents; he is also restless, night and day."

It was stated that there was no hereditary predisposition, and that the patient was not suicidal.

On admission he was quiet and sullen, and for a few days he conducted himself pretty well, and his bodily health was good. He then became restless, noisy at night, and obstinately mischievous through the day, annoying the other patients. His condition after this, for some months, was very variable; at times he was quiet and civil, at others, surly and noisy, and again sullen and silent.

He gradually became more demented in the course of the next four years. He was dangerous in his temporary attacks of excitement, and struck on the slightest provocation; and on one occasion he effectually cowed one of the most violent men in the house, by the unexpected and entirely successful manner in which he returned an assault.

In the course of another year he became rather more demented, and was rarely excited; he worked daily in the garden, his bodily health continuing good. Last year he was removed unchanged and incurable to the lunatic wards of his parish.

The following case, although an uncommon one, is more typical of traumatic insanity than any of the foregoing.

CASE 4.—Mr G., æt. 23, lately an officer in the army, had served in India; while there he had a sunstroke.

History.—At the time of his admission (in 1863), he had been insane for eighteen months, and his insanity is believed to date from the time he received his sunstroke.

He is stated to be homicidal.

On the voyage home from India he *felled* the mate of the vessel with a large mallet, and was in consequence kept a close prisoner till his arrival in London; he was then detained in Newgate three weeks, was *liberated* on the intercession of his friends, and placed in lodgings in London, living in charge of an attendant. After travelling about for a little with this attendant, he was placed under Dr Skae's care, in the village of Morningside, and was sent into the asylum in consequence of a violent attack upon his attendant.

On admission he appeared very restless and suspicious.

His first request was that he might have a female instead of a male attendant.

For a week after admission he behaved quietly, but frequently expressed a wish for a knife or a dagger, and he repeatedly said he must have a life; he attempted to break his attendant's head with a poker. After this he was at times suspicious and silent, at other times talkative and agreeable; he was fond of violent exercise, saying his health required very long and regular walks.

His delusions, which he frequently expressed, were most extraordinary. Such as: he showed a mark on his finger which, he said, proved that he was either the Duke of Gordon or the Earl of Fife. He saw a sword and two daggers pass across the wall when in London, and the sword is now preserved in Duff House; a naked man stood over him at night, holding over him a drawn sword, and this only happened to members of the Gordon family. The delusion on which his homicidal impulse seemed to depend was, "that he possessed the sword of justice, and that he must have a life to satisfy justice." Under this delusion, when travelling in the North, and staying at the house of a farmer, he walked very quietly up to his attendant, who had just entered the room, holding the poker concealed behind his back, and if the lady of the house, who fortunately happened to be in the room, had not warned the man in time, Mr G. would no doubt have carried out his design for satisfying justice.

Mr G. had various other delusions on a variety of subjects; but he could converse rationally at times and make himself very agreeable. He spent much of his time playing at cricket and taking country walks with his attendant.

No change ever took place in his case.

In his conversation he passed with abrupt rapidity from one idea to another. He generally talked about himself,—his rank, his prospects, his origin, destination; on these subjects he expressed freely his startling delusions.

He would frequently, in the course of the day, make very extraordinary proposals, the most common of which was to "take a man's life."

The following instance will illustrate his distorted perception of the proper relations of things. He was living at a cottage in the country at the time, with several other patients and attendants.

He had conducted himself quite well and quietly until one morning he appeared at breakfast fully dressed, with the exception of his *trousers*. When he declared his intention of going out and walking about in this state, his attendant remonstrated with him and asked why he wished to go out without trousers; he said, "O, somehow I look so *bloody conspicuous* in my trousers that I thought I'd be best without them!" A short time before his removal this patient again exhibited his homicidal propensity in a savage and murderous attack upon his attendant, who had interfered to prevent his escape.

Soon after this he was removed by his friends to be boarded with a medical man in the Highlands.

This, I need hardly say, was done in the face of warning and advice. I have been informed that he has made several violent assaults on his attendant since he left the asylum.

He had been one year and a-half in the house, and no change for the better had taken place.

Remarks.—The most prominent feature in this case is the persistent homicidal tendency shown throughout, in connexion with a fixed delusion that to take a life would be a meritorious action.

In his case, also, the characteristic fixed delusion of the possession of exalted rank is well marked.

The relative frequency of the symptoms of the entire number of cases occurring in the Asylum, from 1848 to 1864, from injuries to the head and from sunstroke, I have endeavoured to tabulate as follows:—

Traumatic Insanity.

TABLE I.—*Relative Frequency of the Symptoms in 38 Cases of Traumatic Insanity, due to Injury of the Head.*

Epilepsy, ...	4
Mania, ...	30
Delusions of Exaltation, ...	5
" of Pride and Self-esteem, ...	13
" of Suspicion, ...	11
Melancholia, ...	1
Dementia, ...	3
Hallucinations of Hearing, ...	2
" of Vision, ...	1
" of both senses, ...	2
Suicidal, ...	8
Dangerous, ...	27
Homicidal, ...	7

TABLE II.—*Relative Frequency of the Symptoms in 16 Cases of Traumatic Insanity from Sunstroke.*

Mania, ...	9
Delusions of Exaltation, ...	5
" of Pride and Self-esteem, ...	7
" of Suspicion, ...	6
Kleptomania, ...	1
Dipsomania, ...	2
Dementia, ...	4

Hallucinations of Hearing,	.	.	.	.	.	4
" of Vision,	.	.	.	.	.	5
Suicidal,	.	.	.	.	.	2
Dangerous,	.	.	.	.	.	11
Homicidal,	.	.	.	.	.	2

These Tables show that, in traumatic insanity, the most *constant* symptoms are, *excitement* more or less acute, fixed delusions of pride and self-esteem, delusions of suspicion.

They show also that most patients labouring under this form of insanity are dangerous, and that the so-called homicidal impulse is comparatively frequently met with in this class of cases.

The remaining symptoms require little comment.

Dementia.—Three cases from injury and four from sunstroke were admitted in a state of dementia. *Melancholia*.—It is seen at once that in traumatic cases melancholia is rarely met with, none of the sunstroke cases being so affected, and only one of those where insanity was caused by injury.

Hallucinations of the senses, in such cases, are generally persistent to a somewhat remarkable degree, and they appear to give rise to delusions of suspicion and to dangerous impulses in those who are liable to them.

Of the *ten* cases marked in the Tables as *suicidal*, one only was melancholic. In the remaining *nine* the impulse was transient, and displayed only during a paroxysm of maniacal excitement.

The results of these cases are as follow :—

TABLE III.—Results in the 38 Cases due to Injury.

Recovered,	.	.	.	.	.	4
Removed improved,	.	.	.	.	.	7
Removed not improved,	.	.	.	.	.	9
Became demented,	.	.	.	.	.	8
Died,	.	.	.	.	.	9
Under treatment still,	.	.	.	.	.	1
Total,	.	.	.	.	.	38

TABLE IV.—Results in 16 Cases due to Sunstroke.

Recovered,	.	.	.	.	.	1
Removed improved,	.	.	.	.	.	2
Removed unimproved,	.	.	.	.	.	4
Remain unimproved,	.	.	.	.	.	4
Became demented,	.	.	.	.	.	1
Died,	.	.	.	.	.	4
Total,	.	.	.	.	.	16

TABLE V.—Causes of Death in the Cases due to Injury to the Head.

Epilepsy,	.	.	.	.	.	1
General Paralysis,	.	.	.	.	.	3
Meningitis,	.	.	.	.	.	1
Hyperæmia of the Brain,	.	.	.	.	.	3
Exhaustion,	.	.	.	.	.	1
Total,	.	.	.	.	.	9

TABLE VI.—*Causes of Death in the Cases due to Sunstroke.*

Phthisis,	.	.	.	.	.	.	.	2
General Paralysis,	.	.	.	.	.	.	.	2
								4

The Tables show at once the incurable nature of this form of insanity. Of the whole fifty-four cases only five recovered. Of these five, two had suffered from *previous* attacks, which render it highly improbable that their recovery in *this instance* would be permanent.

The *majority*, it is seen, were either removed *unimproved*, remain unimproved, or became hopelessly demented.

It is seen that thirteen of the fifty-four cases died.

The limits of this paper will not permit me to enter into the pathology of these cases, as it is quite impossible to do justice by condensed remarks to the importance of this part of the subject. I will confine myself at present to pointing out, that in this form of insanity a comparatively large proportion of deaths are due to brain disease.

Of the thirteen deaths ten were due to brain disease, and in this traumatic insanity differs in a marked way from other forms. Like general paralysis, it has a tendency to a fatal termination.

The pathological condition on which traumatic insanity appears to depend is *hyperæmia* of the brain and its membranes, the evidences of which are pretty constantly seen in the post-mortem examinations made in these cases.

From a careful consideration of the history of all the cases whose symptoms and results are given above, from observations of the characters and progress of many such cases, and from a review of the most constant pathological appearances, I am led to the following conclusions:—

1st, That traumatic insanity is generally characterized at its commencement by maniacal excitement, varying in intensity and duration.

2d, That the excitement is succeeded by a chronic condition, often lasting many years, during which the patient is *irritable*, *suspicious*, and *dangerous* to others.

3d, That in many such cases distinct homicidal impulse exists.

4th, That the characteristic delusions of this form of insanity are those of *pride*, *self-esteem*, and *suspicion*, melancholia being very rarely present.

5th, That this form of insanity is rarely recovered from, but has a tendency to pass into *dementia*, and to terminate fatally by brain disease.

6th, That the symptoms, progress, and termination of insanity, resulting from traumatic causes, are sufficiently distinctive and characteristic to entitle it to be considered a distinct form of insanity.

ARTICLE II.—*On Certain Causes affecting the Origin of Disease in Large Towns.* By THOS. WILLIAMSON, M.D., F.R.S.E., F.R.C.S.E., etc.

IN the year 1856, I published a pamphlet, titled "Sanatory Remarks in Connexion with Nuisances," and mainly addressed to students and young physicians.

At the period referred to the health of towns did not engage so much attention, either on the part of doctors or legislators, as it has latterly and very properly done. How far my publication may have helped on the good cause I have no means of knowing, but have reason to believe that through its influence many of those sanatory measures which have recently proved of so much importance to the town of Leith owe their origin.

The following observations, then, may be regarded as simply a practical sequel to the remarks contained in my pamphlet.

As one of the oldest parochial surgeons in Scotland, and occupying one of the largest town districts, I venture to submit a few observations, which may not be altogether uninteresting, upon one or two *operative* causes in the *production* and *development* of disease, more especially in large towns.

I commence by remarking, that I have frequently had reason to regret that no power was vested in me, either as a private or public practitioner, *under certain circumstances*, to compel the removal of patients labouring under infectious disease from their own homes to an hospital for treatment. We are all painfully alive to the rapidity with which contagious disorders frequently spread themselves throughout a town or district. Whatever may be the *direct* cause in the development of zymotic disorders—different individuals, perhaps, adopting opposite views of the matter—I think that we are, nevertheless, all agreed upon this, viz., that when an epidemic is about to visit a particular locality, it invariably, so to speak, shows a partiality for pouncing upon a prepared soil. In other words, its favourite haunts are to be found in the abodes of squalid misery, where, from a variety of operating causes, the ordinary laws which regulate health have not been maintained or cared for.

Once having fastened its poisonous grasp upon any given spot, its natural tendency resembles the fire in the forest creeping slowly and insidiously from tree to tree, and halting not until its history is unfolded in the complete destruction of vitality and strength. To follow out the analogy, however, when the crackling of the burning bushes is first heard, and before the conflagration has spread its devastating influence very wide, all that is in general requisite to stop it on its onward destructive course is to clear an open space between it and the adjacent brushwood, which is so prone to ignite. In like manner, I believe that we possess a prodigious agency for the arrestment of the spread of contagious disease, if we were but

alive to the fact, that timely alarm and prompt and vigorous action in removing the first victims from their own hovels to a well-conducted and efficiently ventilated hospital, constitute the likely means towards the consummation of so desirable an end.

I admit that we may be perfectly alive to the fact, that such a course of procedure is desirable, and nevertheless have the mortification to be deprived of the power of carrying it into full and practical operation. There is, I believe, in every great town a large class of migratory beings who, in general, constitute the staple community of low and ill-constructed lodging-houses. I have again and again been called to visit such parties, just at the commencement of the outburst of some epidemic disorder; and knowing the benefit likely to accrue to themselves *directly*, and *indirectly* to the general public, by their removal to a public hospital, have strongly urged upon their consideration the adoption of such a step. In too many instances, however, my advice has been completely thrown away. Moral suasion is inoperative, and upon the slightest attempt to assume a high-handed mastery over the patient, his friends will snap their fingers in your face, and triumphantly remind you that, in accordance with the law of the land, no lodging-house keeper can turn a sick inmate to the door; and as the house is not in the strict sense of the term overcrowded, they will just remain where they are. This is by no means a solitary instance in my experience. I have frequently had occasion to deplore its occurrence, and to find myself completely powerless in the hands of prejudice and error. But very recently I visited, in a very small room, situated in a dense population, a boy labouring under typhus fever. His mother turned a deaf ear to my entreaty for removal to the hospital. Keep him she would, although I pointed out her own personal danger, and keep him she did; but it was at the sacrifice of her own life, and the manifest detriment of the patient himself. Had I possessed the power to have had that boy removed, even against the inclination of his mistaken parent, it is not unreasonable to suppose that both mother and son would have been alive at this day.

It is sometimes more easy to point out an evil than to suggest the means of remedy, and here probably difficulties may present themselves the moment you attempt by legislative enactment to interfere with the so-called liberty of the subject, even for his own benefit. The cry of class legislation rings in our ears. I, for my own part, would be very well contented to listen to that cry, provided I possessed the power of benefiting the individual even against his own desire, and perhaps by so doing, at the same time, be the means of saving many valuable lives.

I do not know whether it could have been accomplished, but I should have liked well if my friend Provost Lindsay of Leith, in his zeal for the improvement of the health of burghs in Scotland, had introduced into his bill some clause, in virtue of which the

hands of medical men could have been so strengthened that no case such as I have described should ever occur again. To my view, at least about the outbreak of an epidemic, a virulent case of small-pox or typhus fever being allowed to pass through all its stages in the midst of an over densely populated locality is a worse nuisance by far than the grunting of a few pigs, or the existence of some small ash-pit, and yet these are not suffered to remain. I remark, as still bearing upon the same subject, that in all considerable-sized old towns there are many dwellings placed under the level of the general surface, used in the double capacity of a *private* and *public* house. For the sake of ample accommodation, the apartments or compartments are of the most diminutive character, some being used by the unwary traveller or the creature of dissipation, whilst others are designed as sleeping abodes. In the latter, it not unfrequently happens that some of the members of the family are lying with bad typhus, whilst all the time, to the manifest detriment of the patient himself, and the positive danger to those who, unaware of the circumstance, frequent the house, the business of the establishment is still carried on. It would never do for an hospital-van to be seen at that door; *the door itself might as well be shut*, the house would get a *bad name*, and business might take its flight for ever. Such is the train of argument frequently indulged in, and, if not openly advanced, plainly left to inference, when the prejudice regarding a public hospital forms an element against the removal of the sick. By way of illustration, I may mention that some time ago I attended a case of typhus fever in one of such houses as I have described. The case ran its usual course, and fortunately the patient recovered. Hospital accommodation was in this instance scorned, and the house, being what is called a "*Free-and-Easy*," was crowded every night with the low and dissolute, who regaled themselves with emptying their cups, and listening to the discordant strains of music. Now, I mean to connect the case just referred to with a fact, and thereafter to draw an inference.

For several months prior to the occurrence of this case, we had in Leith a very severe and fatal epidemic of typhus, which, however, had apparently all but left the town. Curiously enough, and whilst the case now under consideration was passing through its various stages, the disease again broke out in various districts of the town, carrying off many victims. I ask, is it too great a stretch of the imagination to suppose that this very case to which I have referred might not have been the medium of transmitting the contagious poison from its own immediate locality to various other districts of the town, through the many parties who, from time to time, were in the habit of frequenting this place of resort? I felt, in connexion with this case, how thankful I should have been could I have been able, by application to a sheriff or magistrate, to obtain a warrant for the forcible and compulsory removal of the patient to a public hospital. I love liberty of conscience, but I love the en-

lightenment of a dark and blinded conscience still more ; and when we find human beings wrapped up in error, warped with prejudice, the moral suicides of their own comfort, and, it may be, the unconscious murderers of their fellow-race, are we to stand by and say, Let this state of matters continue ; or, rather, should we not use every effort, not even stopping short of legislative interference, for the arrest of so foul and palpable a misdemeanour ?

In still further attempting to connect the progress of disease with some manifest cause, I make the somewhat trite observation, that from whatever source *filth* accumulates in a small household, it invariably brings along with it a powerful element not only for the origin but the propagation of infectious disorder. In my experience of the habits and houses of the poor, the *accumulation*, and, above all, the *retention* of *slops* within doors for an indefinite period, enters most largely as a cause into the spread and development of contagious ailment. Indeed, in many instances, the poor can scarcely be blamed for this state of matters, occupying, as they do, hovels in the centre of a town near which no dust-cart can ever approach, and deprived as they are of water-closets ; as a necessary result their nasal organs become so accustomed and reconciled to the odour of corruption, that, rather than put themselves much about, so long as the common pail does not run over, it is too frequently permitted to remain undisturbed underneath the form of some wretched pallet of straw or chaff. Its presence, however, cannot *always* be endured, and, as I have elsewhere remarked, perhaps at the dark hour of midnight, the squalid mistress of some festering household descends from her lofty altitude, and, stealthily watching her opportunity—perfectly aware of the necessity of caution in the act—hurriedly dashes the contents of her filthy pail, the recipient, it may be, of a week's gatherings, into the most convenient nook or gutter. The deed thus done in darkness may yet proclaim itself upon the hometop, and not unfrequently, it is believed, returns to its old home, though it may be in a somewhat altered form.

It is scarcely necessary to illustrate the truth of what I have been advancing : we all know that in clean, well-ventilated houses, a case of malignant typhus may show itself, and yet those who are continually going about the patient, and dwelling within the same walls, altogether escape the disease. The reason is obvious. Generally speaking, fever must have something to feed upon ; deprive it of its aliment, and it forthwith languishes and dies ; minister to its requirements, it flourishes and extends. Hence, on the other hand, in the houses which I have been referring to, the common course observed is, for the disease to lay down one after the other of the inmates until the whole family become tainted. We all deplore this state of matters, but until the poorer classes have themselves some more enlightened idea of the laws which regulate health, and more especially until they are provided with other house accommodation than they now enjoy, I am afraid things will remain as they are at present.

One means of cleanliness, however, should be employed by the poor in common with the rich, and that is the existence of water-closets in their houses. Whilst the proprietor enriches himself at the expense of the tenant, he is morally bound to return an equivalent in the form of a guarantee that the health of those by whom he is directly benefited shall upon no account suffer through any omission of his.

As a necessary result of the want of water-closet accommodation, we have unfortunately in all large Scotch towns a condition of the streets and lanes thoroughly to be deplored. I have been told that educated and refined Englishmen, when visiting some of our Scottish cities, have been disgusted with the filthy habits of the people; for it is no uncommon sight to witness young children (and some of those who ought to know better), without the exhibition of the smallest degree of delicacy, at once squatting themselves down upon the public pavement, and there and then obeying the laws of nature. As a direct consequence our streets are too often loaded with excrementitious matter; the travelling stranger has to pick his steps with careful observation; the feelings of the more sensitive of the people are outraged, and an additional cause thus engendered for the spread and propagation of infectious disorder.

As it appears to me, another very abundant cause of disease is to be found in the want of proper *breathing space* for those who are compelled, from force of circumstances, to pass the night in some small lodging-house. If we admit that, at the *very least*, not less than five or six hundred cubic feet of air are required for the healthy breathing of each single individual, what shall we say if *twelve* adults are found sleeping together in a small apartment with not more than 1200 cubic feet of air in all? And yet this is no theoretical case: I have seen it again and again. The only remedy for this state of matters is, in so far as regularly recognised lodging-houses are concerned, to have a properly-qualified inspector appointed, to furnish him with a correct list of such houses, and let him exercise his own discretion in sudden and unlooked-for visitation. This, in so far as publicly recognised lodgings are concerned, might go far to correct the evil, although, doubtless, it would not prove the means of diminishing the population of the otherwise overcrowded dwellings of the poor in general.

That accumulated and undisturbed filth may remain long in a locality without appreciably affecting the health of the neighbouring population is a truth now very generally admitted by medical men.

The profession, however, are equally agreed upon this, viz., that in certain conditions of the atmosphere, these filthy deposits become charged with all the elements of death and destruction; so that, upon the occasion of a typhoid epidemic, the localities where it will most fatally rage can be predicted with something like moral certainty.

This leads me to advert to what, in my opinion, although an *occult*, is nevertheless a most powerful agent in the spread and dissemination of disease. I allude to the enormous accumulation of

old beds and bedsteads, with the accompanying drapery, such as it is, which is always to be found in the crowded localities of the poor. Some of those beddings may not have seen the light of day for years; in fact, many of them are in a state of putridity, at once replete with life, and pregnant with death. Upon the occasion of epidemic outbursts, we have here, so to speak, a prepared soil for the disease to feed upon, and thus extend its deadly influence to other, and, it may be, more cleanly localities.

Could it be accomplished, it would indeed be a step in the right direction, were the wealthy philanthropists of the present day to originate some scheme by means of which, and in connexion with epidemic visitations, much of the garniture of our old hovels might be consigned to the flames, and something not very costly substituted in its stead.

So far as my observation extends, I think that the poor in general are but little alive to the importance *of ablution* as a powerful means of retaining health and warding off disease.

Many may probably have the desire, but lack the opportunity for thus cultivating habits of cleanliness. The water may be had, but its application to the body would outrage every feeling of delicacy were it resorted to in apartments so small, and in which a whole family may be huddled together. Hence the propriety of all large towns possessing public baths, available at all times by the poor, at next to a nominal charge.

In passing, I may simply repeat what is already well known, that intemperance and improvidence on the part of parents tend most materially to swell our mortuary tables, so far as the juvenile population is concerned.

How far improvidence on the part of parents may be fostered by the present character of state relief, I cannot take upon myself to say. It is, however, I think, an undeniable fact, that much of that sturdy and independent spirit which in former years used to characterize the Scottish poor has seemingly passed away. Parents are now seemingly less careful for the welfare of their offspring, whilst children grow up to man's estate little aware of the obligation they lie under to contribute towards the comfort and support of their parents in the declining years of life. As other well recognised sources of disease, I have simply to mention insufficient and imperfect drainage, along with a limited quantity and impure quality of water. Whilst the sources of disease to which I have hitherto been referring are general or applicable to every large town, there are one or two which specially affect the town in which my professional labours are carried on. Leith has for a long time past enjoyed the unenviable notoriety of exceeding many other large towns in respect of juvenile mortality, and the attention of the local authorities being directed to the circumstance, three of the medical practitioners (of whom the writer was one) were requested to draw up a report on the matter. Amongst other manifest causes for the excess of juvenile "mortality," the reporters were constrained

specially to particularize *two*, namely, the existence of the old churchyard or burying-ground of South Leith, and the filthy condition of the harbour of the town itself. I may mention that this old burying-ground is situated in the very heart of the town, and has been used as a place of sepulture for at least ten or twelve centuries. In extent it occupies about two acres of ground. I find from the records of the kirk-session, that from 1846 to 1860 no fewer than 6395 individuals were interred within its area, or, in other words, nearly one-third of the entire population were buried there during a period of about fifteen years (assuming the population at that time to have averaged about 21,000 individuals). Connecting this fact with the antiquity of the place itself, it would almost baffle the power of arithmetic to compute the number of the dead who lay beneath its surface. In certain conditions of the atmosphere, the odour exhaled is exceedingly offensive, particularly during warm sunshine after the prevalence of continued rain. So densely packed are the remains of the dead, that in some instances the coffins are not placed more than twelve or eighteen inches below the general surface of the ground; and so much is the want of space experienced, that it is very much to be feared, that for some time past, the sexton has been in the habit of too early disturbing the last resting-place of some sleeper in the dust, in order to afford room for the depositing of some other corpse. This fact may be inferred from the circumstance that coffin-plates have been discovered among the debris of what has escaped the consuming agency of fire, bearing a date not more than twenty months from the date of death, as recorded in the Registrar's journal. Under these circumstances, it is not difficult to conceive that the existence of this old churchyard should exercise a most important influence upon the public health. In the report upon this subject, transmitted to the local authorities, the reporters say, "that bearing upon malarious influence as a fruitful source of disease and death, they invite attention to No. 4 district of the town, containing a population of 2340, and including within its area Brickwork Close, Coatfield Lane, etc., all of which localities being more or less in close juxtaposition to the old burying-ground of South Leith, as contrasted with No. 2 district. This latter district contains a population of 1915, and embraces Bath Street, Campbell's Court, etc., these localities, however, being comparatively removed from the neighbourhood of the burying-ground. Whilst, therefore, the population in No. 4 is 2340, and that in No. 2, 1915, the rate of mortality is highly disproportionate to the difference of population. For, whereas district No. 4, in the neighbourhood of the burying-ground, gives as proportion of deaths under *five* years to deaths at all ages at the rate of 66 per cent., district No. 2 only yields at the rate of 43 per cent. This result must appear all the more striking, when it is remembered that, until very recently, No 2 district, which furnishes the least amount of juvenile mortality, was one of the worst-drained localities in the town." I have the gratifi-

cation of adding that in consequence of our report, this old place of sepulture is now, practically speaking, closed.

Another very special cause as affecting the health of the community of Leith is to be referred to the present condition of its old harbour. Much has of late years been both written and said regarding the Water of Leith as a public nuisance. With this troubled question I do not mean at present to intermeddle: its existence may be injurious to public health, or it may not; that is not the point which I intend to establish: I do not wish to disturb its troubled streams, but prefer coming nearer home, by directing attention to the old harbour of the town, which may be said to be the grand depository of all its treasures. It is an admitted fact that, during the ebb of the tide, the basin of the harbour is continually exhaling from its bed a large quantity of most deadly and poisonous gases, the result of animal and vegetable matter undergoing the process of decomposition, whilst at the same time numerous eyelets, connected with different large drains, are continually discharging their fetid contents into the common reservoir. Standing on either of the draw-bridges at low tide, and looking down upon the surface of the water, one would at first be disposed to fancy that it was disturbed by a heavy fall of rain: the numerous bubbles are, however, not the result of rain, but the constant and rapid escape of poisonous gas from the slimy bottom of the harbour. One cannot fail to recognise in this state of matters a most fruitful source of disease. Accordingly, if district No. 6 of the town—including Broad Wynd, Burgess Close, Shore, etc., all in immediate proximity to the harbour—is contrasted with district No. 7, which includes Anderson's Place, Arthur Street, Brown's Entry, etc., but remotely situated from the harbour, it will be found that, whereas the population in both districts is nearly similar, No. 6 being 2224, and No. 7, 2289, the rate of juvenile mortality is nevertheless most strikingly different, No. 6 (harbour district) furnishing, as proportion of deaths under five years to deaths at all ages, no less than 63 per cent., whilst No. 7 gives a return of only 32 per cent. During the past twelve months typhus fever of a very bad type has prevailed in Leith, and from the return of the hospital house-surgeon, it appears that from August 12, 1862, till July 1, 1863, not fewer than 181 cases of the disease had been under treatment in our local hospital. I find, in running over the Table, that the large proportion of 55 out of the 181 cases treated were resident in close proximity to the harbour.

I trust that the present disgraceful condition of our harbour, so fraught with danger to the lives of the community, will not be permitted to continue so much longer; but that, through the harmonious co-operation of the town councils of Edinburgh and Leith, who are at present engaged in the matter, a new and happy era in its history may soon arise.¹

¹ Since the above remarks were written, the Edinburgh and Leith Sewerage Bill has been passed; so that, in the course of a short time, we may expect confidently that the harbour nuisance will cease to exist.

ARTICLE III.—*A Plea for Criminal Epileptics.* By J. B. THOMSON, L.R.C.S. Edin., Resident Surgeon, General Prison for Scotland, Perth.

EPILEPTICS, and especially the criminal class, seem to me entitled to more consideration and sympathy than they have hitherto obtained. They surely ought not to be regarded by the parochial authorities as able-bodied, being generally disqualified, by reason of epileptic seizures, for continuously following any regular trade or calling; and from their mental condition, defective in self-control, they are very liable in common life to lapse into poverty and crime. One is tempted, looking at the neglect of this class, to believe that the old superstition embodied in the names, "*morbis scelestus*" and "*morbis demoniacus*," and in the sentence, "he hath a devil," may have operated in turning away the humane and the intelligent from the claims of epileptic patients.

For many years criminal epileptics have been brought under my medical care, and the study of them has led me to prefer a statement in their favour. Accordingly, I have drawn out a table of their admissions to the General Prison of Scotland, during the decennial period 1855 to 1864 inclusive, with some particulars of each case, so far as I could collate them; and after setting forth this table, with an analysis of its details, I shall venture to offer some deductions therefrom applicable to my plea for criminal epileptics.

The cases are not selected, but set forth in their chronological order of admission.

The following table shows the admissions during the ten years under review to have been in all 69, but the total prisoners were only 59, some appearing as admitted several times during this period. The annual admissions, therefore, average about 6. The total average daily population of the General Prison for the ten years 1855 to 1864 inclusive, amounted to 6273, or 627 per annum,¹ so that 0·94 per annum, or nearly 1 was the percentage of our criminal epileptics. The calculation taken from the death rate in England has been given at 0·009 per cent. of epileptics among the civil population; and the Army Statistics, by Dr Balfour, from extensive data show the same. There must, therefore, be a great excess of epilepsy among the criminal compared with the civil and army populations.

¹ Note of daily average prison population.—

Years :	1855.	1856.	1857.	1858.	1859.	1860.	1861.	1862.	1863.	1864.	Total.
Numbers	498	583	593	561	632	675	637	688	714	692	6273

TABLE, with the Particulars of 59 Epileptic Prisoners admitted to the General Prison for Scotland during the Decennial Period 1855 to 1864 inclusive.

No.	Name.	Ages.		Year of Admission.	Epileptic before Admission.	Causes of Epilepsy.	Offences.	No. of times in General Prison.	Mental Condition, etc.
		M.	F.						
1	F. W.	22	...	1855	Before.	Unknown.	Theft and housebreaking.	Once.	1. Weak-minded, and in delicate health.
2	W. C.	21	...	...	Do.	Do.	Forgery.	Once.	3. Weak-minded; had frequent fits, three or four daily, followed by mental aberration.
3	B. M. C.	33	...	...	Do.	Do.	Theft and previous conviction.	Twice.	4. Health indifferent.
4	A. H.	19	...	...	Do.	Do.	Do.	Once.	Do., some doubts whether the man is a malingerer.
5	R. E.	18	...	...	Do.	Do.	Do.	Twice.	5. Weak-minded; was insane.
6	C. M.	28	...	1856	Do.	Do.	Do.	Twice.	7. Do., very often in prisons; at present a convict.
7	M. B.	20	...	...	Do.	Do.	Do.	Once.	8. Imbecile.
8	R. L.	18	...	...	Do.	Do.	Do.	Once.	
9	C. B.	19	...	...	Do.	Do.	Contravention of Stat. Will IV. c. 24, § 2.	Thrice.	
10	A. M. C.	48	...	...	Do.	Do.	Theft and previous conviction.	Once.	11. Weak minded; had frequent fits.
11	J. W. Q.	18	...	1857	Do.	Do.	Uttering base coin.	Once.	12. Do.; this man's offences were assaults on his wife; was often in prison for the same offence.
12	J. C.	26	...	...	Do.	Intemperance.	Assault and previous convict.	4 or 5 times	13. Weak-minded.
13	T. B.	20	...	...	Unknown.	Do.	Theft and previous conviction.	Once.	15. Weak-minded on admission, and became insane.
14	J. C.	30	...	...	Do.	Do.	Do.	Once.	16. Imbecile; had led an irregular life, fits frequent, and about the time violent, and given to assault. Head malformed. He became fatuous. Fit for an asylum.
15	A. R. or G.	44	...	...	Do.	Do.	Mobbing, robbery, assault.	Once.	17. Had been insane before admission, and had temporary attacks of acute mania after imprisonment, often violent, requiring restraint.
16	A. B.	27	...	...	Do.	Do.	Do.	Once.	18. Imbecile; became temporary insane, often violent, and [restrained].
17	J. W.	38	...	1858	Do.	Injury to skull.	Assault, injury to person.	Once.	19. Do.
18	H. G.	18	...	...	Do.	Unknown.	Theft, housebreaking, pre. con.	Twice.	20. Do. Few fits.
19	J. M. K.	23	...	...	Do.	Do.	Reset of theft.	Once.	21. Partially paralytic, given to masturbation; frequently violent, and given to assault his own wife.
20	W. M. G.	40	...	...	Do.	Do.	Theft and pre. con., falsehood.	Twice.	22. Paralyzed in organs of deglutition, imbecile, with frequent fits of acute mania, after which she became idiotic.
21	W. J.	30	...	...	Do.	Do.	Fraud, assault.	Twice.	23. Weak-minded and suicidal.
22	A. M. A.	...	18	1859	Do.	Unknown.	Theft and previous conviction.	Thrice.	24. Weak-minded; had severe and frequent fits. Suicidal.
23	T. L.	26	...	...	Do.	Do.	Bigamy.	Once.	25. Weak-minded, health bad, had been very dissipated; fits sometimes more than one daily, and violent.
24	P. C.	12	...	...	Do.	Do.	Theft and previous conviction.	Once.	26. Imbecile, and partially paralytic; at times fatuous. A frequent prisoner.
25	B. S. M. C.	...	48	...	(The same as Do.)	Intemperance.	Do.	Twice.	
26	J. R.	...	28	...	Do.	Unknown.	Theft, habit and repute.	Once.	

27.	A. C. T.	...	...	Do.	Congenital.	Theft, habit and repute.	Twice.	27. Imbecile; soon after liberation was again taken up and imprisoned.
28.	J. F.	72	...	Do.	Unknown.	Do.	Once.	28. Imbecile; an old man worn out in mind and body.
29.	M. B.	22	...	(The same as Do.)	No. 7.)	Do.	Twice.	29. Weak-minded, often violent, and given to assault; requiring restraint. Now a convict.
30.	M. G.	...	...	Do.	Do.	Do.	Once.	
31.	M. G. J.	...	...	(The same as Do.)	No. 5.)	Do.	Twice.	31. Imbecile; was insane, fits frequent, severe; violent, requiring restraint.
32.	J. C.	...	...	(The same as Do.)	No. 12.)	Do.	Thrice.	32. Weak-minded; a regular jail-bird; set fire to a reformatory.
33.	J. M. D.	...	...	(The same as Do.)	Congenital.	Wifal fire-raising.	Twice.	33. Weak-minded; often in prison; fits frequent.
34.	P. A.	17	...	Do.	Unknown.	Theft and previous conviction.	Once.	34. Do., was insane, head malformed—fits severe, and frequent.
35.	J. D.	17	...	Do.	Congenital.	Theft.	Once.	
36.	G. J. M.	46	...	Do.	Do.	Theft and previous conviction.	Once.	36. Discharged from the army for epilepsy; fits severe, and outrageous at the time.
37.	C. E. or A.	...	...	Do.	Unknown.	Do.	Once.	37. Weak-minded; a violent woman; first took a fit in a dark punishment cell.
38.	W. C.	21	...	After.	Excitement.	Do.	Twice.	38. Weak-minded.
39.	S. M.	...	...	Do.	Do.	Do.	Once.	39. Do. Fits slight.
40.	W. M. K.	40	...	Before.	Unknown.	Do.	Twice.	
41.	P. T.	42	...	Do.	Do.	Do.	Once.	41. Weak-minded.
42.	E. R. or B.	...	...	Do.	Congenital.	Theft and housebreaking.	Thrice.	42. Do.
43.	W. D. or D.	...	...	Do.	Do.	Assault with intent.	Once.	43. Do.
44.	J. M. L.	...	...	Do.	Excitement.	Assault and robbery.	Once.	
45.	J. G.	...	...	Do.	Unknown.	Forgery, and uttering bill.	Twice.	45. Weak-minded.
46.	C. R.	...	...	Do.	Congenital.	Theft and previous conviction.	Once.	
47.	J. J.	...	...	Do.	Do.	Theft and housebreaking.	Twice.	
48.	J. C.	...	...	Do.	Do.	Do.	Once.	47. Imbecile, often violent, requiring restraint.
49.	W. K.	19	...	Do.	No. 31.)	Robbery.	Twice.	
50.	J. B. or M. D.	...	...	(The same as Do.)	No. 32.)	Do.	Thrice.	48. Weak-minded; health indifferent.
51.	H. D.	...	...	Do.	No. 21.)	Theft and previous conviction.	Twice.	50. Died insane from brain disease; fits severe—died none.
52.	J. M. A.	...	...	Do.	No. 27.)	Do.	Once.	51. Weak-minded and paralytic.
53.	A. T.	...	...	Do.	Congenital.	Do.	Twice.	52. Weak-minded on admission; died from brain disease.
54.	J. W.	...	...	Do.	Do.	Do.	Once.	53. Weak-minded, in bad health, scrofulous and syphilitic; severe fits, and often.
55.	J. A.	...	...	Do.	Do.	Do.	Once.	54. Weak-minded; fits frequent and violent.
56.	J. H.	...	...	Do.	Unknown.	Do.	Twice.	
57.	M. F. or K.	...	...	Do.	No. 7.)	Do.	Once.	55. Weak-minded; fits frequent and severe; hæmatemesis.
58.	J. M.	...	...	Do.	Unknown.	Do.	Twice.	56. Health bad.
59.	A. C.	...	...	Do.	Congenital.	Reset of theft and prev. conv.	Once.	57. Weak-minded.
60.	M. B.	...	...	(The same as Do.)	Excitement.	Theft and previous conviction.	Once.	
61.	C. M. M.	...	...	Do.	Do.	Do.	Twice.	
62.	P. A.	...	...	(The same as Do.)	Unknown.	Do.	Thrice.	
63.	H. S. or M.	...	...	Do.	No. 31.)	Do.	Once.	
64.	J. D.	...	...	Do.	Congenital.	Do.	Twice.	
65.	H. R.	...	...	Do.	Do.	Do.	Once.	
66.	C. D.	...	...	Do.	Unknown.	Reset of theft and prev. conv.	Once.	
67.	A. M.	...	...	Do.	Excitement.	Theft and previous conviction.	Once.	

The 59 criminal epileptics enumerated show, in our table, males 38, females 21, a proportion very different in the sexes from what is given from extensive figures by Esquirol and Moreau, who show that females are more liable to this disease than males, in the proportion of 7 to 3. Perhaps this explanation may be offered, that females stand imprisonment better than males, from their habits of seclusion. It is worthy of notice, that the female criminal lunatics of Scotland number generally only half of the males.

The ages on committal were as follow :—

Ages.	Male.	Female.	Total.
16 and under 20	10	2	12
20 " 30	10	8	18
30 " 40	7	4	11
40 " 50	8	4	12
50 " 60	1	2	3
60 and upwards	2	1	3

With the view of ascertaining whether imprisonment here has anything to do with the disease, a column is given to show whether it occurred *before* or *after* admission.

Epileptic before admission, ...	56
" after, " 	2
" doubtful, " 	1

One of the *after* cases was a female, whose first epileptic seizure known took place while she was in a dark punishment cell, under excitement and rage. The history of the two others is not known.

The causes in a large number of cases were congenital, traceable to constitutions depraved and deteriorated by hereditary and habitual vices, and probably early neglect by vicious parents; the causes were frequently unknown.

Congenital causes were traced in	22
Intemperance, ...	3
Mental anxiety and excitement, ...	3
Injury to head, ...	1
Brain disease, fatal in, ...	2

The offences of which these prisoners were convicted are such as belong in common to the criminal population, viz., theft, robbery, and previous conviction. But assaults with violence are peculiar to a number of the cases. This might be anticipated from the irritable temper and wayward dispositions, with enfeebled intellect and will, which afflict such patients, especially about the period when they have epileptic attacks. As a class, they are found fully as troublesome, violent, and requiring restraint, as our criminal insane. They often threaten and strike officials and fellow-prisoners. No. 12, J. C., from habits of intemperance, has been in this prison four or five times, and always for assault. No. 16, A. B., was imprisoned for mobbing and assault, and he was, in my opinion, altogether unaccountable for his acts. No. 17, 21, and others, belong to the same category.

The number of times these prisoners have been in the General

Prison is nearly correct, but most of them had previously been very often in local prisons, of which we have no account.

We state the times in the General Prison :—

34	have been	once,
17	„	twice,
8	„	thrice or oftener.

We come now to the last column of our table, and the most important, viz., the mental condition of these 59 criminal epileptics. My remarks, of course, were limited to the means of observation had during imprisonment, the prisoners being visited by myself at least twice a-week, but under careful supervision of other officials. The details under this head give a sorry and hopeless picture of mental weakness and imbecility, with temporary attacks of insanity. The whole 59, with the exception of 14, have been noted for various grades of intellectual weakness, imbecility, or temporary insanity. In 7 there had been here, or before imprisonment, attacks of acute mania and temporary insanity. Many of them were subject to excitement, with threatening and violence offered to officers or fellow-prisoners, and these mental conditions were most common previous to or soon after an epileptic seizure.

It remains after this analysis of the table of criminal epileptics to consider the deductions legitimately to be drawn from the statement of facts.

1. A cursory glance at the details will satisfy any one that our criminal epileptics are in a very low mental condition, and this, accompanied by frequent fits, entirely disqualifies them from pursuing any regular trade or honest calling. They are, therefore, physically and psychically to be regarded as entitled to public care and treatment, and, in a judicial point of view, cannot be looked upon as common criminals.

2. It must also be evident that they cannot be subjected to the ordinary prison discipline; separation, with strict seclusion in a cell, is a prison rule inapplicable to them. The epileptic may die suddenly from a fit, as happened in one of the English prisons a few weeks before I visited it. The prisoner having taken a fit during the night, was found suffocated in the morning, having fallen upon his face. Besides, their mental condition is such as to require association, to save them, under the separate system, lapsing into insanity. Accordingly, the practice has been here for many years to have an epileptic ward, on a ground-flat, where all are associated; also, an airing-ground for themselves, under supervision day and night of a prison nurse and officials. They are kept apart entirely from other prisoners; as it is admitted that some peculiar minds are liable by sympathy and imitation to take on epilepsy. Farther, on liberation every epileptic is escorted to the place whence he came, and put under the charge of friends, and if he or she has none, the patient is given over to the inspector of poor. All this peculiar care and treatment of criminal epileptics of course involves the

country in considerable expense; and, taking into consideration their hospital treatment, and relief from prison discipline, the question is a grave one, whether a prison is a fit and proper place for their reception and reformation.

3. Another very serious impression must be made by these details of mental disease in criminal epileptics, that many of them at certain times cannot be held responsible for their acts. Against only about one-fourth of the 59 reported upon could no imputation be made in regard to partial unsoundness of mind. The medical profession must be satisfied generally upon this subject, but the legal mind, we think, must also be convinced that many epileptics are not accountable for their conduct. I know that many of my patients have done acts during their semi-conscious state, about the time of their fits, for which they could not be held responsible. M. Trousseau, and others, have indeed asserted that sudden impulses without motive leading to homicidal acts under transitory mania are to be explained by the existence of unrecognised epilepsy. This seems to me an exaggerated statement, but may be partially, although not absolutely true; at all events, I believe that the mental condition of criminal epileptics is not normal, and in most instances ought to extenuate in the eyes of medical jurists the sufferer's responsibility.

My corollary from the whole statement is, that criminal epileptics cannot be looked upon as fit to battle with the business of life so as to earn an honest livelihood; that many of them, from want of public surveillance, must fall into crime; that prison life cannot benefit them morally; and that it would be well for the legislature and the authorities to consider whether a SANATARIUM might not be the fittest place for this class of criminals for the sake of civic economy and true Christian sympathy.

With such opportunities as I have, I ought to say something of the results of my medical treatment of epileptics. I have elsewhere given an account of my medical treatment by some reputed drugs, but I have been driven to rely chiefly on diet, regimen, and the general principles of hygiene, and acknowledge that epilepsy has in my hands justified its appellation, "*opprobrium medicinae*."

ARTICLE IV.—*Researches on the Daily Excretion of Urea in Typhus Fever, with Remarks.* By KEITH ANDERSON, M.D., Resident Physician, Royal Infirmary, Edinburgh; President of the Royal Medical Society.

GREAT importance has been attached to the amount of urea formed and excreted in fevers: 1st, In regard to the prevalent doctrine, that fever consists essentially in an increased metamorphosis of tissue; and, 2d, In regard to the question, how far the comatose or head

symptoms of fever are due to a retention of urea in the system (uræmia). Although these points have been made the subject of chemical investigation in the typhoid or enteric fever, chiefly on the Continent, they have not been hitherto sufficiently tested by similar observations in the more common fever of Britain, the epidemic typhus. The only analyses of the *whole* urine, in cases of typhus, which I have been able to find, are those of one case by Dr Parkes,¹ and, since my own observations were made, of two by Mr Dunnett Spanton.² The analyses of urine in so-called typhus, published by Becquerel and others, there is every reason to suppose, refer in reality to examples of typhoid fever. Barralier³ has published analyses of the urine in several cases of true typhus; but the experiments were made on limited quantities of the urine, and the amounts excreted daily are not given. It is obvious, therefore, that there is a deficiency of accurate knowledge, based on observation, of the relations of urea to typhus fever. To supply this want, in some degree, was my object. I have confined myself to the determination of the amount of urea excreted daily in six cases of undoubted typhus, presenting the characteristic eruption, and I have omitted several cases in which the diagnosis was doubtful. The total amount of the daily urine was carefully collected, measured, and analyzed for urea by Davy's process.⁴ My best thanks are due to Dr Sanders for his kindness in permitting me to make the observations on his patients in the Royal Infirmary; to Professor MacLagan, for the valuable privilege of performing the analyses in his laboratory; and to Dr Arthur Gamgee, to whom I am indebted for my knowledge of the chemical processes required.

The general results of the inquiry have been that, *in all of the cases the quantity of urea excreted daily, during the second week, was decidedly below the standard of health*, notwithstanding that the patients were in a state of high fever, with the temperature and the pulse much above the normal rate. These results differ markedly from those of Dr Parkes' analyses, and are opposed to the belief which many entertain of an increased excretion of urea in typhus. With regard to the daily excretion, during the first week, nothing definite could be made out, as the patients entered hospital at too late a period of the fever to admit of a sufficient number of observations. In three of the cases, however, viz., Cases 1, 4, and 6, the excretion of urea was found to be comparatively high at the end of the first week, but not above the normal amount.

The cases may be divided into two classes; one, in which the crisis occurred on the fourteenth day, while the nervous symptoms continued to some extent for a day or two longer; the other, in

¹ Medical Times and Gazette, 1857, Feb., and Parkes on Urine.

² Medical Times and Gazette, 1864.

³ Du Typhus Epidémique à Toulon.

⁴ London, Edinburgh, and Dublin Phil. Mag., June 1854. Also, Beale on Urine, etc., 2d Edition, p. 21.

which the crisis took place by a process of gradual improvement, beginning about the tenth or eleventh day, and completed about the fourteenth. To the first class belong Cases 1, 2, 4, and 5; to the second, Cases 3 and 6. In the *first* class, an increased discharge of urea took place at, and immediately after, the crisis. Subsequently to the occurrence of this increased discharge, the daily excretion of urea fell to a very low amount, and then rose gradually as the patient improved in health. In the *second* class, the nervous symptoms abated about the tenth or eleventh day, after an increase in the excretion of urea. The completion of the crisis on the fourteenth day was followed by a fall in the amount of urea excreted daily, which then gradually rose with the return of health, as in the other class. To illustrate the distinction between the two classes, I have appended a diagrammatic view of the range of the daily excretion of urea in a specimen of each class.

In his Gulstonian Lectures on Pyrexia, Dr Parkes has shown that the febrile condition is attended by increased metamorphosis of tissue, and, in consequence, by an augmented formation of urea. It also appears from his observations that this increased formation of urea is not necessarily followed by an increase in the quantity eliminated, but that, in certain circumstances, a considerable amount of urea may be retained in the system. In my cases, such a retention seems to have taken place, since an increase of urea was observed, as before mentioned, either on the occurrence of crisis, or on the appearance of the first symptoms of improvement. That this increased excretion was due to a previous retention, and not to a more active tissue-metamorphosis, consequent on the improvement in the condition of the patient, is rendered probable by the fact, that the excretion of urea was greatly lowered after the sixteenth or seventeenth day, and rose in amount exactly in proportion to the rapidity with which the patients regained health and strength; and also by the fact of the excretion having been very low for some time previous to its increase.

It is generally admitted that in Bright's disease and cholera, and in many other affections, stupor, delirium, and other nervous symptoms of a low or so-called "typhoid" character, are due to a retention of urea. In regard to the head symptoms of typhus, a similar explanation has been proposed. In my cases, the fact of those symptoms being most intense while the urea-excretion was lowest, and of their improvement coinciding with, or following on, an increase in that excretion, appears to favour this view very strongly. In two of the cases in which the increased elimination took place on the days immediately following the fourteenth, the nervous symptoms continued to a considerable extent during those days, although the fourteenth had evidently been the critical day. In the cases in which the increased elimination took place before the fourteenth day, no delirium was observed after the occurrence of the increased discharge. The probability of this theory is strength-

ened by the fact that the other symptoms retained their febrile character, in the latter class of cases, till the ordinary period of crisis.

On the other hand, it may be argued that the retention of urea was merely a consequence of the abnormal condition of the nervous system, and not in any way a cause of that condition. This view would appear to derive support from my cases, in so far as, in the majority of them, the increased elimination did not take place till *after* the crisis had begun; but I think this support is more apparent than real, as, in half of these cases, the nervous symptoms continued to a considerable extent during the occurrence of the increased elimination, and did not finally disappear till the urea had fallen to the low standard natural in the enfeebled state of the patients.

It is very difficult to say why, failing organic lesion of the kidneys, urea should be retained, or why it should be retained to a greater extent in some cases than in others. Perhaps the retention is due to a defective innervation; the theory at present entertained, that in fevers there is a degree of paralysis of the nervous system, gives countenance to such a view.

In submitting the facts observed, I shall give a short abstract of each case, followed by the detailed statement of the condition of the urine, which I have put in a tabular form for convenience of reference and comparison. Details of the daily symptoms of the cases are, for the sake of completeness, added, in the form of an appendix, at the end of the paper.

I have calculated the total solids by an empirical formula of Dr Christison,¹ the results of which are a sufficiently close approximation to the real amount.

Before giving the results of my analyses of the urine, it may be well to note the amount of urine and of urea which is considered to be natural to adults in a state of health. Parkes² estimated the normal quantity of urine at about $52\frac{1}{2}$ ounces, and of urea at 33.18 grammes, or 512.4 grains, and, although other writers have calculated both at a much higher rate, this estimate may be assumed as sufficiently accurate for comparison. Women excrete less both of urine and of urea than men. Parkes estimates the average excretion of urea in women at from 16 to 28 grammes (246.9 to 432.1 grains) per diem, but says that it may vary from 12 to 30 grammes (185.2 to 463 grains).

CASE 1.—A domestic servant, æt. 19, admitted on the third day of the fever. Her case was a serious one, the nervous symptoms being very severe, as shown by extreme restlessness, panting respiration, moaning, and crying, during the first week; and afterwards by an extreme degree of oppression and stupor, by very violent tremors and subsultus of both hands and feet, and by retention of urine at one time, and its involuntary discharge at another. Convalescence occurred on the fourteenth day, but a considerable degree of oppression and stupor remained for two days after that

¹ Tweedie's Lib. of Med., vol. iv. p. 248.

² Parkes on Urine.

date. The pulse was remarkably low during the second week of the fever.

Days of the Fever.	Urine.		Urea.		Sp. Gr.	Reaction.	Estimated Total Solids in Grains.
	In Cubic Centimetres.	In Oz.	In Grammes.	In Grains.			
5	580	20·4	27·37	422·31	1026·0	Neutral.	554·4
6	915	32·2	5·79	89·36	1028·6	Neutral.	944·4
7	600	21·1	5·31	81·95	1026·9	Neutral.	573·4
8	585	20·6	4·04	62·35	1023·7	Neutral.	493·8
9	1240	43·7	5·31	81·95	1025·4	Slightly acid.	1141·4
10	845	29·8	3·39	52·32	1020·2	Slightly acid.	619·5
11	Urine not collected.						
12	540	19·0	1·17	18·05	1018·5	Acid.	354·7
13	820	28·9	4·29	66·21	1023·5	Slightly acid.	662·0
14	640	22·5	2·68	41·36	1024·9	Acid.	563·6
15	Urine not collected.						
16	1170	41·2	18·69	288·46	1020·2	Acid.	856·5
17	520	18·3	2·28	35·18	1018·6	Neutral.	341·6
18	760	26·8	8·47	130·72	1019·6	Acid.	528·7
19	965	34·0	8·51	131·34	1018·3	Neutral.	634·7
20	1150	40·5	9·74	150·32	1013·7	Slightly acid.	543·5
21	780	27·5	11·24	173·47	1016·6	Acid.	455·6
22	1540	54·3	20·73	319·94	1015·3	Neutral.	842·7
23	1500	52·9	16·19	249·87	1014·5	Acid.	765·4
28	1670	58·9	23·45	361·89	1018·1	Slightly acid.	1099·6

CASE 2.—A labourer, æt. 30, a strong, tall man. He was admitted to hospital on the eighth day of the disease. The case was one of moderate severity. Convalescence commenced on the fourteenth day, but the typhoid stupor continued, to some extent, for a day or two longer.

Days of the Fever.	Urine.		Urea.		Sp. Gr.	Reaction.	Albumen.	Estimated Total Solids in Grains.
	In Cubic Centimetres.	In Oz.	In Grammes.	In Grains.				
9	1145	40·3			1020·7	Neutral.		837·8
10	585	20·6	10·04	154·95	1019·5	Neutral.	Considerable.	406·4
11	745	26·2	6·49	100·16	1015·8	Neutral.	Considerable.	375·6
12	Urine not collected.							
13	835	29·4	2·66	41·05	1015·8	Slightly acid.	Considerable.	456·2
14	950	33·5	2·27	35·03	1017·7	Acid.	None.	590·2
15	1450	51·1	15·89	245·24	1014·8	Acid.	None.	739·4
16	1340	47·2	12·63	194·93	1012·3	Acid.	None.	585·8
17	1450	51·1	16·69	257·59	1014·1	Acid.	None.	739·4
18	1460	51·5	5·37	82·88	1020·2	Acid.	None.	1070·6
19	960	33·7	4·71	72·69	1023·0	Acid.	None.	808·1
20	1050	37·0	10·42	160·82	1023·7	Acid.	None.	887·2
21	1225	43·2	10·72	165·45	1023·2	Acid.	None.	1035·9
22	1260	44·4	17·18	265·15	1020·9	Acid.	None.	923·0
23	1585	55·9			1019·0	Acid.	None.	1102·9
24	1800	63·4	Urine not examined.					
27	2315	81·6	37·29	575·53	1017·1	Acid.	None.	1437·7
32	2640	93·1	36·46	562·72	1016·4	Feebly alkaline.	None.	1542·6

CASE 3.—A labourer, æt. 40, a tall, very powerful man. This case was a mild one in many respects, the patient being singularly intelligent for a case of typhus, uniformly sleeping well, and never having any nearer approach to delirium than a slight wandering of the mind, and that on only a few occasions. On the other hand, the pulse was rapid, the temperature high, and there was considerable subsultus from the time of his admission (eighth day) till an advanced period of convalescence. The case was one of those in which a gradual improvement was observed for several days before the actual occurrence of crisis. He convalesced on the fourteenth day.

Days of the Fever.	Urine.		Urea.		Sp. Gr.	Reaction.	Estimated Total Solids in Grains.
	In Cubic Centimetres.	In Oz.	In Grammes	In Grains.			
8	540	19.0	6.82	105.2	1023.3	Acid.	455.6
9	920	32.4	13.88	214.2	1021.3	Acid.	707.9
10	1540	54.3	23.92	369.1	1015.1	Acid.	842.7
11	1270	44.7	21.56	332.7	1015.1	Acid.	693.7
12	790	27.8	11.70	180.5	1024.3	Acid.	696.3
13	650	22.9	11.58	178.7	1026.4	Acid.	622.4
14	700	24.6	10.74	165.7	1022.6	Acid.	563.5
15	600	21.1	7.94	122.5	1025.6	Acid.	551.1
16	770	27.1	13.09	202.0	1024.7	Slightly alkaline	678.8
17	850	29.9	20.76	330.4	1023.5	Slightly acid.	690.0
18	850	29.9	11.20	172.8	1023.9	Acid.	690.0

CASE 4.—A baker, aged 14, a very weakly lad, who had just recovered from typhoid fever, having been up only a fortnight when he took typhus. The case was by no means a very severe one. He slept tolerably well, and the nervous symptoms were not strongly developed. He convalesced on the fourteenth day.

Days of the Fever.	Urine.		Urea.		Sp. Gr.	Reaction.	Albumen.	Estimated Total Solids in Grains.
	In Cubic Centimetres.	In Oz.	In Grammes	In Grains.				
5	740	26.1	7.66	118.2	1025.2	Acid.	None.	681.7
6	450	15.8	4.02	62.0	1027.1	Acid.	Slight.	446.4
7	920	32.4	21.7	335.0	1020.0	Acid.	Slight.	673.5
8	655	23.1	9.31	143.6	1015.1	Acid.	Considerable.	364.5
9	395	13.9	4.46	68.8	1013.8	Neutral.	None.	186.5
10	370	13.0	4.32	66.6	1013.0	Neutral.	None.	174.4
11	590	20.8	7.67	118.3	1010.6	Feebly acid.	Considerable.	338.2
12	835	29.4	13.15	202.9	1010.9	Neutral.	Considerable.	302.2
13	650	22.9	8.86	136.7	1020.1	Neutral.	None.	416.0
14	Urine not collected.							
15	990	34.9	15.68	242.0	1012.3	Alkaline.	None.	431.7
16	850	29.9	14.72	227.1	1013.1	Alkaline.	Considerable.	401.2
17	390	13.7	5.87	90.5	1013.0	Acid.	Very slight.	183.8
18	630	22.2	10.14	156.5	1012.8	Acid.	None.	274.6
19	750	26.4	13.08	201.8	1013.1	Acid.	None.	360.2

In a case like the above, where the patient was weak and emaciated from a previous illness, one would expect a much lower excretion of urea than in a previously healthy, robust man. The age of the patient (14) must also be considered. From two analyses, one by Uhle, the other by Bischoff,¹ it would appear that the natural excretion of urine in boys from 13 to 16 years of age, in a state of health, is about 25 or 26 ounces, and of urea, from 180 to 190 grains. These analyses are, however, too few in number to be absolutely relied upon. Few researches seem to have been made to ascertain the influence of youth on the quantity of urine and of urea excreted daily in a state of health. I have failed to find the record of any other analyses bearing on this point than the two quoted above.

CASE 5.—A tailor, aged 46, of a somewhat delicate constitution. This case was, towards the crisis, complicated with pneumonia. After the twelfth day the urine became alkaline and deposited phosphates, and this condition continued till the patient had fully regained health. He convalesced on the fourteenth day, but the stupor and other nervous symptoms continued for a day or two longer. The pulse was unusually low during the fever. The temperature varied from 103° to 104° Fahr.

Days of the Fever.	Urine.		Urea.		Sp. Gr.	Reaction.	Estimated Total Solids in Grains.
	In Cubic Centimetres.	In Oz.	In Grammes.	In Grains.			
8	570	20·1	4·85	74·85	1027·6	Acid.	568·0
9	545	19·2	9·80	151·2	1026·2	Acid.	521·8
10	835	29·4	15·59	240·6	1025·5	Acid.	767·9
11	1050	36·9	...	...	1031·3	Acid.	1201·8
12	900	31·7	15·95	246·1	1017·8	Acid.	558·5
13	1230	43·3	18·38	283·6	1019·9	Alkaline.	854·3
14	910	32·0	14·8	229·6	1019·6	Acid.	631·3
15	1150	40·5	24·4	377·3	1020·3	Alkaline.	841·9
16	890	31·3	...	...	...	...	...
17	1020	35·9	17·05	263·1	1020·4	Alkaline.	746·3
18	925	32·6	11·22	173·1	1015·4	Alkaline.	505·9
19	965	34·0	11·43	176·4	1020·8	Alkaline.	706·8
20	845	29·8	15·80	243·8	1028·1	Alkaline.	874·0
21	655	23·1	11·64	179·6	1026·2	Alkaline.	627·8

CASE 6.—A baker, aged 20, a stout young man. This was a very mild case, in which crisis occurred imperceptibly about the fourteenth day. Previous to that period, however, the symptoms underwent a marked improvement. The temperature continued high till the fifteenth day. The urine was more or less alkaline during the whole progress of the case. In this case, wine was administered towards the crisis.

¹ Referred to in Parkes on Urine, 1860, p. 44.

Days of the Fever.

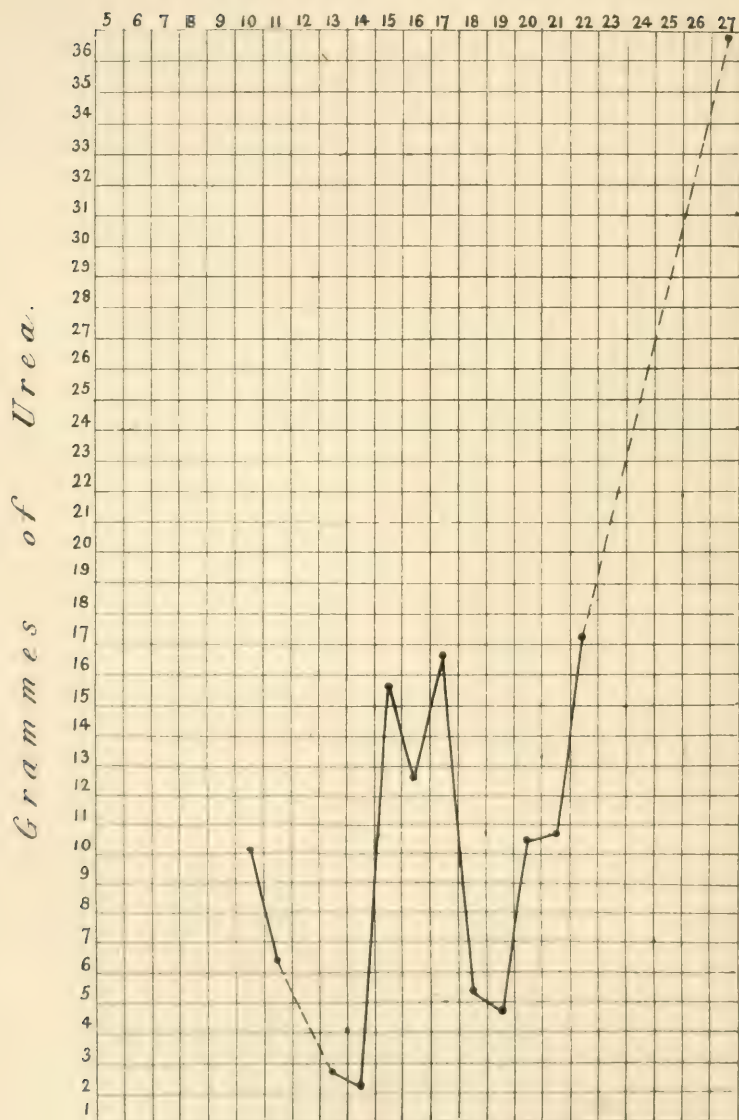


DIAGRAM OF THE DAILY EXCRETION
OF UREA IN CASE II.

Days of the Fever.

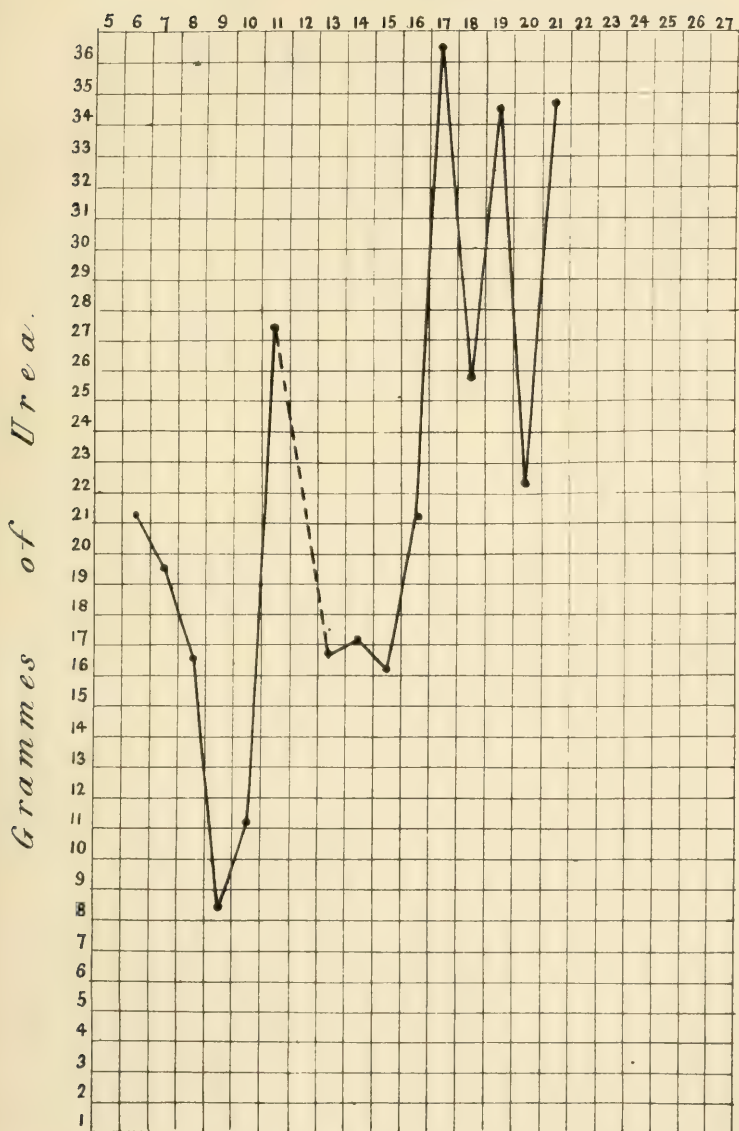
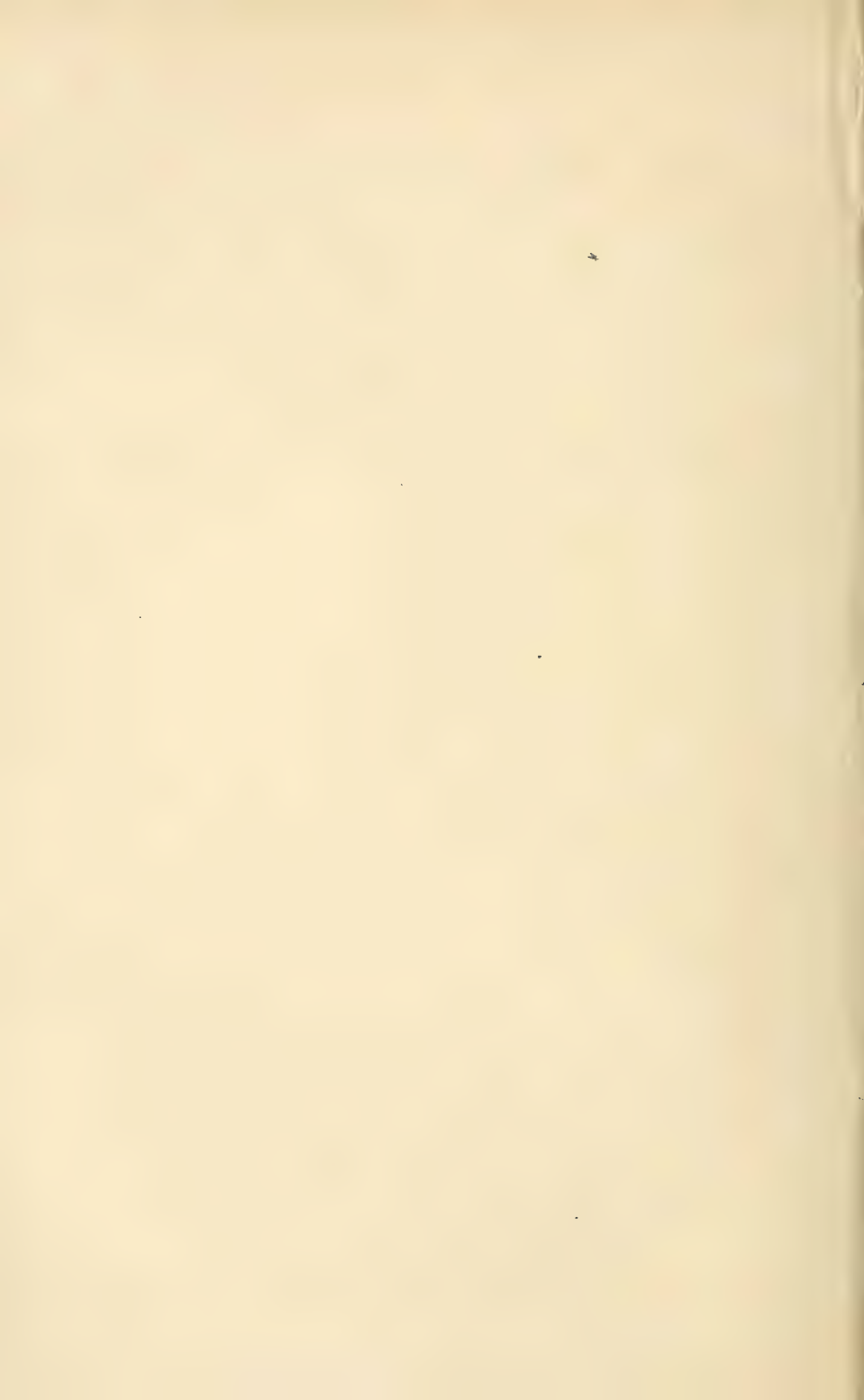


DIAGRAM OF THE DAILY EXCRETION
OF UREA IN CASE VI.



Days of the Fever.	Urine.		Urea.		Sp. Gr.	Estimated Total Solids in Grains.
	In Cubic Centimetres.	In Oz.	In Grammes.	In Grains.		
6	1260	44.4	21.26	328.1	1027.4	1254.7
7	1190	41.9	19.62	302.8	1031.5	1364.6
8	1050	36.9	16.65	256.9	1025.2	963.8
9	795	28.0	8.40	129.6	1033.5	1021.0
10	1500	52.9	11.19	172.7	1033.9	1785.3
11	1435	50.6	27.40	422.8	1020.9	1051.9
12	Urine not collected.					
13	1185	41.8	16.85	260.0	1023.4	1002.3
14	1045	36.8	17.15	264.6	1024.7	921.8
15	950	33.5	16.13	248.9	1024.1	859.2
16	1190	41.9	21.43	330.7	1025.8	1091.8
17	2190	77.2	36.42	562.1	1015.9	1198.1
18	2070	73.0	25.94	400.3	1017.4	1286.2
19	2320	81.8	34.59	533.8	1017.3	1441.3
20	1830	64.5	22.29	344.0	1013.8	865.5
21	2150	75.8	34.95	539.4	1017.1	1345.5

APPENDIX OF DAILY NOTES OF CASES.

CASE I.—*3d day, Evening.*—Temp. 105°. Eruption abundant ; panting respiration ; tongue moist, coated with white fur ; severe headache.

4th day.—Temp. 103°. Pulse 106, soft and of fair strength ; a few wheezing râles over the chest ; tongue red, with white fur ; eyes suffused, pupils large ; sight and hearing impaired ; headache gone ; very restless all night, moaning and crying ; a considerable degree of oppression.

5th day.—Pulse 104 ; tongue dry in centre, white at edges, yellow at back ; restless through night, but slept towards morning ; delirious and wandering during the night.

6th day.—Pulse 106 ; slight subsultus ; stupid, but understands when spoken to ; inclined to sleep ; restless and wandering through the night, and sometimes did not know the nurse. Had retention, and urine was drawn off by catheter.

7th day.—Pulse 106 ; slept last night, after two doses of tinct. hyoscyami, 3j each ; restless and delirious towards morning.

8th day.—Pulse 90 ; tongue dry, and brown in centre, white at edges ; slept at beginning of night, but talked and “rambled” later ; now (1 P.M.) inclined to sleep ; subsultus of hands ; unwilling to speak ; takes nothing but milk and water.

9th day.—Pulse 90 ; breathing easier ; tremors of hands. Had 3i of tinct. hyoscyami last night. Very restless all night ; talking and “rambling” much ; retention of urine this morning ; understands when spoken to.

10th day.—Pulse 80, of fair strength ; breathing occasionally hurried ; tongue dry, brown, and fissured in centre, moist at the sides, sordes on teeth ; inclined to sleep ; very restless and delirious during the night ; hands tremulous ; complains of headache. Had a draught of 40 minims of tinct. hyoscy. with 20 minims of sol. mur. morph. last night ; takes only a very little gruel.

11th day.—Pulse 74 ; nervous “sniffling” breathing ; very restless, wishing to get out of bed ; hands and arms trembling ; fumbling at bed-clothes ; sobbing occasionally ; understands when spoken to ; passing urine in bed.

12th day.—Pulse 84 ; tongue still dry in centre, but the moistness of the edges appears to be extending ; less sordes on teeth and lips ; was wildly delirious last night, and very anxious to get out of bed ; still much nervous excitement, her hands trembling and her manner agitated ; speaks sensibly enough,

but is very confused; says she has a difficulty in speaking; appears to be getting the expression of convalescence.

13th day.—Skin moist; eruption gone; sudamina on the skin; tongue still dry and brown; no headache; dreamed a great deal; muttered much during the night; languid and stupid, but answers correctly; tremors and subsultus of hands and feet, and twitching of facial muscles; slept very little during the night. Ordered 10 grains of nitrate of potash, three times a-day. Eating very little.

14th day.—Pulse 88, soft and full; skin moist; tongue dry, glazed at edges; pupils dilated; inclined to sleep; cries out at times; more stupid, and does not answer questions; tremors and subsultus rather less than before; had a drachm of tinct. hyosey. last night.

15th day.—Pulse 80; tongue dry, and glazed at tip, and still fissured; very languid, and does not answer questions; muscular tremors almost gone; inclined to sleep; rather quiet in first part of night, afterwards restless; passed urine in bed.

16th day.—Pulse 72; tongue moistening, still fissured; bowels constipated; slept well, and is inclined to sleep; is very confused, and contradicts herself when answering questions.

17th day.—Pulse 76; tongue moist, and covered with white fur, still fissured in the centre; no tremors; sleep disturbed by a purgative; still very languid; taking a little arrowroot and beef-tea.

18th day.—Pulse 76; tongue getting clean, it is moist and whitish; did not sleep well on account of repeated action of bowels; quite conscious and intelligent; eating rather more.

19th day.—Pulse 70; tongue moist; slept well; appetite improving.

20th day.—Pulse 83; tongue moist, whitish; bowels rather costive; somewhat restless during the night.

She continued rapidly to improve from this time, regaining her appetite and strength very quickly.

CASE II.—*9th day.*—Temp. 105°. Pulse 107; slept very little, but was not delirious; slight tremors of hands; great thirst and no appetite.

10th day.—Temp. 101°. Pulse 110. Other symptoms as before.

11th day.—Temp. 100°. Pulse 112, small and weak; great headache; slight tremors of hands; slept pretty well, but was delirious during the night; answers intelligently; eats very little.

12th day.—Temp. 104°. Pulse 108, weak; has a very stupid typhous appearance, but answers correctly; delirious during the night.

13th day.—Temp. 104°. Pulse 104; heart sounds weak; severe headache; inclined to sleep; delirious through the night; right pupil larger than left; answers correctly.

14th day.—Temp. 104°. Pulse 96; quieter during the night; very confused, but answers correctly, though slowly; severe headache; not so thirsty; decubitus on side (previously on back).

15th day.—Appearance much improved. Temp. 102°. Pulse 96, fuller and softer than before; pain in head; slept well, and was not delirious.

16th day.—Temp. 100°. Pulse 84; tongue moist; right pupil still dilated, but less so than before; slept well; expression bewildered.

17th day.—Temp. 98°. Pulse 70; no headache; giddy when he sits up; right pupil still dilated; eating more.

18th day.—Temp. 99°. Pulse 76; slept well; left pupil larger than right; eating well.

19th day.—Temp. 98°. Pulse 66; slept very well; head quite clear, wishes to rise. Diet full, with the exception of meat.

From this date he rapidly improved.

CASE III.—*8th day.*—Eruption fully out and very copious; pulse 135, weak; tongue moist, coated with yellow fur; bowels loose (from purgatives); very

thirsty; no appetite; slight subsultus; severe frontal headache; confused, but answers correctly.

9th day.—Pulse 130; tremors and subsultus of hands; no delirium; headache as before; slept pretty well; eating very little, but drinking much.

10th day.—Temp. 103°. Pulse 128, "wandering" a little, but intelligent at visit; tongue dry, glazed at tip, brown and furred at back; bowels regular; very thirsty.

11th day.—Temp. 104½°. Pulse 140; great tremors and subsultus; bronchitic râles over both sides of chest; headache on coughing; tongue dry and brown; bowels regular; slept pretty well; no delirium. Ordered 8 oz. of wine per diem.

12th day.—Temp. 103°. Pulse 110; great tremors and subsultus; no delirium observed; tongue dry, brown, and fissured; bowels regular; slept well.

13th day.—Temp. 103½°. Pulse 120; cough troublesome; sputum streaked with blood; quite intelligent; no delirium; slept very well; eating little.

14th day.—Temp. 100°. Pulse 100; tongue moist, with yellow fur; slept very well; quite intelligent; eating considerably more.

15th day.—Temp. 100°. Pulse 92; bronchitis much better; slept very well; still tremors; tongue moist.

16th day.—Temp. 98°. Pulse 80; tremors less; tongue moist, furred; bowels regular; eating pretty well.

17th day.—Temp. 99°. Pulse 80; cough almost gone; tremors nearly gone; tongue moist, and nearly clean.

18th day.—Temp. 98½°. Pulse 98; very slight tremors; bowels loose.

He continued steadily to improve, and soon regained his strength.

CASE IV.—5th day.—Temp. 104°. Pulse 112; eruption distinct; tongue dry in centre, with white creamy fur; slept well; no delirium.

6th day.—Temp. 105°. Pulse 112; sleeping well; no delirium; taking a fair amount of nourishment.

7th day.—Temp. 104°. Pulse 108; tongue moist, yellow, fissured in centre; slept well; eating very little.

8th day.—Temp. 103°. Pulse 110; slept well, but was delirious during the night; delirious at visit.

9th day.—Temp. 103°. Pulse 104. Had a draught of tinct. hyoscyami last night, and slept well, and without delirium; eating little.

10th day.—Temp. 103°. Pulse 110; slept well, but was somewhat delirious; eating rather more.

11th day.—Temp. 102°. Pulse 100; slept well; no delirium.

12th day.—Temp. 103°. Pulse 116; slept well; no delirium observed; eating better; bowels regular.

13th day.—Temp. 102°. Pulse 96; slept well; appearance much improved, eating pretty well.

14th day.—Temp. 102°. Pulse 86; slept well; eating well; tongue moist, whitish, fissures nearly gone.

15th day.—Temp. 100°. Pulse 82; doing well. He continued to improve.

CASE V.—7th day.—Eruption copious; temp. 104°; pulse 95; tongue moist, with white fur; no appetite; great thirst; bowels loose (from purgatives).

8th day.—Temp. 103°. Pulse 95; bronchitic râles over chest; severe headache; slept pretty well; tongue dry, with yellowish white fur.

9th day.—Temp. 104°. Pulse 96; headache not so bad.

10th day.—Temp. 103°. Pulse 100; did not sleep well; headache bad; delirium during the night.

11th day.—Temp. 104°. Pulse 96; slept well; headache bad; delirious during the night; had retention of urine; tongue dry and brown.

12th day.—Temp. 103°. Pulse 100; headache still severe; tremors and subsultus; slept well.

13th day.—Eruption distinctly petechial; temp. 103° ; pulse 88; slept well; bad headache; tongue dry and brown; eating very little.

14th day.—Temp. 103° . Pulse 88; headache better; tremors and subsultus; pupils contracted; slept pretty well; tongue moist, getting clean.

15th day.—Temp. 104° . Pulse 84; bronchitic râles over chest, and crepitation at right base; slept pretty well; beginning to show appetite.

16th day.—Pulse 76, of fair strength; physical signs much the same; slept well; headache on coughing; tongue moist, with yellow fur; bowels regular; eating little.

He gradually improved from this date, making, however, a very slow recovery.

CASE VI.—6th day.—Eruption distinct; temp. 103° ; pulse 84; severe headache; quite intelligent; tongue moist, with yellowish white fur; great thirst; bowels loose (from purgatives).

7th day.—Temp. $105\frac{1}{2}^{\circ}$. Pulse 92; headache rather better; no delirium; bowels still loose.

8th day.—Temp. $105\frac{1}{2}^{\circ}$. Pulse 92; slight cough; no headache or subsultus; slept well, but was slightly delirious during the night.

9th day.—Temp. 105° . Pulse 92; slept pretty well; low, muttering, stupid delirium.

10th day.—Temp. 104° . Pulse 116, very weak; answers correctly, but is very confused; delirious in early part of night; very thirsty.

11th day.—Pulse 104; very confused, but answers correctly; tongue moist, brown in centre; delirious in first part of night.

12th day.—Temp. $103\frac{1}{2}^{\circ}$. Pulse 87; slight subsultus; slept well; no delirium; tongue dry, fissured in centre; ordered $\frac{3}{4}$ vi. wine daily.

13th day.—Temp. 103° . Pulse 80; no subsultus; intelligent; slept well; tongue dry and brown all over; eating rather more.

14th day.—Temp. 102° . Pulse 76; slept well; tongue moistening.
He improved rapidly from this date.

ARTICLE V.—*Address on the Recent Progress of Chemistry*, delivered by Dr STEVENSON MACADAM, at the *Conversazione* of the Royal College of Surgeons held on the evening of Thursday, 14th December 1865.

THE progress of every department of science at the present day is so rapid that it becomes a difficult task for even professional men to keep pace with the times; and this remark applies with greater force to the science of Chemistry than probably to any other.

No branch of knowledge has made more rapid progress in modern times; and, connected as Chemistry undoubtedly is with many other departments of science, such as Physiology, Geology, and Botany, and with the majority of the arts and manufactures, it will be at once apparent that a review of the progress of Chemistry, which would embrace all its ramifications, would be a lengthy and laborious task. My duty this evening will be to lay before you such a resumé as will enable you to observe in a general way how rapid the steps and how lengthened the strides have been which Chemistry has taken within the last few years.

In the prosecution of the science, and the adaptation of the dis-

coveries which are made to the benefit of mankind, three classes of workers are diligently pursuing their labours.

First, Those who have time and means at their disposal to make researches into unknown regions, and who elaborate therefrom new compounds and new doctrines;

Second, Those whose special province it is to arrange the discoveries made, and by their teachings to impart a knowledge of the facts and principles of the science to others in a clear and tangible form; and,

Third, Those who, connected with the arts and manufactures of the country, seize hold of new things, and practically work them into material specially suitable for the age we live in.

The present time is particularly applicable for a discourse on the progress of Chemistry, not only on account of the many discoveries of new substances, and their novel applications in the arts, but also from the fact, that we are on the threshold, or rather, I should say, we have but lately passed the threshold of a change in the symbolic representation of chemical substances, and the groups they form, which must lead to much change in our chemical treatises, our chemical teaching, and our chemical examinations.

The new views have an important bearing on the advance of the science, and they already occupy a conspicuous part in the representation of the constitution of compound substances. I will take occasion, at the close of this discourse, to refer shortly to the more important doctrines recently being worked out, but which are apparently still merely the germ of what will follow, so that, as the change proceeds, and the new views germinate and are developed, we may be prepared in some measure for them.

During the last few years, two new modes of analysis or methods for the investigation of substances have been discovered, and have been applied to the examination of materials. One of these processes is the separation of chemical substances from each other, by allowing solutions containing such to pass through a diaphragm or septum, and has been styled Dialysis; whilst the other method of research depends upon the production of certain lines or bands by the various elementary bodies, when these or their compounds are burned in a flame, and the light is then decomposed by a prism, so that the spectrum is obtained. The latter mode of research is termed Spectrum Analysis.

The various researches in Diffusion Analysis have been worked out by Professor Graham, Master of the Mint; and the well-known researches undertaken by that distinguished chemist in regard to the diffusion of gases, supplied us years ago with the laws regulating the passage of the various gases through each other, either in a common atmosphere or through porous plates. The later researches on diffusion have reference to liquids and the substances dissolved therein. Certain liquids have no power of diffusing through each other, such as mercury, water, and oil,

which, when mingled together, arrange themselves according to their densities; and thus the mercury remains at the lower part of the vessel, the water in the middle, and the oil floats above. Agitation of the liquids may cause a momentary intermingling of the substances, but subsequent quiescence will enable the mercury, water, and oil, to separate again from each other and arrange themselves as before. When water and alcohol are taken, however, and cautiously introduced into a vessel, so that the water may remain at the lower part and the alcohol is above, it is found that the two layers of liquid gradually intermingle; and in the course of time the water will rise in part through the alcohol, whilst the latter descends in part into the water layer; and at length the two substances, which were at first separate, will have become so thoroughly intermingled that there will be as much alcohol below as above, and the liquid will form a uniform mixture.

The diffusion of liquids may be observed by various arrangements, but the simplest apparatus is a tall glass jar, which is nearly filled with water, and to the lower part of which a solution of the substance to be experimented upon is carefully introduced by a long pipette. The saline matter in the lower stratum begins to rise slowly through the upper water, until in some instances it can be recognised at the summit of the column of water; whilst in other cases the process of diffusion proceeds so slowly that the material scarcely finds its way half-up the water column, in the time which has enabled other materials to reach the top layer of water. Professor Graham has made an elaborate series of observations on the whole subject; and arranging that the conditions of the experiments were similar, and the temperature as nearly as possible uniform during the continuance of the trials, he has determined the approximate time required for the same degree of diffusion of many substances through water. Hydrochloric acid was found to be one of the most rapid in its rate of diffusion, succeeded by saline substances; and the slowest in diffusing themselves throughout the water were such bodies as albumen and caramel. Thus, whilst hydrochloric acid diffuses itself throughout the upper column of water in a time represented by 1·00, chloride of sodium will take 2·33, sugar 7·00, sulphate of magnesia 7·00, albumen 49·00, and caramel 98·00, or nearly 100 times the period required for the diffusion to the same rate of hydrochloric acid. The difference in the rapidity of diffusion has suggested the possibility of separating, to some extent, substances from each other, as in a mixture of chloride of sodium and sulphate of magnesia, where the former would tend to diffuse through upper layers of water at a rate three times quicker than the latter. Professor Graham has suggested, that the substances which have a quick rate of diffusion should be called *volatile* or *crystalloids*, the latter term indicating that crystalline bodies have a ready diffusibility; whilst the substances which are tardy

in their rate of diffusion should be styled *fixed* or *colloids*, the latter term being used owing to gelatine (collin) being representative of the class. A modification of the experiments in jar diffusion may be carried out, by placing the substance to be experimented upon in a phial, and introducing such into the lower part of a tall jar containing water, when a similar series of results will be obtained to those already referred to.

The special department of diffusion which is properly termed Dialysis, has reference to the separation of substances dissolved in liquids by means of allowing them to pass through a diaphragm or septum. Various membranes will serve for the diaphragm, such as a bladder or part thereof, but the best material for the purpose is parchment paper. This material was first prepared by M. Gaine, and afterwards by De la Rue, and it is most easily formed by taking a mixture of one part by volume of water, and adding thereto exactly two volumes of oil of vitriol. When this acid mixture is allowed to cool, and ordinary bibulous or unsized paper is soaked therein, the paper undergoes a molecular change, becoming transparent to a great degree; and when washed in dilute ammonia to neutralize the acid, and thereafter in a stream of pure water and subsequently dried, the bibulous paper has been manufactured into parchment paper. This peculiar form of paper is tough even when wet, and very much resembles in general appearance a piece of ordinary parchment. The proportions of acid and water required in its preparation require to be very exact, as a larger quantity of either acid or water will destroy the paper and convert it into a pulpy substance. Instead of using oil of vitriol and water in the preparation of the parchment paper, Mr T. Taylor has suggested the use of a strong solution of chloride of zinc, which acts in the same way. In employing the parchment paper as a dialyser, the paper is placed between two hoops of gutta-percha or other material not readily acted upon, in the same manner that an ordinary sieve is constructed, or the parchment paper is firmly tied round the open mouth of a small bell jar shaped like an inverted cup, and having an opening at the upper part through which the liquid may be introduced into the bell jar or bulb.

The substance to be dialysed is placed in the upper part of the diaphragm of parchment paper, and the apparatus called the dialyser is then floated on the surface of water contained in a vessel placed below. Certain substances, especially those of a crystalline nature, such as metallic salts, arsenious acid, sugar, strychnine, morphia, and quinine, pass through the diaphragm of parchment paper into the water underneath with considerable rapidity, and the bodies of this class are styled *crystalloids*. Other materials, however, such as gelatine, albumen, pectin, animal mucus, vegetable gelose, and caramel, pass through the dialysing membrane very tardily, and are named *colloids*. As a class the crystalloids are more or less sapid to taste, whilst the

colloids are more or less insipid. When a mixture of a crystalloid, such as arsenious acid, and a colloid such as gelatinous matter, is taken and placed on the dialyser, the crystalloid or arsenious acid passes comparatively rapidly through the diaphragm, whilst the colloid or gelatine is left in greater part on the upper side of the septum, and thus the process can be successfully followed in the separation of substances from each other. Thus, arsenious acid may be readily dialysed from the albuminous and other organic ingredients present in the stomach of an animal which has been killed with that poison; and in employing this mode of analysis in the separation of poisons from organic mixtures it is interesting to observe that all the organic and inorganic poisons are crystalloids, and hence tend to pass readily through the diaphragm. Professor Graham has taken a mixture of a solution of arsenious acid and albumen, and having coagulated the albumen by heat, he has dialysed the arsenious acid out of the mixture to the extent of four-fifths of the entire amount of the poison. In a mixture of arsenious acid and porter, which was thrown on the dialyser, the poison passed through the membrane, accompanied by a faint yellow colour, which stained the water, and, at this stage, one-half of the arsenious acid had been dialysed from the porter mixture. This common poison has likewise been separated by this process from blood, the contents of the intestines, etc. My late assistant, Dr Alexander T. Machattie, now of Glasgow, has suggested that, in cases of poisoning, the stomach or the intestines, or both, might be simply secured at the openings, and the organs, with their contents, be placed in a vessel of water, when the crystalline poison would dialyse through the membrane of the coats of the stomach or intestines, and pass into the water, which might then be examined. This operation might be carried out as a preliminary operation in cases of poisoning; and it would have this manifold advantage, that an indication of the poisonous ingredient might be obtained without actually disturbing the contents of the stomach or intestines, and without removing them from these organs. Mr Whitelaw of Glasgow has suggested that the large quantities of brine from salted meat, which are at present thrown away, might be utilized by being subjected to the process of dialysis. He proposes to place the brine from the salted meat in bladders provided with stopcocks, and to suspend these bladders in a tub or tank of water. The crystalloid chloride of sodium will quickly pass through the membrane of the bladder, and in a few days the contents of the bladder will be found comparatively free from salt, which will have passed into the water in the tub or tank, whilst the extract of meat, which possesses colloid properties, will have remained behind in the bladder, and may thereafter be utilized as extract of meat or beef-tea.

The importance of the process of dialysis in the physiological changes which occur in the animal economy can hardly be over-

estimated. Many of the changes in the position of the matter of the living economy, as in the diffusion of substances through the animal system, must be regulated or influenced by dialysis. In pharmaceutical operations, it is worthy of consideration how far dialysis might be employed in the separation of crystalline substances from their source, as strychnine from nux vomica. In many geological phenomena, as in the infiltration of substances into rocky cavities, dialysis no doubt plays an active part.

The second mode of investigation which the recent progress of chemistry has placed in the hands of man is Spectrum Analysis, or the determination of the ingredients of a substance by the various lines or bands which it may produce when it is burned in a flame, and the light evolved therefrom is decomposed into its rainbow tints. The admirable researches of Bunsen and Kirchhoff have contributed the greater part of our knowledge on this important branch of the science, and have formed the key-note to the department of celestial chemistry. What the microscope is to the physiologist, and the telescope is to the astronomer, the spectrum apparatus or spectroscope is to the chemist. It supplies a most searching and delicate mode of analysis, and serves to indicate the presence of substances in quantities where, in some instances, they do not exceed $\frac{1}{100,000}$ th of a grain.

Spectrum analysis has already revealed the existence of four new metals, which probably, except for this mode of examination, would have still remained unknown even to scientific men. Bunsen was successful in discovering cæsium and rubidium, Crookes discovered thallium, and Reich and Richter were fortunate enough to make the most recent discovery of the metal indium. The credit of the practical application of the spectrum apparatus belongs to Bunsen and Kirchhoff, and they led the way in the detection of new substances.

The new metal, cæsium, was first discovered in the Durekheim mineral spring, 100 gallons of which yield about a grain of the metal, which is about the proportion of a ton of the water yielding two grains of the metal. It has also been found in other mineral springs, and, through the kindness of Professor William A. Miller, of King's College, London, I have here a minute quantity of a compound of cæsium, obtained by him from the Wheal Clifford hot-spring in Cornwall. Cæsium has also been found in certain minerals, such as lepidolite, which contains merely traces, as generally obtained, but one sample of this mineral, brought from the state of Maine, had 0.24 per cent. of the new metal, and a rare mineral, called Pollux, has yielded 32 per cent., and is the most extensive source of cæsium at present known. Cæsium is recognised by giving two bright blue lines in the spectrum.

Rubidium was also discovered by Bunsen and Kirchhoff in the Durekheim mineral spring, and is present in larger quantity than the cæsium. It is also found in other spring waters, and in several

minerals, including lepidolite. It is characterized in the spectrum by giving two violet lines and two red lines. I am indebted to Professor Roscoe of Manchester for the sample of chloride of rubidium which I now exhibit.

Thallium was discovered by Crookes in certain sulphur ores, and is most easily obtained from the flues attached to the burners of the sulphuric acid chambers where pyrites is employed. The thallium gives a beautiful bright green line or band of intense brilliancy. I have a sample of the metal of considerable size, which I now exhibit, and a large specimen of the crude chloride of thallium. The metal is very dense, and much resembles lead in general appearance.

Indium was first detected by Reich and Richter, but has been more fully investigated by Winkler. It is obtained from zinc-blende, especially from that employed in the Freiberg mines. The metal zinc obtained at these works contains traces of lead, iron, arsenic, cadmium, and 0.0448 per cent. of indium. It is a white metal, resembling platinum in general appearance, but is soft like lead, with which it has a similar fusing point, and marks paper when pressed upon it. It is not oxidized by the air, and, at a red heat, it burns with a violet blue flame. The spectrum of indium gives a decided indigo blue line.

Spectrum analysis has not only yielded discoveries in terrestrial matter, but it has likewise done good service in the examination of the constituents of the other planets, and also of the sun and stars. The researches on this subject have been principally made by Huggins and W. A. Miller in England, Cooke in America, Donati in Italy, and Father Secchi in France. Jupiter and Saturn possess atmospheres which contain some gases and vapours similar to those present in the atmosphere of the earth, but others are not identical. In Mars, the lines which are observed as characteristic of Jupiter and Saturn are not to be found, whilst in Venus all the lines of the solar spectrum can be seen. The general result of the observations on non-terrestrial orbs is, that there is a community of matter in the visible universe, and that whilst our sun, the centre of our planetary system, has been demonstrated to contain many of the elementary substances found on the earth, other stars have also been found to be composed, in part at least, of the same elements. Thus, the star Aldebaran has been shown to contain sodium, magnesium, hydrogen, calcium, iron, bismuth, tellurium, antimony, and mercury.

Advantage has been taken of the powers of the spectrum analysis to indicate the presence of blood-stains, and even the quality of that blood. Thus, Hoppe and Stokes have shown that when blood mingled with water is placed in a tube, and a ray of light is transmitted through the liquid, and thereafter through a prism, the spectrum which is obtained exhibits a number of dark bars or spaces which are bands or lines of absorption. These vary according as the blood is

arterial or venous, and many different series of bands have been obtained. It is suggested that this method of research might be resorted to in investigations on the blood in health and disease. Sorby has applied these observations to the detection of blood-stains in criminal cases, and has contrasted the colouring matter with cochineal, cudbear, etc. The blood in its various stages of oxygenation has been found to yield eight different spectra, four or five of which are quite distinct from everything else.

In metallic chemistry several novel points have been recently brought to light. The researches of Deville and Troost have shown that, at high temperatures, the metals are permeable to gases. Platinum and iron, when raised to a white heat, are so easily permeable that hydrogen passes readily through sheets of the metals; and in the case of platinum of one-twelfth of an inch in thickness the gas can be passed through under the ordinary pressure of the atmosphere. Stahlchmidt has announced that pure iron, which he has been the first to prepare, is a silver-white metal, and is so soft that it can be cut readily with an ordinary knife. The conversion of cast-iron into steel by the rapid process of Bessemer is now carried on on a most extensive scale in England, and likewise in Glasgow, at the Atlas Works, belonging to Messrs Rowan and Co., who have kindly forwarded to me a most complete series of specimens illustrative of the whole process. The principle involved in this operation is to rid the cast-iron of carbon and other impurities, by subjecting it, when in a molten state, to a powerful blast of air, which burns away the greater part of the extraneous matters; thereafter, a small proportion of a German iron called *Spiegeleisen*, and which contains much carbon, is run in amongst the molten and purified iron, which thus acquires sufficient carbon to give it the chemical properties of steel; and on pouring out the molten matter into moulds, blocks or ingots of steel can be procured of greater or less weight and size. In this manner, the lengthened processes of converting cast-iron into steel, which even yet are resorted to, and which comprise the tedious puddling operations, with the subsequent action of the steam-hammer and numerous rollers, and, lastly, the cementation process of baking the bar-iron for days surrounded by charcoal, are dispensed with, and, within a tithe of the time ordinarily employed, the cast-iron is converted into Bessemer steel, which only requires the steam-hammer and rollers to fit it for many important purposes. Bessemer steel is now superseding ordinary steel in many ways, and, doubtless, from the rapidity with which tons of this steel can be manufactured, there is every reason to believe that it will come into use for many purposes where ordinary steel, from its dearness, could not be employed.

A highly ingenious application of cast-iron to filling-in thin moulds of brass, copper, bronze, etc., has recently been brought out by Mr Atkin, of the firm of Messrs Winfield and Co.,

Birmingham. The very high temperature of molten cast-iron would cause the destruction of any thin mould of copper, brass, or bronze, were the cast-iron poured into the mould in the open air; but by immersing the thin mould in water, so that it shall be surrounded by the water, and then pouring in the cast-iron, the latter, in heating the metal of the mould, merely conveys heat to the outer film, which is immediately imparted to the water, and thus the mould is kept from being damaged or even tarnished. This process of filling in a mere shell of a more expensive metal with cast-iron is eminently suited for the preparation of weights for ordinary beams or for gas-lustres, for giving solidity to electrotypes of figures or statuettes, for stair-rails, metal bedsteads, door handles, etc., and, indeed, wherever iron can be employed, and which, by this process, can be coated with a thin covering of a more costly metal, and thus acquire the appearance of being solid brass, bronze, or copper.

A new method of coating iron with copper has been suggested. The iron is plunged into a boiling solution of a compound of copper with an organic acid, such as the double tartrate of copper and potash, with excess of alkali, and holding the iron article with a brass wire during the time of immersion. The iron is lifted out of the bath with a greater or less thickness of copper, according to the time in which it has been immersed in the copper solution. The investigations as to the presence of phosphorus in metals has led to the determination of two points: that a very minute proportion of phosphorus in copper wire lessens its power for the transmission of electricity, and consequently for the conveyance of telegraphic messages; and that the presence of one-eighth to one-half of a per cent. of phosphorus in copper imparts much greater tenacity to the copper, and renders it much more fit for the manufacture of tubing.

The metal magnesium continues to be manufactured in quantity, and the vividness and intensity of the light evolved during its combustion have been determined with much accuracy. The light has been observed at a distance of twenty-eight miles, and, considering the production of light over a period of ten hours, it has been observed that two and a-half ounces of magnesium, in burning, give out as much light as would be obtained during the combustion of seventy-four candles for the ten hours, and which would involve the consumption of twenty pounds of stearine. A curious point regarding the combustion of magnesium is, that it burns as readily in carbonic acid gas, which immediately extinguishes ordinary lights, as it does in common air.

A white gunpowder has been announced by Schultze. A sawdust is prepared by cutting cross veneers from logs of wood, and crushing these into powder. The material is then boiled for several days in a solution of soda, which is renewed from time to time, when the residual matter is washed with water, steamed for fifteen minutes,

and washed again in water for twenty-four hours. The sawdust can then be bleached with chlorine or bleaching-powder, thereafter boiled with water, washed and dried. In order to act upon this sawdust, a mixture of 40 parts of fuming nitric acid (sp. gr. 1480 to 1500) and 100 parts of sulphuric acid (sp. gr. 1840) is made up and allowed to cool, by standing two hours or so. 100 parts of the acid mixture are placed in an iron vessel, surrounded by cold water, and 6 parts of the sawdust are well stirred throughout the acid, and allowed to remain in contact therewith, with repeated stirring. The whole is then placed in a centrifugal machine, during the revolution of which the excess of acid is separated, and the sawdust is then subjected to a process of washing in cold water during several days, immersed in a dilute solution of soda to neutralize the remaining traces of acid, again washed with water and dried. The sawdust is finally treated with a solution of 26 parts of nitrate of potash in 220 parts of water, and dried at a temperature not exceeding 111° Fahr.; after which, the fine dust can be separated by a sieve. The white gunpowder thus prepared, and which is in reality a gun-sawdust, possesses highly explosive and great propelling powers.

A cheap blasting powder has been recommended, in which part of the nitrate of potash of ordinary gunpowder is replaced by nitrate of soda. The proportions employed in the manufacture of the new blasting powder are, nitrate of potash 57 parts, nitrate of soda 18 parts, charcoal 15 parts, and sulphur 10 parts. The cheapness of this powder is its recommendation, but there is a probability of its absorbing damp from the atmosphere.

A highly explosive oily substance, called nitroglycerine, with the composition $C_6H_5O_3(NO_5)_3$, has been discovered. When used for blasting, the bore or hole is made water-tight, the nitroglycerine is run in, and thereafter some water, and the oil is lighted by a fuse. One volume of the oil when exploded evolves 1300 volumes of gas, which consist of steam 554, carbonic acid 469, oxygen 39, and nitrogen 238. Weight for weight, the nitroglycerine is said to be five times more effective than ordinary gunpowder.

In the waterproofing of cloth, Dr Stenhouse has suggested that paraffin might be employed with great advantage. Previously, for the thorough waterproofing of the cloth, caoutchouc and gutta-percha had been used, and for the partial waterproofing bees'-wax and linseed or other drying oils had been employed, accompanied by oxides of lead and manganese as driers. The cloth, when treated with the latter mixtures, allowed the rain to fall off, without materially wetting the cloth, whilst at the same time the textile fabric was porous, and admitted of the ready passage of vapours, such as the perspiration from the body of the animal. This oily coating on the cloth, however, was liable to oxidation, and in time lost to some extent the property of being impervious to water.

Dr Stenhouse's patent suggests the employment of the solid paraffin, accompanied by a small percentage of linseed-oil; and the cloth which can be rendered water-tight by this mixture may be of any description and texture, such as cotton, linen, woollen, etc. Such cloth, impervious to rain, would be highly serviceable in the construction of tents and in the covering of packages, as well as in outer garments; and the protecting mixture has the advantage of wearing well, and of not being liable to oxidation or decay.

In the preservation of museum specimens of decaying skulls, bones, etc., M. Stahl has proposed that melted spermaceti should be brushed over the firm specimens, whilst the more decayed and more fragile might be immersed in a fused mixture of 4 parts of spermaceti and 1 of colophony resin. It has occurred to me that solid paraffin might be advantageously employed for this purpose. Ancient stained glass, the obscuration of which has now been shown by E. Chevreul to be due to the adhesion of smoke, earth-dust, and other matters, can have its original colours revived with all their early freshness by steeping the glass in a solution of carbonate of soda, of sp. gr. 1068, for a few days, and thereafter for a few hours in dilute hydrochloric acid, of sp. gr. 1080. When the glass is subsequently washed in water, the colours come out as pure as when the stained glass was first manufactured.

In Sanitary Chemistry, much progress has been recently made. The examination of the air in many places has been carried out by Dr Angus Smith of Manchester, who has already published a most elaborate series of observations on the proportions of oxygen and carbonic acid in the atmosphere in town and country districts, on the tops of hills, and in the depths of mines. Dr Smith has brought out a most simple and ingenious instrument, consisting of a small bottle with a valvular arrangement, for the estimation of the carbonic acid in the atmosphere of any locality. A measured quantity of baryta water is placed in the bottle, and by means of a small india-rubber ball, which is attached to the bottle by a short length of tubing, and which can readily be compressed by the hand, air can be drawn through the baryta water; and according to the number of times that the ball requires to be compressed before the air has imparted enough of the carbonic acid to the baryta water to cause the appearance of a distinct precipitate, the purity or freedom of the atmosphere of the district may be determined. The more largely contaminated the air happens to be with carbonic acid, the fewer the number of compressions of the india-rubber ball which are required to supply the carbonic acid requisite for the fulfilment of the test.

The purity of the water supplied to towns and mansions for dietetic purposes has occupied much of my attention for several years. The impregnation of the water by organic matters of the nature of sewage does not only lead to the presence of such organic matter in the water, which at times can be readily detected by the

senses of sight, and taste, and smell, but in the greater number of cases the water appears clear, possesses a good agreeable taste, and exhibits no odour. At such a stage the organic matter has in good part become so thoroughly decomposed as to lead to the formation of nitrates, which are dissolved in the water. In this state, the water will be found to be robbed of much of the oxygen gas which is dissolved in all fresh and healthy waters, and besides being unwholesome from the presence of the organic matter and nitrates, and the diminution of the oxygen gas in solution, the water, from the presence of the nitrates, has acquired the power of acting more or less powerfully on lead, so that it becomes dangerous to transmit it through lead pipes, or store it in lead cisterns. Water, therefore, when contaminated with organic matters, is liable to be doubly unwholesome from the impurity present, and the power which that impurity may confer upon the water to dissolve lead to a dangerous extent. A most ingenious apparatus for the distillation of water from sea-water, or even from partially foul water, has been patented by Chaplin of Glasgow; and this distil-aerator not only admits of the distillation of pure water, but also supplies to the water the air which is found dissolved in all wholesome waters. The importance of such an instrument in ocean-going steamers and sailing vessels, and in all positions where there is a practical difficulty in procuring or providing good water, can hardly be over-estimated.

The various disinfectants and deodorizers which may be employed in rendering less powerful the odours evolved from decomposing organic matter in cesspools, drains, etc., have lately received special attention. The most important of these agents is chloride of lime or bleaching-powder, which, in the proportion of 1 lb. to the 1000 gallons of sewage, proves itself a valuable and true disinfectant. The permanganate of potash, or Condry's fluid, is likewise serviceable in treating sewage, but is principally of value in the disinfecting of clothes, which lose all disagreeable odour when treated with the fluid, and at the same time the fibre of the cloth is not injured. M'Dougall's disinfecting powder, which mainly owes its efficacy to carbolic acid and sulphurous acid, removes instantly the offensive odour of sewage, and portions of this powder strewn over the floor of stables, byres, etc., quickly renders the atmosphere of such places untainted by ammonia or other gases. A room in which a patient is located, who is suffering from disease giving rise to offensive effluvia, may have its atmosphere much improved by introducing a vessel containing some of this powder. Solutions of chloride of zinc, chloride of manganese, and perchloride of iron with more or less water, are serviceable in fixing all odorous effluvia during the emptying of cesspools, etc. Charcoal, when strewn over decaying and putrefying matters, hastens their decay, and at the same time absorbs the gases. When the charcoal is associated with earth, its employment may be practically carried out. Quicklime,

when treated with water and used for whitewashing the walls and ceilings of rooms which have been inhabited by parties labouring under infectious diseases, or where much overcrowding has occurred, does good service in the destruction of any organic matters which may be lodging there. The mere application of heat, ranging from 210° to 250° Fahr. to woollen bedding or clothes and other articles, destroys any infectious matter which may contaminate them.

The large amount of sulphur which is burned off from metallic ores in Swansea and elsewhere, and which escapes into the atmosphere as sulphurous acid, and thereafter becomes in part at least sulphuric acid, has recently called forth the attention of scientific and practical men. In the neighbourhood of works discharging such sulphur smoke, the ground is barren, scarcely any vegetation can be seen for miles, and even high chimney-stalks are of little avail, as they merely carry away the sulphur smoke, and distribute it over a wider and more distant area. One extensive firm of copper-smelters discharge in this manner into the atmosphere about 1000 tons of sulphuric acid every week; and it is estimated that annually there are burned off from the copper ores worked in Swansea about 70,000 tons of sulphur, of the value of £455,000, and which might produce no less than upwards of 210,000 tons of sulphuric acid of the strength of oil of vitriol. Many of the manufactories of sulphuric acid have begun to use the copper ore as a source of sulphur, and thereafter hand over the roasted ore to the copper-smelter at Swansea. The raw ore contains about 50 per cent. of sulphur, and when roasted there are only left about 4 or 5 per cent. The ore is obtained in large quantities from the Guadiana river, Port Pomoron, in Portugal, from mines which were worked by the Romans, and it is used extensively for making sulphuric acid in London, Newcastle, Bristol, and other places. This is an excellent instance of the successful and economic employment of a material in the arts and manufactures, which was till lately, and in many places still is, a nuisance over extensive tracts of country. The sulphur smoke in a very modified condition occurs in all large towns where much coal is burned, and especially in manufacturing towns, where the coal is often of inferior quality. In such towns, by the mere burning of the sulphur in the coals, many gallons of sulphuric acid must be formed, and in rainy weather be washed down on the umbrellas and clothes of the people.

The aniline or coal-tar colours have now been extended in number, so that all colours of the rainbow and all shades can be obtained from coal-tar. Aniline was discovered by Unverdorben in 1826, who procured it by the destructive distillation of indigo. It is now obtained in small quantity directly from the destructive distillation of coal, as in gas-works, but is generally manufactured from the lighter coal-tar naphtha. When the naphtha is rectified, the portion which distils over at a temperature of 180° F. is benzol, and this substance was discovered by Faraday in 1825. By the

action of strong nitric acid, the benzol is converted into nitro-benzol, and the latter, when agitated with water, acetic acid, and iron-filings, becomes aniline. By the action of oxidizing agents, such as chloride of lime, bichromate of potash, chloride of mercury, etc., the aniline, which is colourless by itself, can be transformed into all shades of violet, mauve, majenta. By the researches of Hofmann, the number and beauty of the aniline colours have been much increased. Whilst numberless shades of reds and purples can be obtained, there is a splendid green called verdine, discovered by Eusebe, which remains a true pure green even by candle or gas light, and there are also a blue which is as clear as opal, a good yellow, and a fair black. In short, dyes of all hues can be obtained from aniline, which, in its turn, is procured from coal-tar. The intensity of these aniline colours may be indicated by the fact, that one grain of majenta in a million of water gives a good red; one grain in ten millions of water exhibits a rose-pink; one grain in twenty millions communicates a blush to the water; and one grain in fifty millions tinges the water with a reddish glow. The powerful tinctorial virtues of these dyes may be learned from a circumstance which occurred during the passage of the Great Eastern between Liverpool and New York, when the sea was observed to exhibit a crimson hue for some distance around the vessel, and when it was afterwards discovered that the bloody sea owed its colour to a wave having stove in a plate of the Great Eastern, and the sea got access to certain vessels which contained majenta.

The action of two organic substances on the animal economy has received special attention from scientific men, on account of the startling power which they possess in evolving vapours which are highly poisonous. In 1860, a workman was engaged in handling a vessel containing nitro-benzol, when some of the liquid was spilt upon his apron. Not suspecting any ill effects therefrom, he breathed the vapour arising from the liquid on the apron, and in four or five hours he began to feel unwell, and in nine hours he was dead. Five cases have occurred where a few drops of nitro-benzol have been taken accidentally into the mouth, and four of the cases died. It is a powerful narcotic poison, and the patients feel drowsy, have convulsions and coma, and death ensues. A small dose of nitro-benzol given to an animal may take three or four days before the poisonous action is developed, and in one case the death of the animal did not occur for nine days. Aniline closely resembles nitro-benzol in its poisonous properties, but the salts of aniline are said to be harmless. Nearly an ounce of a salt of aniline has been given to a patient within a few days, and no serious effects were observed.

A still more mysterious agent of destruction is presented to us in mercuric methide. This substance is prepared by placing ten parts of iodide of methyl, one part of acetic ether, and some sodium amalgam in a flask, where the materials are heated and then cooled.

Thereafter the mixture is distilled, and the water being separated, the acetic ether removed by alcoholic potash, and the remainder washed with water, the mercuric methide is obtained. This substance contains more than 80 per cent. of mercury, and its vapour has a most remarkable and injurious effect on the animal system. A chemical assistant, 30 years of age, and who had for ten years been engaged in investigations, was employed for three months in the preparation of mercuric methide, and must apparently have inhaled portions of the vapour. He began to suffer from impairment of sight, was thereafter seized with numbness in the hands, deafness, and great general weakness. His gums became sore, swollen, and tender. He moved his limbs slowly and spoke indistinctly. He was removed into hospital, but his case got worse and worse. The breath and the whole body evolved an offensive odour; the patient became maniacal and died in fourteen days from his admission into hospital. Another instance of the highly poisonous nature of mercuric methide had as melancholy a termination, though not a fatal one. About the same time as the preceding case, viz. the beginning of 1865, an assistant in the same laboratory worked for a fortnight with mercuric methide. In two months afterwards he went into hospital with the serious symptoms of having been affected with the vapours of the mercuric methide. This patient soon got quite idiotic, ceased to recognise any one, became deaf, lost the faculty of speech, and only retained the power of muttering and crying out. He is still living in hospital, with no prospects of recovering, and is a helpless idiot, who can only gather strength to move his limbs convulsively when he becomes violent.

The employment of gas in the chemical laboratory and in the workshop of the manufacturer for heating purposes and as a motive power, has recently been much extended. The very high temperature obtained by the combustion of oxygen and hydrogen in the oxy-hydrogen blowpipe, has drawn attention to gas as a heating agent of the first class. A blowpipe consuming hydrogen with air, has been in use for some years for soldering together the plates of lead forming a sulphuric acid chamber, by merely fusing the edges of the plates, and running the lead of both together. A powerful apparatus for the fusion of small quantities of metals and other substances has been brought out under the name of Gore's furnace; and it is exceedingly useful in chemical laboratories, in dental establishments, and elsewhere, on account of the readiness and despatch with which it raises the temperature of the substance being experimented upon. A gas-engine has been suggested by Lenoir, where coal-gas and air are admitted in proportions to form an explosive mixture, and the alternate explosion of such, by a battery on either side of the piston, propels the piston from one end of the box to the other. The employment of gas-engines for motive power in manufactories would be somewhat expensive, but wherever a small amount of force is required, or where the motive power

is desired for uncertain and short periods during the day, the gas-engine will be found highly serviceable. M. Arbos has suggested that the gas required for the engine might be procured more cheaply by passing steam over red-hot charcoal, which would yield carbonic acid, carbonic oxide, and hydrogen; and by transmission over milk of lime, the carbonic acid would be arrested, and the carbonic oxide and hydrogen would be obtained in a mixed state. The cost of such gas is stated not to exceed one-fourth to one-third the price of coal-gas.

Coal-gas of inferior quality may be much improved in illuminating power by passing it through or over naphtha, and experiments made on an extensive scale in the street lamps of London have shown a great increase in illuminating power at a much less cost than that of gas. Mr Bowditch has been very successful in arranging carbonators for this purpose, and his experiments have proved that a gallon of oil costing 2s., and 1000 cubic feet of London coal-gas charged 4s. 6d., making together 6s. 6d., will give out more light than 4000 cubic feet of the gas, which will cost 18s.

Chemical philosophy has advanced with great strides in these later times, and the grouping of substances and their symbolic notation are undergoing great alterations at the present moment. Many of the elementary substances have their combining weights doubled by the new system, and this will necessitate much change in the symbols of the compound substances. The changes which are now taking place had their origin in the observations of Gerhardt, a French chemist, who, desirous of upholding a strict relation between the combining and atomic volumes of substances, announced as a theory or hypothesis, that equal volumes of the elementary gases and vapours, when compared under similar conditions of temperature and pressure, contain the same number of atoms. There is no proof or special test to demonstrate the truth of this theory, but the conclusion was drawn from the desire to reconcile and equalize the combining volume of all elements with their combining weights or atomic weights. The working out of this theory by English chemists has led to a better understanding of the groups of substances, especially those of the kingdom of organic chemistry.

Four distinct groups of elementary substances can be constructed, owing to the proportion of hydrogen with which the elements can unite. Thus, chlorine can combine with hydrogen in the proportion of one volume of chlorine with one volume of hydrogen, forming *two* volumes of hydrochloric acid, from which the one volume of chlorine and one volume of hydrogen can again be separated. The chlorine thus unites with one volume or equivalent of hydrogen, and hence the chlorine is regarded as un-equivalent or univalent, and as the chlorine is merely the type of a class, it is stated to belong to the monohydric group of elements. On the other hand, oxygen (with its new combining proportion, 16 instead of 8), always combines with hydrogen in the proportion of one volume of oxygen to two volumes of hydrogen; forming *two* of

steam, and when these two volumes of steam are separated again into oxygen and hydrogen, they are resolved into one volume of oxygen and two volumes of hydrogen. Oxygen, therefore, unites with two proportions of hydrogen, and it is said that oxygen and its class are bi-equivalent or bivalent, and the group is named the dihydric class.

Again, nitrogen unites with hydrogen in the proportion of one volume of nitrogen with three volumes of hydrogen, forming *two* volumes of gaseous ammonia, from which one volume of nitrogen and three volumes of hydrogen can be separated. The nitrogen, therefore, is tri-equivalent or trivalent, and the class of which nitrogen is the type is the trihydric group. And, lastly, carbon (with its new combining proportion of 12, instead of 6) when it combines with hydrogen, does so in the proportion of one volume of carbon (theoretical vapour) with four volumes of hydrogen, forming *two* volumes of marsh gas, from which can be separated the one volume of carbon and the four volumes of hydrogen. Carbon and its class always combine with four of hydrogen, and are therefore quadrequivalent or quadrivalent, and the class of substances of which carbon is the type is called the tetrahydric group. These classes of bodies may be summed up in the following statement,—one volume of chlorine is satisfied with *one* volume of hydrogen; one volume of oxygen demands *two* volumes of hydrogen; one volume of nitrogen calls for *three* volumes of hydrogen, and one volume of carbon is not content until it receives *four* volumes of hydrogen.

In the construction of the various organic substances the relative satisfying powers of the chlorine, oxygen, nitrogen, and carbon atoms must be taken into account, and if the carbon does not obtain its four volumes of hydrogen, or its quadruple power of combining be not satisfied, then an imperfect or unstable compound is formed. Several atoms of carbon may be associated together, and, indeed, compounds of the alcohol series are known as high as thirty atoms of carbon, and all these must be satisfied at four points by being in union with hydrogen or the neighbour atoms of carbon. Where a vacant space or point of the carbon occurs, then it represents an unfinished or unsatisfied molecule.

The law of substitution in organic substances enables the chemist to vary the compounds which are produced. Thus, whilst in marsh gas the one of carbon unites with four of hydrogen, there is no difficulty in taking the compound thus formed and abstracting one, two, three, or even all the four atoms of hydrogen, and putting in one, two, three, or four of chlorine instead; so that there can be formed compounds of one of carbon with three of hydrogen and one of chlorine, with two of hydrogen and two of chlorine, with one of hydrogen and three of chlorine, and, lastly, with four of chlorine alone. In this manner many new substances can be formed, and instead of the chlorine, other elements may be employed in substitution, such as bromine and iodine, forming a new series of substances.

The researches in organic chemistry have led to the actual construction of complex organic compounds from the simplest elements. Till within the last few years, organic compounds were regarded as being the property only of the vegetable or animal kingdoms, and it was believed that when the chemist wished any of the members of the group, he must have recourse to the plant or the animal, and crave from it a part of its substance. Now-a-days, however, the organic compounds are manufactured by the chemist without the aid or intervention of the plant or the animal, and every year adds to the number of substances thus produced. Among the organic compounds which have thus been artificially formed are urea, glycerine, grape sugar, alcohol, and prussic acid.

The advances which have been made in organic chemistry in modern times teach us that by merely following the beaten track of observation, by simply travelling the roads already laid down, there are apparently many millions of substances waiting to be formed and named. Indeed, it has been calculated that there are several sextillions of organic bodies which may yet be discovered by carrying out the present modes of research.

The progress of the science of Chemistry, where the elements are combined together to form compounds, may be likened to the union of letters to form words; but the process does not stop there, but may be still further considered to be the throwing of these words into sentences, forming an endless chain of ideas—an endless chain of new chemical compounds. It should therefore be observed that the various substances which form types of the classes of bodies are representatives of hundreds of substances which are already known, and doubtless of millions of bodies to be yet formed. And remember that twenty years ago this country was undiscovered: the roads or modes of operating were few; but each year must add to the number of new substances as the great domain of organic chemistry is explored.

And how much may we expect to reap as the reward of human labour in chemical science? Natural substances, as found in the mineral, vegetable, or animal kingdoms, are few as compared with the compounds which are artificially formed, and yet not artificially. The ingenuity of man is exerted on natural substances by natural laws, and he as the artificer merely places stone upon stone, and chemical affinity or attraction binds them together.

How many more substances of vast commercial interest, such as the aniline colours, may be compounded by man, it would be hard to tell, and how many more of medical interest, such as chloroform, it would be presumptuous to speculate upon. As far as the locomotive or iron horse surpasses all the energies of the animal as an aid in locomotion, and gunpowder in the rifle eclipses the human arm in the range and power of its aim, so, doubtless, the researches and applications of Chemistry will continue to transform natural substances into new compounds of vast interest to humanity.

Part Second.

REVIEWS.

The Elements of Prognosis in Consumption. With Indications for the Prevention and Treatment. By JAMES EDWARD POLLOCK, M.D., F.R.C.P., Physician to the Hospital for Consumption and Diseases of the Chest, Brompton, etc. London: Longmans: 1865.

ALTHOUGH a great deal has been written regarding consumption, the subject is in many respects still incomplete; and it is Dr Pollock's endeavour in the volume before us to fill up one of the existing blanks, by classifying cases of phthisis, indicating their "varieties and their ultimate progress by such natural characters as shall assist us to their recognition in the daily work of practice." To the performance of his task, Dr Pollock has brought sound pathological knowledge, habits of accurate observation, and the experience gained during ten years at the Brompton Hospital for Consumption.

After some preliminary observations regarding chronic organic disease, and the resistance opposed to it by the system, Dr Pollock proceeds in his fourth chapter to treat of the systemic origin of phthisis. Dr Pollock thus summarizes his views upon this point. "It is to be maintained that the disease never arises without a preceding disorder of general health; that the fever and emaciation may, and frequently do, indicate established local mischief, but that they are not found invariably in this relation; that it has never been demonstrated that a hæmoptysis has given rise to tubercle in the lung, and that all arguments derived from analogy are opposed to this view; and, finally, that there is no invariable direct relation, but a very constant difference observed between the amount of local mischief and the intensity of constitutional disturbance."—P. 45.

To all of these statements we have no hesitation in subscribing; although we must say that Dr Pollock's views regarding a pre-tubercular stage of phthisis are rather more decided than our own. That such a stage does generally precede the development of local disease we fully admit, at the same time we do not regard it as essential or specific. We look upon this stage as merely a condition characterized by deficient vital energy, and as very likely to be followed by the deposit of tubercle in consequence of the system not being able to resist the causes which lead to the local lesion. We do not believe that there is at this period any specific

alteration in the blood ; in all probability it is impoverished, possibly improperly purified ; but we agree with Virchow in believing that dyscrasiæ have not their starting point in the blood, but that all have a local origin.

The next chapter is entitled, "Duration of Phthisis." Dr Pollock very properly begins by stating that cases of consumption differ so much among themselves that it is difficult to get trustworthy data for the history of cases, and that the early symptoms are often so obscure that no average derived from all instances of the disease can be of the slightest use in assisting us to form an idea of the probable duration of an individual case. Dr Pollock accordingly divides his cases into classes ; he takes as a standard "ordinary phthisis," under which are included all cases presenting the ordinary features of the disease, and which had lasted eighteen months. Cases of this kind are intermediate between cases of "acute phthisis" and those of "chronic first stage," "chronic second stage," "chronic third stage," and "strumous phthisis," all of which are treated of in subsequent chapters.

In the sixth chapter, Dr Pollock treats of the premonitory stage of ordinary phthisis ; we cannot follow him through it in detail, but append his prognostic summary.

"a. The insidious form of phthisis, scarcely announced in its beginnings, is more likely to become chronic than that which invades with acute access of fever.

"b. The acute form is ushered in with much febrile action, and considerable gastric disturbance ; the chronic rather by a gradually progressive emaciation than by fever.

"c. Severe sweating is indicative of rapid cases.

"d. Very slow emaciation is most frequently seen in the phthisis of advanced life, the changes in the nutrition of the body generally proceeding to a great degree before local disease is established.

"e. Phthisis succeeding acute affections, as fever, rubeola, etc., generally belongs to the tolerably rapid form of the disease, but not inevitably to the most acute.

"f. Phthisis after pregnancy, and during lactation, is generally rapid.

"g. The rheumatic temperament tends to chronic rather than to acute phthisis.

"h. The multiplication of hereditary tendencies in a single case has more a diagnostic than a prognostic value. The *form* of phthisis which prevailed in other members of the same family may help us to estimate the probable future of the case. If two or more brothers have had, say, chronic phthisis, the chances are in favour of the development of the same type of the tubercular disorder ; if a brother or sister has been struck down by the acute variety at the same age, we may fairly anticipate a similar progress.

"i. The absence of assignable cause for any case presenting the features of early consumption, is a feature favourable for chronicity. Where neither individual temperament, previous disease, nor hereditary tendencies are present, the prognosis for a slow rather than an acute form of the disease may safely be formed.

"j. Where any of the conditions rarely concurrent with phthisis are observed, as emphysema, etc., the consumptive disease is often latent, and generally slow."

Of acute phthisis, treated of in the seventh chapter, we have but

little to say. Dr Pollock distinguishes two varieties, (1.) the *acute inflammatory*, in which the local disease is a combination of inflammatory products and of tubercle; (2.) a passive exudation of crude tubercle, involving the whole of both lungs, with slight febrile symptoms. The cases included under the first class are characterized by their rapid progress and continuous fever, unattended by remissions; the prominence of gastric symptoms; rapid emaciation; death, which may occur within a few days, but which is more commonly delayed for some months. Tubercular deposit is diffused, often very extensively, through both lungs, softening occurs early, but the resulting cavities are irregular and undefined, never circumscribed. Under the second head are included cases where deposit is rapid and general, but with little tendency to softening; where febrile symptoms are not acute; where the digestive system may remain unimpaired, but in which emaciation steadily advances. These cases are usually characterized by strong hereditary predisposition, and death generally occurs in from three to four months.

Dr Pollock then passes on to the consideration of "ordinary phthisis." Tubercle he considers, we believe incorrectly, as "an exudation from the capillaries," and he distinguishes the grey from the yellow, the latter having the greater tendency to soften, and having in it more elements of danger than the former. Once deposited, tubercle may, in Dr Pollock's opinion, follow four different courses. It may be absorbed, leaving the lungs and the constitution uninjured. It may remain unchanged, and no fresh deposit taking place, may be tolerated for an indefinite time. It may undergo calcareous transformation, and may be expectorated, or remain embedded in the lung. Finally, and most frequently, the tubercles become aggregated, disintegrate, and soften, involving the neighbouring structures of the lung in the destructive process. Each of these terminations of tubercle is considered by Dr Pollock. That tubercle is capable of being completely absorbed cannot be said to be perfectly proved. Still arguments from analogy are not wanting. The deposits of scrofula, nearly or quite identical with tubercle, may be absorbed. Cases occur where such absorptions are witnessed in young persons, and where at a later period symptoms connected with the lung, having all the character of tubercular disease, show themselves, but afterwards completely disappear, leaving no abnormal physical signs. The circumstances under which Dr Pollock thinks the absorption of pulmonary tubercle most likely to take place are the following:—

1. The nature of the tubercle. Scattered grey tubercles, most favourable.

2. Absence of signs of irritation in the lung, crepitant râles, much dulness, the various rhonchi, etc.

3. Phthisis not inherited occurring in an individual previously healthy; well-made chest; no previous syphilis or courses of mercury.

4. Speedy subsidence of constitutional symptoms, which may at first have been very acute.

5. One lung affected; the right the most favourable.

6. The patient in the best hygienic conditions.

In the next chapter Dr Pollock considers the history, signs, and prognostics of the first stage of phthisis. The conclusions arrived at are the following :—

“ 1. Of the two kinds of deposit, scattered and aggregated tubercles, the latter is the more serious.

“ 2. The physical condition of the lung is never to be regarded singly and without reference to the symptoms.

“ 3. The physical signs resolve themselves into those indicative of simple deposit, and those arising from irritation of the lung.

“ 4. Moderate dullness of the percussion note, and alterations in the intensity and duration of the respiratory murmurs, but chiefly of the expiration, have reference more especially to the tubercle deposited.

“ 5. A tubular quality of the respiration, dry crepitation and crackling, with sibilant and other rhonchi, combined with a more intense dullness of the percussion note, may be referred to the greater condensation of the lung from inflammatory products, and to irritation of the minutest air vesicles. This class of signs, if persistent, is the more grave. In certain instances they are capable of removal by local depletion, while the former are permanent.

“ 6. Wavy inspiration, if a single sign, is of doubtful import; but if long persistent, generally means slow phthisis.

“ 7. Wavy inspiration, if accompanied by the signs referable to condensation of the lung tissue from inflammatory products, is indicative of rapid phthisis.

“ 8. Alterations in the chest walls in the first stage indicate an already long-existent cause, and are not unfavourable prognostics for duration.

“ 9. A deposit in the apex (the ordinary form) is more likely to undergo rapid changes than a similar condition at the base of the lung.

“ 10. Deposits at the base occur most frequently in children (the strumous form), and in adults about middle age. This form is rare in females, and generally means slow phthisis.

“ 11. Hæmoptysis in the first stage indicates congestion of the lung. When profuse, its import is bad, as implying a likelihood of the rapid passage of the tubercle into the second stage.

“ 12. Cases of profuse hæmoptysis in the first stage may afterwards become chronic, and the average duration of cases so commencing is above that of ordinary phthisis.

“ 13. Profuse hæmoptysis is likely to be repeated in the later stages, and so forms an element of danger.

“ 14. The intensity of the fever and absence of complete remissions, with gastric symptoms of severity, taken together, indicate rapid phthisis. The extent of physical signs is here of less moment than the symptoms.

“ 15. Rapid waste of flesh, especially when occurring in persons rather above the average weight, is a bad prognostic; males are most subject to rapid reduction of weight.”

The next chapter treats of those cases where the deposit is tolerated. Two classes of cases are distinguished, the first where there are neither local nor general symptoms; the second where there is slight irritation of the lungs, with recurrent febrile attacks. “Both these forms may end in softening and its concomitant symptoms, and such is the result in a very large majority of the cases; but it is possible for the patient to die of some non-tuber-

cular disease." Such chronic cases do not occur in very early life, and appear to be equally frequent among males and females.

Calcareous transformation of tubercle is most likely to occur in advanced age, but may take place in the young. The chief symptoms consist in expectoration of chalky, horny or bony matters, frequent slight hæmoptysis, occasional severe exacerbations of cough, with fever, alternating with comparative cessation of the more distressing symptoms. Secondary disorders, such as diarrhœa, are long delayed. Such cases are generally very chronic, and a safe prognosis for duration may be offered.

The thirteenth chapter treats of chronic second stage, where the usual softening of tubercle takes place.

The prognoses of this stage are thus stated by Dr Pollock :—

"The favourable prognostics in chronic diffused tubercle may be summed as follows :—

"1. Phthisis not hereditary.

"2. The male sex.

"3. Age over thirty.

"4. Dulness ranging over a large extent of one or both sides, and not extreme.

"5. In most cases slight contraction of the chest walls on one or both sides, and movements impaired.

"6. In certain fewer instances, large well-developed chest, with movements good.

"7. A diffusion of the physical signs, consisting of roughness, expiration prolonged, and humid or crackling crepitation over a large space.

"8. A dry character of the crepitus, or (more favourable still)—

"9. A transition from moist to dry crackle, and, still further, to simple harshness of the inspiratory tone.

"10. A moderate and stationary amount of emaciation. These cases never fatten, as chronic cavity cases occasionally do.

"11. A lessening amount of expectoration; slight occasional hæmoptysis being generally productive of little or no inconvenience, and not accompanied by an extension of the physical signs.

"12. Absence of fever.

"13. Integrity of the digestive processes.

"The above facts must be ascertained from more than one examination of the patient, and by a careful comparison of the history of the case with the actually existing signs and symptoms.

"The bad prognostics are, simply an expression of the liabilities of this form of phthisis, and stand in the following order of danger :—

"1. The occurrence of congestion of the lung—intercurrent pneumonia.

"2. Profuse hæmoptysis, especially if rapidly repeated.

"3. Rapid formation of cavity of the anfractuous kind in any part of the lung.

"4. The rapid extension of disease in the same or the opposite lung.

"5. The ordinary events of advanced phthisis, which are in this form generally delayed; such are ulcerated larynx, diarrhœa, etc."

Dr Pollock next treats of the third stage of phthisis, that stage in which cavities are formed, and, after a careful consideration of a number of cases, arrives at the following conclusions.

"The prognostics in the third stage of phthisis are to be derived from a most careful study of the history and symptoms of our patient, always in conjunction with the physical signs. The following summary may assist :—

" Good prognostics derived from personal history.

" 1. Family antecedents not phthisical.

" 2. The male sex.

" 3. Age not under twenty.

" Good prognostics derived from the physical state of the lungs.

" Supposing a cavity to have formed, to afford a reasonable hope of its becoming tolerated it should be—

" 1. Confined to one lung—the apex.

" 2. The right lung is more favourable, especially in males.

" 3. The physical signs should be accumulated in one spot of limited extent.

" 4. The physical signs should be well marked and well defined; that is, the more evidence there is of a perfect excavation, from percussion, breath, cough, and voice sounds, the better; and there should be an abrupt limit of these signs at the lower edge of the cavity.

" 5. The cavity should be of moderate size, with the physical signs included in 3 and 4, and not extending to other parts of the lung; while fever is continuous, and the constitutional disturbance intense, a very guarded prognosis should be offered, always, however, holding in reserve the possibility of such a case becoming one of *limited* and *tolerated* cavity. On the cessation of the constitutional disturbance, the further favourable prognostics are—

" 6. A gradual flattening of the subclavicular space, the other parts of the chest retaining their mobility.

" 7. Alteration of the heart's position.

" 8. An expansion of the opposite lung to fill up the pulmonary space lost by the shrinking cavity.

" 9. The absolute immunity from disease of other parts of the same lung, or of the opposite.

" 10. The cavity sounds become more dry.

" 11. The absolute cessation of gurgling.

" 12. The substitution of leathery creaking sounds.

" 13. A diminution of the area over which the phenomena of cavity are heard.

" 14. The ultimate permanence of dulness, fixity, retraction of chest walls, and blowing sounds with prolonged expiration, as the only phenomena.

" The favourable prognostics derivable from symptoms are inseparable from those furnished by the physical signs, and by the progressive character of these latter. They consist in—

" 1. The subsidence of all the constitutional symptoms— hectic, waste, and secondary gastric disorder—soon after the formation of a cavity in the lung.

" 2. A quiet pulse.

" 3. Diminished cough, and lessened expectoration, moderated dyspnoea, absence of local pain.

" 4. The absence of the secondary complications of phthisis, diarrhoea, laryngitis, etc.

" 5. The gradual restoration of the processes of nutrition, improved appetite, gain of flesh, improved colour of face, lips, and tongue, and nails.

" 6. Improved nervous power, evidenced in subsidence of irritability, more tranquil sleep, less anxiety, and revived hopes.

" 7. The restoration or gradually improved regularity of the catamenial functions in females.

" The continuance of the stage of toleration is evidenced by—

" 1. An habitually quiet pulse.

" 2. The absence of sweating.

" 3. No hæmoptysis (or its rare occurrence).

" 4. The integrity of the digestive system, in its primary and secondary processes.

" 5. A maintained average of weight, lower than the mean of health, but never rapidly sinking.

" The possibility of obtaining the requirements for a maintenance of this state of toleration, viz. :—

"1. Freedom from anxiety, whether that due to precariousness of the means of providing the necessaries of life, or to grave concerns for the patient or his dependents.

"2. Abundance of the comforts of life.

"3. Pure air, well-ventilated apartments, exercise.

"4. The absence of the sedentary occupations, or of those of a generally unhealthy character, and especially of that which appeared to act as a promoting cause of the disease in its beginning."

We have quoted so largely from Dr Pollock, that we have little space left to consider the remainder of his work, but can only allude very briefly to one or two of the principal chapters.

Age is an important modifier of phthisis; in early life, the most formidable manifestations are shown, and the cases run a rapid course. In advanced age, the disease, though much less common than in youth, is by no means rare. It is more common in men than in women, and in a considerable number of cases the base of the lung is affected. At this period of life hæmoptysis and the other complications of phthisis are usually absent; gradual emaciation is generally the earliest symptom. "The disease may be confined to one lung in the aged, and yet prove fatal; a fact, which as it is the reverse of the ordinary events of phthisis, should be borne in mind in offering a prognosis."

There is an interesting chapter on the relation of phthisis to menstruation, pregnancy, and lactation. As to menstruation, the following are Dr Pollock's conclusions:—

"1. A rapid invasion of phthisis may be dreaded when the disease is hereditary, and attacks at the age of puberty in either sex.

"2. Tuberculosis delays or wholly puts a stop to uterine regularity at the age of puberty in females.

"3. The establishment of this function is often attended by relief to the tubercular disease, and its persistent regularity is a good sign.

"4. The permanent arrest of menstruation in phthisis is a very common occurrence. This event is preceded by much exhaustion of the system and vital powers, and is a bad prognostic.

"5. When menstruation is pretty regular in a phthisical patient, all the symptoms are liable to undergo an increase at the monthly period.

"6. When menstruation is suddenly checked, a hæmoptysis is a frequent event, and may be regarded as vicarious, and with less import of pulmonary mischief than under other circumstances.

"7. In very chronic first or second stage cases, without fever, the catamenia are often absent altogether. In such cases a resumption of the uterine function is preceded by a higher sanguification, and is a good prognostic.

"8. In very chronic cavity cases of the anæmic variety the absence of the catamenia is not a sign of bad omen, the system living, as it were, at a low average, and all vital functions being economized.

"9. In chronic cavity cases, where the system has recovered to a considerable extent the injury suffered, the regularity of this function is a positive sign of vigour, and an essential element in a favourable prognosis for time."

Pregnancy to a woman with a predisposition to phthisis is fraught with great danger. The period of gestation is generally one of latency, or rather of quiescence, of the symptoms. However rapid the form of phthisis, it is very rare that the patient dies

undelivered. From the time of labour, however, the symptoms become more severe. Hectic speedily sets in, and diarrhœa or laryngeal disease exhausts the patient. Cases, which up to this time had been chronic, break up, and death often occurs very speedily. Lactation is a highly dangerous condition for the phthisical or those predisposed to phthisis. "If pregnancy retard the course of phthisis, lactation accelerates it." The drain is greater than the system can support; and in Dr Pollock's words, "the rule, therefore, ought to be imperative, that mothers already in consumption, or threatened with that affection, should not nurse. The health of the child is also concerned in the question; for, although the mother may have an abundant supply of milk, it is seldom of a nutritive quality, and the infant wastes from want of proper food." It is well that such facts should be generally known; for a popular opinion to the contrary, especially with regard to pregnancy, still prevails, and marriage has been frequently counselled as a mode of arresting or warding off a tendency to phthisis. The danger of such advice, especially as phthisical women are not deficient in fecundity, need not be insisted on.

The value of hæmoptysis as a prognostic sign in phthisis is specially considered. It is peculiarly liable to occur at two periods; first, at the very beginning of tubercular disease in the lung, and, again, when a cavity has formed.

Hæmoptysis is very rarely fatal in the first stage of phthisis; but, when profuse, it indicates a rapid passage from the first to the second stage, though the subsequent progress of the case may be chronic. In the third stage, hæmoptysis is very seldom fatal where there is only a single cavity. "Double disease is the condition fraught with most danger, as the remaining respiratory space is blocked up, and the patient may die instantly from suffocation." It is an interesting fact, that although spitting of blood is a much more common symptom in females than in males, profuse hæmoptysis occurs much more frequently among males.

Pneumo-thorax is a very serious complication of phthisis. It may prove very rapidly fatal by collapse; more slowly by irritative fever and exhaustion, due to effusion into the pleura; or the effects of the complication may be recovered from, and the patient sink in the ordinary course of the tubercular disease. The following are the conditions of temporary recovery from the lesion of the lung and pleura implied in pneumo-thorax.

"1. A tolerably well-defined cavity.

"2. A well-thickened and protective pleura.

"3. A small and fistulous opening into the pleura, large enough to admit the passage of air, and small enough to forbid the passage of liquid matters. Its position in the lung must also favour this condition.

"4. The persistent absence of effusion.

"5. A tolerably sound condition of the opposite lung."

The remainder of Dr Pollock's work consists of five chapters on the treatment of phthisis, to which space does not allow us to allude. They are sensible and practical, founded upon an extensive experience, and will constitute a safe guide to the practitioner.

In conclusion, it only remains for us to thank Dr Pollock for this interesting contribution to the literature of phthisis, and to recommend it to the careful consideration of our readers, who will find in it much to assist them in endeavouring to arrive at a prognosis in doubtful cases.

The Principles and Practice of Medical Jurisprudence. By ALFRED S. TAYLOR, M.D., F.R.S. 8vo, pp. 1186. London: Churchills: 1865.

It has long been matter of reproach to this country that it possessed no complete treatise on Medical Jurisprudence which could bear comparison with several continental works. The Manual of Dr Taylor, which has now reached its seventh edition, is confessedly imperfect, and omits such topics as—identification and the important questions connected with the signs of death. It was known, however, that the author intended to publish a complete work on the subject; and the publication of various lectures in the Medical Gazette, on the Signs of Death, Identification, and Life Insurance, showed, by their fulness of information and the references to the latest cases, how satisfactorily Dr Taylor could execute such a task. The Manual has long occupied a high place in the esteem of the legal and medical professions, and is a most trustworthy work. As an editor, the author is most conscientious, and every edition afforded proof of the care he exercised in revision, and of the pains he took to collect the most recent facts. Subject-matter prepared and revised so well, it was almost impossible to rewrite, and we were not surprised to find the entire contents of the Manual absorbed into the present volume, and constituting by far its largest portion. Hence to those acquainted with the smaller volume (and who is not?), and with the lectures in the Medical Gazette, the present bulky volume presents little novelty. We have, in numerous instances, compared the corresponding passages in the two works, and we observe that the author has subjected the various chapters to careful revision, and has brought them up to the requirements of the date of publication.

In an introductory chapter on Medical Evidence, the author gives excellent advice on the subject of medical reports, conduct in the witness-box, and traces the progress of a criminal case in the English courts. He thus defines the subject of the volume, and shows the value of a correct knowledge of it to the practitioner:—

“Medical Jurisprudence—or, as it is sometimes called, Forensic, Legal, or State Medicine—may be defined to be that science which teaches the application of

every branch of medical knowledge to the purposes of the law; hence its limits are, on the one hand, the requirements of the law, and on the other, the whole range of medicine. Anatomy, physiology, medicine, surgery, chemistry, physics, and botany, lend their aid as necessity arises; and in some cases all these branches of science are required to enable a court of law to arrive at a proper conclusion on a contested question affecting life or property.

"Medical jurists are by no means agreed upon the exact boundaries of their science. Some authors include forgery and coining, and all offences requiring purely chemical evidence; others include feigned diseases, nuisances, and subjects connected with public health or sanitary legislation; while one German writer introduces the government and regulation of temperance societies. Those who thus propose to enlarge the science forget the maxim, '*Ars longa, vita brevis*;' and they also forget that, by demanding too much of a medical practitioner, they may deter him from undertaking the study of that portion which may really prove serviceable to him in the practice of his profession. The purpose of this work has been to bring as far as possible within a reasonable compass those subjects which especially demand inquiry, and which more particularly concern the duties of the educated physician and surgeon. The definition above given necessarily implies that a medical jurist should have a theoretical and practical knowledge of all branches of the profession, a large range of experience, and the rare power of adapting his knowledge and experience to emergencies. He should be able to elucidate any difficult medico-legal question which may arise, and be prepared at all times to make a cautious selection of such medical facts, and a proper application of such medical principles, as may be necessary to enable a judge to place the subject in an intelligible light before the jury, and to enable a jury to arrive at a just conclusion. Medico-legal knowledge does not consist so much in the acquisition of facts as in the power of arranging them, and in applying the conclusions to which they lead to the purposes of the law. A man may be a most skilful surgeon, or a most experienced physician; his mind may be well stored with professional information; yet if he is unable by the use of simple language to make his ideas known to others, his knowledge will be of no avail: one far below him in professional standing and experience may make a better medical witness."

In Scotland, when a medical report is placed in the hands of the authorities, it cannot afterwards be amended or rewritten. A supplementary document may be appended, explaining any omissions that have been made. But such a proceeding would tell against the reporters, whose subsequent evidence would not fail to be closely criticized, when read in the light of the imperfections of the report. The examination of medical men, styled the precognition, is conducted in private, and the statements then made are not communicated either to the judge or to the counsel for the defence, who must institute an examination for themselves. In England it is different; hence the importance of the caution conveyed in the following quotation. In both countries, the medical reports forming portion of the documents produced on the trial and libelled upon, are copied for the judge, and are also placed at the disposal of the defence.

"It is necessary that a medical witness should remember that copies of his report and depositions, either before a coroner or a magistrate, are usually placed in the hands of counsel as well as of the learned judge, and that his evidence as it is given at the trial is compared word for word with that which has been already put on record. There is reason to believe that this is not

generally known to members of the medical profession, and thus it happens that, either from failure of memory, want of accurate observation, a carelessness in giving evidence at coroners' inquests, medical witnesses lay themselves open to severe censure, either by stating matters differently at the trial or by giving a very different complexion to the facts. Any serious deviations from what is on record will, of course, tell unfavourably for the witness, supply materials for a severe cross-examination, and form an excellent ground of defence for the prisoner. The witness's weakness is the prisoner's opportunity, and, of course, his counsel will not lose the occasion of impressing upon the jury that a man who can upon oath give two different accounts of the same transaction is not to be believed on either."

For a considerable period it was customary in Scotland to exclude medical witnesses from the court during the progress of the case. But during the last few years a change has come over the practice of our highest criminal court; and in all cases where it is deemed of importance that the medical witnesses on both sides should hear the evidence, they are allowed to remain in court. The reference to Scotland in the following extract is therefore no longer correct:—

"In England, medical and scientific witnesses, except under special circumstances, are allowed to be present in court and hear the whole of the evidence in the case. This is, in some instances, absolutely necessary if the court requires medical opinions; for unless the witnesses are fully acquainted with the facts they can give no opinions, and they can only become fully acquainted with the facts by being allowed to be present and hearing the evidence in court. If excluded, the judge or counsel will be compelled to read to the witness notes of the evidence before an opinion can be given, and it may then appear that some small point which counsel did not think of importance is omitted. This, if known to the witness, might, however, materially affect his opinion. A failure of justice is likely to occur when medical witnesses are excluded, and it is generally where there is no defence, or a false defence, that the right of excluding them is exercised. The rule in Scotland is different; medical witnesses are there rigorously excluded from court until after they have delivered their evidence."

The license permitted by the judges in England to counsel has long been subject of remark. In this respect Scotland contrasts most favourably with the sister country. The medical man is invariably protected by the court from insult; and such questions as Dr Taylor refers to, "When and by whom were *you* first retained in this case?" would not be allowed. In England, it appears that "judges seldom interfere unless appealed to by the witness;" and in some cases we have read of, the remonstrance of the judge to the offending counsel was couched in the gentlest terms.

On the subject of examination in the witness-box, Dr Taylor has some excellent remarks, and very clearly points out the different aims of the prosecution and defence, and the widely different manner in which the examination is conducted on the two sides.

"In cross-examination the counsel for the prisoner extracts from the medical witness, by questions *which may suggest the answer in the strongest form*, any facts that may appear to be favourable to his client, and which he believes to be within the witness's knowledge. Leading questions are not only allowable in this part of the examination, but, according to good authority, a counsel for

the defence can hardly lead too much. The theory of the law is, that the witness is unfavourable to the prisoner, and has come to bear evidence against him. The more he has shown himself by conduct or conversation a partisan in the case, the more severely will he be treated. Anything which he may have said in the hearing of others, or published in journals, or even written in private letters (if the contents transpire), in reference to the case or the guilt of the prisoner, is now brought to light, although he may have supposed that what he did say was in perfect confidence. It is at this stage of the case that any exaggerations which may have been most favourably received by the counsel for the prosecution are reduced to their true proportions. Any bias by which the mind of a witness may have been influenced, or any imperfection or confusion of memory as to facts, is here brought out. It is in this part of his examination that the witness will be closely questioned, as to his qualifications, the time during which he has been engaged in practice, the accuracy of his judgment, his general professional knowledge, and his special experience in reference to the matter in issue, the number of cases he has seen, etc. Straightforward answers should be given to all these questions. No harm can be done to the witness by the answers, unless they are given evasively, since it is not to be supposed that the witness wishes to represent himself differently from what he is. If he does make the attempt, he will assuredly fail. The most striking distinction between the examination in chief and cross-examination is in reference to leading questions. It rests upon the assumption, that there is a danger that a witness will say whatever is suggested to him by the one side, and conceal everything that is not extorted from him on the other. . . . In dealing with a skilled witness, whose evidence may be of importance, the questions in cross-examination are usually put by the counsel for the prisoner with great caution, as the answers brought out may be more adverse to his own case than those elicited in the examination in chief."

Dr Taylor is in favour of the employment of experts in criminal cases, and deprecates the evidence given in the English courts by men possessing imperfect knowledge, and whose standing in the profession carries with it little or no respect. In speaking of the coroner's court, and of the careless way in which medical men are selected for the responsible task of making post-mortem examinations and reporting upon cases of serious injury, he speaks with approval of the practice in Scotland, where the selection is made by the public prosecutor—the procurator-fiscal—"generally a skilled solicitor, nominated by competent authority, and not elected by scot-and-lot voters." In Scotland the cry is always for an extension of the coroner's court north of the Tweed, while in England the necessity of having public prosecutors is daily becoming more apparent. Both countries must borrow from each other; and while in Scotland too much is left in the preliminary investigation of crime to the caprice of a single official, there can be no doubt that in England the appointment and qualifications of the coroners admit of much improvement, and that public prosecutors would be a boon to the public, and would assist in repressing crime. One advantage would, according to Dr Taylor, attend the employment of experts. The public would be spared those displays of medical evidence where medical men have appeared in equal numbers on either side, and exposed themselves to the taunt that they made "a traffic in testimony."

"The scandal attending such displays as these, whether considered in relation to science or jurisprudence, may, it appears to me, be removed by the appointment of experts, as assessors to the court, or by giving a power to the judge to summon to his assistance independent scientific witnesses."

In conclusion we have to state that, like all Dr Taylor's publications, the present treatise is a most readable work, and an excellent specimen of clearness of expression and skill in condensation of illustrative cases. The subject is itself of absorbing interest; and although the volume is large and unwieldy, it is sure to command a large audience in both professions. A special feature is the introduction of woodcuts, representing forms of apparatus, crystals, blood, etc., and also anatomical diagrams for the use of non-medical readers.



Manual of Materia Medica and Therapeutics. Being an Abridgment of the late Dr Pareira's Elements of Materia Medica. By FREDERIC JOHN FARRE, M.D. Cantab., F.R.C.P., etc., etc. Assisted by ROBERT BENTLEY, M.R.C.S., and ROBERT WARRINGTON, F.R.S. London: Longmans: 1865.

OF the many works on *Materia Medica* which have appeared since the issuing of the *British Pharmacopœia*, none will be more acceptable to the student and practitioner than the present. Pareira's *Materia Medica* had long ago asserted for itself the position of being the most complete work on the subject in the English language. But its very completeness stood in the way of its success. Except in the way of reference, or to those who made a special study of *Materia Medica*, Dr Pareira's work was too full, and its perusal required an amount of time which few had at their disposal. Dr Farre has very judiciously availed himself of the opportunity of the publication of the new *Pharmacopœia*, by bringing out an abridged edition of the great work; and this now appears before us in the form of a handsome volume of about 600 pages. In order to reduce it to these dimensions, the editor has acted upon three rules:—

1st, To omit all such agents as the imponderables, mental and hygienic remedies, and to treat only of drugs properly so called.

2d, To omit all drugs which are not officinal, or contained in the *British Pharmacopœia*.

3d, To omit all classifications of medicines except the two adopted by Dr Pareira: namely, that founded on the chemical classification of the inorganic bodies, and on the botanical and zoological classification of the plants and animals which yield organic bodies, and that founded on their physiological effects.

There has also been a considerable saving effected by somewhat abridging the botanical and zoological characters, and, in many

cases, the description of the drugs themselves. While it was necessary to resort to many contrivances in order to save space, we think Dr Farre was mistaken in excluding some of the medicines which have not yet been introduced into the British Pharmacopœia. These are not, it is true, very numerous, or in general important; still there are a few, such as cerium and some others, which deserved notice.

This edition of Pareira is by no means a mere abridged re-issue, but contains many improvements, both in the descriptive and therapeutical departments. We can recommend it as a very excellent and reliable text-book.

Part Third.

PERISCOPE.

PRACTICE OF MEDICINE.

ON THE TREATMENT OF THE ACUTE DISEASES OF THE CHEST.

BY DR W. T. GAIRDNER.

W. R., a man twenty-four years of age, a miner, was admitted to hospital on 8th November 1865, and before I saw him had got from Dr Aitken a mixture of two grains of tartrate of antimony to eight ounces of water, a tablespoonful to be given every hour—that is, one-eighth of a grain of tartrate of antimony to be given every hour. On the 9th, I saw this patient for the first time with you, and we recorded the following:—"He is sent over from the fever ward, in which he has been for about twenty-four hours before admission to the medical hospital. Patient is obviously suffering from oppression of the breathing; respirations upwards of 40 in the minute, and performed with undue movement of the chest, somewhat of a gasping character; also some wheezing, but the breathing not noisy in proportion to its difficulty. Countenance rather pale, not livid. Pulse 116, soft. Expectoration, as preserved, is of almost pure and transparent mucus, rather viscid, with some froth, not very abundant." Now, although I think that according to the physical signs in this case there was ample evidence of disease of a portion of the lung, and I have very little doubt that the disease was essentially of the character of pneumonia, I do not uphold it in all points as a typical case of pneumonia; and one of the points in which it departed from the type was in the expectoration. Many of you are familiar with the rust-coloured expectoration in typical pneumonia, and you will appreciate the difference here as indicated in the report. The expectoration was composed of almost perfectly clear and transparent mucus. That is not the characteristic expectoration of pneumonia. On the other hand, it was also not the characteristic expectoration of bronchitis, inasmuch as it had not the abundant and frothy character of the expectoration of acute bronchitis. Neither was it the characteristic expectoration of phthisis, which, as you know, is purulent and globular. We cannot, therefore, quite clearly define the character of the acute disease in this case; but that it was a very acute disease, involving well-marked condensation of the pulmonary texture, there is, as you will see presently, no doubt whatever. To proceed with the report:—"Tongue

with a white fur, moist. Patient seems in a rather exhausted condition, and has been able to take but little food since admission; patient attributes his complaint to cold, and says that he was at his work till Friday, that is seven days ago, and in good health up to that time." To judge from the history, then, as very carefully recorded, we must assume this to be a disease of just seven days' standing. "Complaint began with a shivering, followed rapidly by cough and breathlessness" (that is exactly the way in which pneumonia very commonly begins). "There is reason to think, both from the external condition of the patient and his mode of expression, that he has been a good deal neglected. He is covered with scabies, and very dirty."

"On auscultation and percussion, necessarily (owing to the great exhaustion of the patient) performed with great rapidity, there is found a general though not very considerable sibilant râle, heard over all the chest."

Sibilant râle is more indicative of bronchitis than of pneumonia; but a degree of bronchitis very often complicates pneumonia, and a degree of pneumonia not unfrequently complicates bronchitis.

"Comparative dullness on percussion of the left side from about the middle of the back downwards; a somewhat diminished respiratory murmur in the same situation, perhaps (but doubtfully) tubular."

Those of you who are acquainted with chest diseases practically will know the value and significance of these signs. You have not here all the signs of pneumonia, but you have some of them, and you have distinct signs of condensation of the lung.

"About the middle of the back (that is, the left back), about three or four inches from the spine, a distinct crepitus, rather coarse and scanty as compared with *typical* crepitus of pneumonia, but not unlike it in general character. Elsewhere no distinct râle other than the sibilant râle above mentioned."

This crepitus was, or rather afterwards became, exactly like the crepitus of pneumonia. You recollect that I asked a gentleman from the Irish school of medicine who had been educated under my friend Dr Banks, of Trinity College, Dublin, to come forward, and to examine this case, for the purpose of showing you what would be made of it by a person educated in a different sphere of observation altogether. This gentleman at once, without the slightest hesitation, struck at the point I wanted. He said, "It is pneumonic crepitation."

"As regards habits, patient admits the habitual use of stimulants on pay-nights, but denies anything like habitual excess. Patient has not been well nourished hitherto, but expresses a wish for porridge and sweet milk, which he says he took last night with satisfaction."

Now, then, this is a rapid examination of the case as seen at the first visit. It was, however, as much as we could do with propriety, or even without danger, in the exhausted state of the man. Now came the question of treatment. I do not quite clearly recollect what I said at the bedside, or whether I thought myself at liberty, or had time, to express my mind to you at this visit with any great degree of fulness. But I know perfectly well what was in my mind, and therefore I am pretty sure that what I say now will not be inconsistent with anything that was said at the time. What appeared to me very plain, apart from all refinements of diagnosis, on examining this man, was, that it was a very nicely balanced case as between life and death. It was a case (let me forego the term "pneumonia" if you will) of very acute disease of the lungs—a case of disease so acute that after having lasted only seven days, having begun with a perfectly distinct shivering succeeded by fever, it had brought the man into a state very closely resembling that of typhus fever. He had, in fact, been admitted to the fever house under a medical certificate of fever given outside. I have not the shadow of a doubt in my own mind that to the eye even of an experienced physician, in a bad light, let us say, and in a dark, dismal, and neglected home, the case may have looked so like one of typhus fever that the mistake may have been practically unavoidable. It was not a case of fever certainly; but, with the remains of a rash on the person that caused something like a mottling of the skin, and of dirt, and filth, and neglect,

it is not all surprising that this was taken for a case of fever; and if of fever, then it was fever pretty far gone, fever attended by a very great amount of exhaustion and debility, and an almost entire prostration of the appetite for food. In fact, the whole character of the case was that of an extremely acute disease, joined with great exhaustion and debility. These were, as regards practice, the ruling facts of the case. Added to these you had certain of the symptoms of a very extreme form of chest disease. In particular, you had very considerable oppression of the breathing. But I did not fail to make you remark that debility and exhaustion were the ruling characters throughout.

Now, if this case had fallen into the hands of a physician of the "Sangrado" type—one thoroughly trained in and bigoted to the old "antiphlogistic" method of treatment (as it was called)—*i. e.*, bleeding and starvation, or systematic depletion of some kind,—he had a very good excuse in certain of the physical signs, and also in the name of the disease, whether we call it bronchitis, or pneumonia, or broncho-pneumonia, and, further, in the great amount of fever and respiratory oppression accompanying the disease; in all these undoubted facts, such a physician would have had a very good excuse for putting in operation the antiphlogistic treatment to the full extent, either in the form of bloodletting—which I suppose, however, any reasonable physician would have been cautious in using in the debilitated state of this patient,—or in the form of tartar emetic or other contra-stimulant remedy used in high and heroic doses. On the other hand, if this case had fallen into the hands of a practitioner of the more modern school of practice (but also not wholly modern, inasmuch as it is essentially the Brunonian practice of the last century revived)—the school in which a regulated and systematic but high scale of stimulation prevails in almost all acute diseases, and in all stages of them when attended by typhoid exhaustion, as in this case, I not only think, but am quite sure, almost as sure as if I had seen it with my own eyes, that this particular patient would have been stimulated up to the very highest point of possible stimulation. I have no doubt whatever that, in the hands of the late Dr Todd, for instance, or of his more immediate followers in London, or in the hands of many practitioners in Glasgow and the neighbourhood at this present moment, who do not profess, perhaps, to be followers of Dr Todd, this patient of ours might have been consuming, consistently with their principles and habits of practice, something like a bottle of brandy or of whisky a-day. At the very least, he would have been prescribed at the rate of from two drachms to half an ounce of spirits every hour or two; and if supposed to be in great danger from exhaustion (as we certainly supposed in this case), it would not have been long before the quantity would have been forced up to twenty or thirty ounces in the day. And observe, there was this to argue in favour of the practice in this man—that his habits, in all probability, had not been temperate before.

Now, having made this very guarded speculation as to what might have been the practice in this case, having made these statements in the most entire good faith, and without the slightest desire to exaggerate the peculiarities of the modes of treatment of which I was speaking—having said, in fact, nothing of which you will not find abundant illustrations in the books of the older authors as regards depletion, and in the book of Dr Todd as regards the stimulating practice, let us see what was, in the actual case before us, the practice pursued; what is, moreover, and has been for many years, the habitual practice with me in nicely-balanced cases like this. Observe in what sense I use the words "nicely-balanced." The patient, let us say, is in a state of danger from acute disease of the chest; hanging, as I firmly believe this patient was hanging, between life and death—a state of great danger, but a danger proceeding fully as much from the side of exhaustion as from the side of a respiratory oppression. In this state of great, but nicely-balanced danger, the treatment followed in the case was almost nothing else but the giving of *good nourishment*. By good nourishment I mean as much nourishment as the stomach will easily receive—nourishment carefully adapted to the weakened state of the digestive organs—nourishment of a good kind, easily assimilated, not pressed, but given in a

sufficient quantity; and with this we gave only a very little stimulant (chiefly guided by the fact that the patient was accustomed to take it), and that little with the view of aiding the assimilation of other nourishment, but not with the view of constituting a nourishment *per se*. I venture to ask you to adopt this principle, and even to write it in your note-books as a rule of practice, because, according to my experience, it comes as near an absolute rule as anything you can have, *that any strong form of alcoholic stimulant is to be used in disease with strict regard to the previous habits of the patient, and at all times in very moderate quantities, and with the view of aiding the nourishment by other things, but not of constituting a nourishment per se*. And in this nicely-balanced case, hanging as it was between life and death for some hours, we further thought it right, after most careful consideration, to give no medicine at all. Observe, however, that I do not wish you to take this, too, as a rule or principle like the former; nor did I take it as a rule in this case. On the contrary, I took great care to leave it quite an open question, and while myself prescribing absolutely no medicine, and even suppressing the medicine that had been ordered the day before, I left it quite open to Dr Aitken upon the appearance of any serious increase of the respiratory oppression, or any suffocation from retained expectoration, either to recur to the medicine of the day before, or to give carbonate of ammonia, with some stimulating expectorant, or to increase the quantity of stimulant according as he saw the indications arise. I gave him that direction deliberately, fully knowing that it was impossible at one single visit to judge of this patient's state in such a way as to rule wisely the whole practice for twenty-four hours, and that it was necessary, therefore, to leave a discretion in the hands of the resident physician.

You will observe, then, that our practice in this case was not founded upon any absolutely unbending rule or formula. It was not founded upon any exclusive favour for one or other nominal way of treating acute inflammations. It is not capable of being characterized by any single expression. It cannot be called distinctively an antiphlogistic practice, or an antimonial practice, or a stimulant practice, or an expectant practice. It is simply a practice moulded upon the particular indications, and pursued with due regard to the special features of the individual case, and even then with caution and watching of details, *experimentally*, as it were; but it is especially a practice founded on a belief in the powers of Nature to cure acute disease, and on personal experience of the great advantage, in nicely-balanced cases, of letting nature have fair play, without undue medicinal or other interference.

I will only add that the patient is now, I trust, convalescent; at all events he felt well enough to leave of his own accord within five days from his admission to our wards, and six or seven days from the time he was pronounced to have typhus fever, and sent into the fever house. The following is the remainder of the report in the journal. He had been ordered on 9th Nov. porridge and milk, beef-tea, and four ounces of whisky, to be taken in small quantities with his food, if relished.

"Nov. 10th.—Patient is obviously better this morning; expression much less anxious; pulse 88; respirations 33; a natural or rather considerable moisture on skin, the temperature of which is slightly elevated; tongue moist, less fur than yesterday; expectoration as yesterday, rather more frothy, no blood or pus (a single streak of blood was seen at yesterday's visit). The crepitating râle yesterday described is still audible in the same position, and with even more distinctly the characters of the pneumonic crepitus; more, however, that of resolution than of the early stage; with pretty distinct tubularity of respiratory murmur audible over a very limited space. Patient has taken his diet as ordered, and two ounces of whisky (he declined to take more).

"Nov. 11th.—Pulse 84 to 86; respirations from 24 to 25, nearly quiet; tongue cleaning and moist; expresses a wish for broth, and is allowed it; otherwise no change.

"Nov. 13th.—Discharged. Having been refused a pass, he insisted on going out, apparently quite convalescent; but, from the circumstance of his going without leave, not particularly examined before dismissal."

On what principles, then, did we regulate the practice in this case? First, why did we not bleed? I think that question is easily answered in the present day. We are none of us great bleeders in the present day. I am certainly not so, and have not been so for years. You may say, in fact, that, looking merely to the personal history of my own convictions and practice, I never was a Sangrado, or anything approaching to this; no, not even what some of my friends would have called a "moderate" venesectionist in pneumonia. If I have erred in this matter I am satisfied it has been on the side of caution on the whole; indeed, for several years after the great epidemic of fever in 1847-8, I think I absolutely never bled, and although not in theory entirely opposed to bloodletting, I do not remember to have found one single case to try it in until 1851 or 1852, when in a very few cases I cautiously tried it again. And since that time, although still extremely, even nervously, cautious in the use of this remedy, and extremely measured in my language as a teacher with respect to it, I feel pretty certain that my convictions have rather tended to ripen in favour of bloodletting than to go in the common direction of getting stronger against it. And the reason is, because I began practice with a full dose of scepticism as regards heroic remedies in general, and bloodletting in particular. I began practice in the year 1846, which was just about the time that homœopathy and other methods of inert practice had begun to make a great ferment in the world of opinion, and had thus induced thoughtful and dispassionate men to question the grounds of the old heroic remedies, and, as a matter of conscience and duty, to sift their doubts to the bottom. And thus, you may say, it happened that I was pretty well initiated in medical scepticism, or perhaps it would be more correct to say, had pretty well sounded the depths of medical unbelief as to strong or heroic remedies, and bloodletting in particular, before I was well out of my studentship. The consequence is, that I have not had to go through this process of gradual dissolution of fixed beliefs, from which many medical men have suffered such violent changes of opinion during the last twenty years. I have indeed much stronger beliefs now than I probably had twenty years ago. But these personal matters are not what I am here to speak about, and they are indeed of small importance. I only think it right to explain to you once for all my personal position with regard to many of the controversies of the last quarter of a century; not with the view of influencing your belief or disbelief in any of these remedies, but rather with the view of clearing off an old account, and being done with it. I do not think it part of my duty as a teacher, especially in the clinical lecture-room, to drag you through the mire of all these controversies. You will perhaps trust me to give you the results of what has passed through much sifting in my own mind; and if you can acquire the conviction that I am doing so honestly, it is far better for you in this stage of your studentship that you should not be obliged to go into these controversies at all. Therefore in what I say now I do not refer to any controversies that have been or are at present going on. I refer to results that have been gathering and accumulating with me in the experience of twenty years, and which have been gathered and accumulated to a great extent from *personal* experience, not without regard for the fair and just views and statements of others, but certainly without any disposition to enter keenly into mere verbal and doctrinal controversies, which are sometimes, no doubt, of great importance in the discovery of truth, but very often are conducted on principles by no means edifying to you.

Well, then, why did we not bleed? Because, to put it in as short words as possible, I have acquired from experience the conviction that bleeding, while still a remedy of great value for an extremely small class of cases, is a remedy which must not be thought of for the routine treatment of cases of inflammatory disease. You must at once remove from your minds altogether the idea that because a thing is an inflammation, therefore bleeding is the normal remedy for it. There is a class of cases, a small and limited class of cases, which I shall endeavour to point out if opportunity occurs, in which you will bleed still, and in which, if I do not mistake, you will bleed with if possible

increased conviction of the value of bloodletting. But that is a very small and limited, a very exceptional class of cases; cases of great danger, in which, however, the danger is not at all adequately represented by saying that the patient is under an acute inflammation, but rather by something much more complicated, and requiring much longer exposition. This was not one of that class of cases, clearly. It was an inflammation of course. It was even a very acute inflammation, culminating in a very brief period of days. Granted all that. But it was an inflammation in a subject the very opposite of the kind of subject in whom bloodletting is occasionally beneficial in great and rare crises. It was an inflammation in a patient who literally could not afford to spare one drop of his blood, and in whom anything of a weakening practice would have been in all probability fatal. And here let me say just one other word about bloodletting from this general point of view, the only further remark that I have time for to-day. *Bloodletting is never, in cases in which it is required or expedient, a weakening or depressing practice; and it is wrongly practised if done with that view.* Put that down as an aphorism. Whenever bloodletting proves a weakening practice, it is bad practice. If you ever bleed, and your patient does not feel even stronger than formerly twenty-four hours after the bloodletting, you have bled at a disadvantage, and probably wrongly. The object of bloodletting, therefore, is the very opposite of what used to be represented as the object of it. It is not to depress the circulation or to weaken the patient, and so to control the disease; but it is to control the disease and so to strengthen the patient by giving him the firm hold and right use of his remaining blood for the proper nutrition and support of the system under the disease. Now that is all I have got to say about bloodletting just now.

Secondly, Why did we not stimulate on the scale that some would have done? One would say, in a sense, that there was every inducement to do so in this man's case. I have reason to believe that he was by no means a temperate man; that he was a man to whom considerable quantities of spirits would have been no very great or unusual stimulus. Why did I not give him a great deal more? Why did I not give, say, a bottle of brandy in the day in this case? (I am trying to put everything in an aphoristic form just now. We shall have opportunities, no doubt, of expanding this by-and-by.) Because I have by experience acquired a very profound conviction that *alcoholic stimulants generally are not food; that they do not directly feed and restore the system; but that they do so indirectly, in so far as they minister to the power of the system to assimilate other and more real food.* I believe, in other words, that every drop of whisky or wine, but especially of whisky and of those stronger stimulants which do not contain the saline and organic matters that constitute part of the value of wine as a restorative agent,—every drop that you give to the patient beyond the quantity that assists in maintaining the digestion of ordinary food is, not to say lost, it is far worse than lost; it goes to poison the blood, just as much as if you were conveying into the system so much belladonna, or so much aconite, or so much opium. And, therefore, while I believe very decidedly in stimulants, as I believe in bloodletting; while I believe that they have a function, and a very great and valuable function, to perform in the cure of disease, you will be under constant temptations to transgress that proper function of them, unless you hold in view the caution that I am now giving you.

Thirdly, Why did we not give tartar emetic, or some other form of antimony, in this case? Not that I do not believe in antimony; for you shall see me give it, and I hope, before the session is over, that you will see me give it in circumstances that will convince yourselves as well as me of its great efficacy and value. But leaving all that question to be discussed hereafter, the reason that I did not give antimony in this case was because I have learned by experience that antimony, while most valuable in one class of cases, is a dangerous medicine in another class of cases; and in particular that in all cases of far advanced disease—in all cases where the nutrition of the body has been ill-supported; where the patient is emaciated and haggard-looking from the effects of acute disease; where the bodily powers are depressed and the tissues imperfectly nourished, and the

stomach has been for many days in a state not capable of assimilating nourishment, —in all such cases (I say) the indication of nourishment is paramount to all other indications, and certainly paramount to anything that you can get out of any drug, and more especially such a drug as antimony. Therefore you observed at the bedside how carefully I inquired into the circumstances of this man's case with a view to detect the state of his nourishment. The moment I found out that this man's nourishment had been in abeyance for a considerable time, I said, "We must give him no antimony until we see that his nourishment is attended to;" and the result of the case itself prevented me giving antimony after that, because the patient got better so rapidly that he did not require it.

Now the fourth question, on which I shall barely touch to-day, and which may be answered in few words now (we may have occasion to come back upon it afterwards), is this: "Why did I not, with this great amount of scepticism that you see underlying my faith in drugs, and which I do not care for a moment to conceal from you—why did I not at once, honestly and manfully as you would say, go into the position that drugs are utterly useless, and instruct you to give no drugs at all in such cases as this, and systematically to put them all away?" For this plain reason, that I do not believe that drugs are utterly useless; that I believe in them still with an undoubting faith, and a faith all the more undoubting that I have gone through the whole process of mind of this scepticism long ago, and have come round again to the conviction that drugs are very useful in some cases; although no doubt, as I trust I shall never be afraid to tell you, they are often given, even by regular physicians, to the very great and serious injury of the sick. It is our great privilege in medicine, Gentlemen, to be able to look one another in the face, and tell each other exactly what we think, without the danger of being brought to book before any conclave of authorities, or by means of any formula of opinions. We are not fettered by dogmas, or bound down by confessions of faith; and therefore I can always speak to you freely and without reserve, not being in the least bound to appear to believe more than I do actually believe. This is our greatest and most precious privilege in the practice and teaching of medicine; it should give us a very solemn sense of responsibility in the handling of medical truth, which is not ours nor of our making, and which we have absolutely no right therefore to employ for party purposes or for personal glorification, but only with a constant and watchful regard for the interests of the sick who are committed to our care. With respect to this matter of giving active remedies, therefore, I desire to be at liberty to give them or not—to tell you that I believe in them or not, according to the circumstances of the particular case, and without the least prejudice to the general doctrines that may be taught in class-rooms here or elsewhere, or expounded in books, or bandied about in controversies, all of which have their proper place and sphere, but all of which, in the clinical wards and class-room, are superseded by a more direct and simple manner of teaching.—*Lancet*.

LOCOMOTIVE ATAXY—TREATMENT BY IODIDE OF POTASSIUM—PARTIAL IMPROVEMENT. BY DR ARCHIBALD REITH, ABERDEEN.

THE following cases are the first cases of locomotor ataxy which we have published in our Hospital Reports. They will be read with interest, as the disease is now exciting considerable attention in this country. Trousseau has given a most interesting account of progressive locomotor ataxy, and it may now, we are glad to say, be read in English. Dr Bazire has just published the first part of his translation of the Professor's lectures. Dr Bazire has added to the lecture on locomotor ataxy a most valuable record of cases observed by himself at the Hospital for Epilepsy and Paralysis, with important observations thereon.

CASE 1.—James M., æt. 39, plasterer, was admitted into John Forbes' Ward, March 23, 1865.

Previous History.—His health generally has been very good, and he has been very temperate in his habits. He has never had any disease with the exception of measles, and whooping-cough in childhood. About three years ago he

passed water very frequently for a week; but he did not feel ill at the time, nor did he observe anything peculiar in the urine. His father died of cancer of the penis, aged 72, and his mother of natural decay, at the same age. Other near relatives healthy. He declares that he was never addicted to venereal excess of any kind.

History of Present Illness.—In the month of November 1863, without any known cause or premonitory symptom, he began to lose the power of his right leg. The weakness seemed to commence at the ankle, and gradually crept up to the knee, and was accompanied by partial loss of feeling and a sensation of deadness. About two months subsequently, the left leg became almost suddenly in the course of one day similarly affected, though not to the same degree. There has been no change in condition for the last fourteen months. He has used a few simple remedies, but without benefit; and he did not persevere in their employment, because he thought he would recover spontaneously.

Present Condition.—Body well nourished, firm, and wiry. Skin natural. Feet habitually cold.

Nervous System.—He had no pain in the head nor giddiness. Consciousness and memory perfect. The mouth is drawn slightly to the right, best seen when he smiles. He is sometimes troubled with jerking of his legs. When he attempts to walk, he feels as if he had got a blow on the thighs, and his legs suddenly start up. If he attempt to put one leg before the other, it is sometimes jerked outwards or forwards, and planted rather forcibly on the ground without his control. His gait is that of a drunken man; but although he staggers forward in a rather precipitate manner, yet with the assistance of a stick he manages to get along without falling. When his eyes are shut, however, or when he has no support, he can scarcely move a step without falling; and if his two feet be placed parallel close together, with his eyes shut at the same time, he falls over as if he had no idea of the perpendicular, and has to be supported. He does not think his legs have wasted since the commencement of his attack. Speech quite unaffected. No abnormality in the spine.

Organs of Sense.—The left pupil is a little larger than the right. Taste unimpaired. There is partial anæsthesia in the right leg. He has sometimes stinging pains in his thighs apparently depending on the feelings above mentioned. Respiratory, circulatory, and digestive systems healthy. Urine natural.

The treatment consisted in the administration of iodide of potassium in doses of ʒj. daily. The facial paralysis disappeared; but when he left the Hospital, on April 17, he did not appear to walk better, although he himself thought he could.

CASE 2.—Jane S., æt. 19, domestic servant, unmarried, was admitted into Ruth's Ward, May 11, 1865, complaining of inability to walk.

Previous History.—She has not had robust health since her childhood, when she suffered from scarlatina, typhus, and "remittent" fevers. She had inflammation of the eyes shortly after birth, and they continued weak for four years afterwards. Her mother died four years ago of cancer of the womb; her father is still alive, and in good health: but he has had three or four attacks of transitory paralysis of one side of the body. Menstruation is regular.

History of Present Illness.—About eight months ago her right knee swelled and became painful. She was treated with tinct. iodin., applied externally; but after some improvement the knee again got worse, and she was admitted into the surgical wards of this hospital about a fortnight ago. The swelling of the knee gradually subsided, and she was then transferred to the medical wards. She now complained of inability to walk, the loss of power being so gradual that she does not remember when she first felt it. She thought that the state of her right knee was the cause of it until it began to appear in the left leg. Before she became unable to walk she had a feeling of weakness across her back, latterly amounting to a dull aching pain. She has suffered for the last two months from a constant headache, like a feeling of weight on the top

of the head, with a darting pain, sometimes over the right side near the brow. She feels her sight getting dim, and her eyes growing tremulous.

Present Condition.—Stout and well-made, though not robust. Hair and iris dark. There is slight swelling of, and a feeling of stiffness in the right knee, and also in the ankle.

Nervous System.—She complains of severe headache, almost constant, like a weight on the top of her head, and of pain of a sharp darting character over the right eyebrow, and over the right side of the head. There is pain behind her eyes, mostly in the right. She complains, also, of a dull aching pain across the loins. When she walks, she staggers as if she were drunk, and would often fall if not supported. She cannot walk at all with her eyes shut, and would instantly tumble down if she had nothing to lean upon. When her feet are put together, and her eyes closed at the same time, she immediately falls over. In bed, however, she moves her legs quite well, and has so much power in them that several persons holding them cannot prevent her from moving them. Sensation is unimpaired in both limbs.

Organs of Sense.—There is a tremulous motion of the eyeballs from side to side; no difference between the pupils, which contract equally to the stimulus of light. Her sight has lately become dim and misty, but she sees moderately-sized type perfectly. Respiratory, circulatory, and digestive systems healthy. Urine healthy. Catamenia regular.

Large doses of iodide of potassium were given till July 4, when she left the hospital relieved of her headache, but with little perceptible improvement in the walking.

Clinical Remarks.—Locomotive ataxy is commonly mistaken for paraplegia. The features of each are so distinct that it is surprising physicians did not sooner recognise the difference. Duchenne's vague definition of locomotive ataxy is, "progressive abolition of the co-ordination of movement, and apparent paralysis, contrasting with integrity of muscular power; these are the fundamental characters of the disease." Trousseau's is more definite, but commits science to what is yet but theory—"a spasmodic neurosis characterized by a want of co-ordination of the voluntary movements, often complicated with disorders of sensibility and partial paralysis." The chief feature of the disease, when fully formed, is, more or less, inability to perform the reflex functions of standing or walking. On attempting to walk the patient finds his legs refusing to obey the ordinary co-ordinating power; they are thrust outwards or forwards spasmodically, and planted on the ground in an irregular way beyond the control of the individual, who is in constant danger of losing his equilibrium. If he succeeds in getting a start, he moves forward in a precipitate manner, staggering and stumbling, and is obliged to use a staff to prevent himself from falling. A paraplegic patient, on the other hand, if able to assume the erect posture, drags his legs slowly, but uniformly along, his difficulty being not to control the movements of his legs, but to get them to move at all. The most characteristic differences between locomotive ataxy and paraplegia are the following:—First, *in bed*, the patient with the former affection has complete power over the voluntary movements of the legs, as was well seen in the two cases above recorded, both of whom tossed about their legs with the utmost facility, and defying all attempts to hold them, while in paraplegia the power of voluntary motion is in a greater or less degree lost; secondly, in locomotive ataxy, if the eyes be shut, the patient can no more walk, or even stand with his feet together, than a person can stand or walk on stilts who has never been accustomed to them, while nothing similar is observed in paraplegia, or indeed any other kind of paralysis. In fact, a patient with ataxy closely resembles a tyro endeavouring to walk upon stilts. These diagnostic marks are, therefore, very important, and not to be overlooked. They were well seen in the above cases. A woman affected with paraplegia was brought beside the girl S., and the differences between them were most striking.

Locomotive ataxy seldom comes on without premonitory symptoms. The chief of these are rheumatic pains, partial and temporary attacks of paralysis,

and disorders of urinary and generative organs. The pains are irregular, coming and going, and are generally mistaken for rheumatism. They were so in the girl S.; but that they were not rheumatic was at once evident from the fact that they were confined to one knee and ankle—a point of some importance, for rheumatic pains of this kind are mostly due to lesion of the nervous system. There does not appear to have been any other premonitory symptom in this girl, except amaurosis, which was, however, but slight, and the excessive pain on one side of the head. Amaurosis is a very common symptom, as also paralysis of the third and sixth nerves, and indeed of any nerve. The paralysis of the ocular nerves is, in general, more persistent than that of others. Transitory attacks even of hemiplegia may occur, and it is singular that the father of the girl S. was thus affected, although there is no history of ataxy in him. The premonitory symptoms in the man M. were partial paralysis of the left facial nerve, and at one time what looks very much like incontinence of urine. He also had cutaneous anæsthesia, which is a frequent attendant on the disease.

The usual post-mortem appearances found after death in these cases are grey degeneration of the posterior columns of the spinal cord, and atrophy of the posterior roots of the spinal nerves. So long as the posterior columns were believed to be the channel for the transmission of sensitive impressions to the brain, this state of parts existing with little or no loss of sensation, but with loss of the reflex power of standing and walking, was rather perplexing, especially as the anterior columns were free from disease; but since the experiments of Brown-Séquard have shown that the posterior columns have not the office formerly assigned to them, and, moreover, that section of them causes hyperæsthesia, instead of anæsthesia, together with locomotive ataxy, the pathology is more intelligible. But how does it happen that with atrophy of the posterior roots there is often little or no loss of sensation? The explanation given is, that the hyperæsthesia which would be produced by disease of the posterior columns alone is counteracted by the atrophy of the posterior roots, in which some nerve tubes remain intact, sufficient to account for the persistence of sensation. This may be true, but it is not quite satisfactory. It is to be regretted that the state of the sympathetic nerve has not received the attention it deserves. There seems evidence to show that that nerve has more to do with reflex motion than has generally been supposed. Last year M. Voisin presented to the Société de Médecine de Paris a case of locomotive ataxy of the upper extremities where the cervical sympathetics were found diseased.

An attempt has been made to distinguish locomotive ataxy caused by disease of the posterior columns from that caused by cerebellar disease. I am afraid this will be found impossible in practice, at least in many cases. It may be possible to diagnose the locality of the lesion, whether in the cerebellum or in the spinal cord; but that both varieties belong to the same category I have no doubt. For, in ataxy from spinal disease, we also find sometimes structural change in the optic nerve, the optic tract, the corpora quadrigemina, and the motor oculi, without disease in the cerebellum; and yet in some cases of so-called ataxy, the only lesion found has been cerebellar. The ataxy likewise is not confined to the legs, but may proceed to the upper extremities, or even affect them solely, as in Voisin's case just mentioned. The cerebellar and spinal diseases are therefore probably varieties of the same evil, the ataxy being the symptom. And here I may be permitted to quote some observations of Dr Hughlings Jackson. Speaking of cases of paralysis of one or more cranial nerves, with paralysis of both legs, and no paralysis of the arms, except now and then a little paralysis of the hands or fingers, he says:—"One of three inferences may be drawn from these facts. 1. The symptoms may by some be considered to occur together by a coincidence. 2. That the paralysis of the cranial nerve is the result of reflex action, set up by the disease of the cord which produces the paraplegia. This is the explanation given by Dr Brown-Séquard. 3. That there is a tract in the higher part of the nervous system which contains nerve-fibres from the legs and not from the arms, or, at least, only from the fingers or hands. Some of these cases resemble, and perhaps

are, locomotive ataxy. There is in some a loss of power in walking properly, whilst the legs seem to retain considerable power. This, of course, applies only to those whose sight is merely impaired. When the guiding power in the legs is lost, we use the eyes to direct our movements. Even in health "we lean," says a distinguished physiologist, "on our eyesight as on crutches." Patients who have ataxy, therefore, cannot walk in the dark, or when their eyes are shut. Blindness, following loss of guiding power in the legs, seems then to be not so much a complication as the loss of a similar faculty. But, when the patient has lost both these helps to guide himself, it is just possible that he may struggle his way from object to object by touch. But a third factor in the general power to guide movements is frequently affected also, there is numbness of the fingers; so that the power to combine movements in the legs, or to do so in a supplementary way by help of the eyes, or of touch, is lost altogether. This, I think, renders it plausible that there may be in the higher parts of the nervous system a tract of nerve-fibres passing from the legs, fingers (tips) to the corpora quadrigemina for the general purpose of guiding movements in progression."

The prognosis of locomotive ataxy is bad. Patients, if they do mend for a little time, mostly go from bad to worse till they can no longer maintain themselves in the erect posture, and die cachectic. Various remedies have been tried. Electricity and nitrate of silver are favourite remedies with some, but not much confidence can be placed in them. There is this comfort, that the disease may remain stationary for many years.

Note.—The second patient has just recovered from an attack of typhus. The symptoms of ataxy are more developed, but there is now some loss of voluntary power in the legs, probably from extension of the disease to the grey matter.—*Lancet*.

PROGRESSIVE LOCOMOTOR ATAXY, FOLLOWING ANGINA DIPHtherITICA.

BY DR M. JENSEN.

A MAN, aged 31, who had previously had good health, came under treatment for a deep diphtheritic ulcer of the right tonsil, which caused pain in swallowing, troublesome accumulation of mucus in the throat, and was accompanied with cardialgia, and suddenly occurring paroxysms of suffocation, coming on especially at night. Three weeks after the diphtheritic affection commenced he had a fall from giddiness, and during the next eight days he became very feeble, and impairment of memory, heaviness of the legs, and a dragging, staggering gait came on. He walked better quickly than slowly, and worst in the dusk, as he required to see his feet in order to direct the movements of them. Then sight began to get dim, and after fourteen days everything appeared as if shrouded in a thick mist; but there was neither strabismus nor double vision, nor any drooping of the upper eyelids; the pupils were normal, and the iris freely mobile. Deglutition ceased to be painful, but became difficult, food returning by the nose. The soft palate hung loose, and flapped on deep inspiration. The ulcer in the still swollen tonsil was deep, but clean. Some days later taste was lost for all but sweet things, a solution of quinine not tasting at all bitter. Peculiar pricking and shooting pains affected the middle of the hard palate and the right corner of the mouth. The nape of the neck was sensitive, and the head could not be held up straight, the muscles of the nape having lost power. The giddiness increased, and he became very drowsy. Later the pricking pains affected the ulnar border of the right hand, and the tips of the fourth and fifth fingers, and the sensibility of those parts was diminished. The voice became highly nasal. He could no longer see his feet, and his gait became extremely uncertain and staggering. There was obstinate constipation, and though laxatives were taken daily the bowels were relieved only once in four or even six days.

On the 22d of September sight returned quite suddenly, after having been lost for a month. Some days later memory was restored, and the giddiness, the drowsiness, and the pricking pains in the gums and corner of the mouth all

ceased. But as the cerebral symptoms disappeared, affection of the spinal cord rapidly increased. Pain fixed itself in the upper vertebrae. The sensations of formication, cold, and deadness in the hands and feet changed in fourteen days into complete anaesthesia. Everything felt to the patient as if his hands were covered with woollen gloves. He could not take up small objects, nor hold fast larger things when given him; he could neither write nor convey food to his mouth; but he could distinguish between heat and cold, and the latter seemed to rather increase the sensibility. In the feet and legs up to the knees he felt an icy coldness, and he could only just feel the ground under his feet; he could neither stand nor walk alone, for he felt as if rocking constantly to and fro. Peculiar convulsive movements of the fingers and toes now came on, some being extended, others flexed or abducted, the movements being altogether beyond his control. He could still taste sweet things, but nothing else. To this was added complete insensibility inside the mouth; difficulty of deglutition continued. If in the dusk he folded his hands together, he could never separate them till light was brought so that he could see the positions of the fingers. In the beginning of October, the delusive rocking motion ceased, and the patient could no longer, when he put down his feet, feel the ground; he was obliged always to sit or lie; he could not rise up without aid, and when lifted up, his legs would not support him; but while lying he could stretch out the legs with tolerable force; he could give a fair squeeze of the hand, and could swing the arms backwards and forwards, but was not able to lift them up. At this time the dysphagia suddenly quite disappeared, so that the patient could again satisfy his imperious appetite. The impulse to eat came on suddenly, and if he could not quickly satisfy it he grew faint. The swelling and ulceration in the tonsil had disappeared, but the voice remained nasal. In the middle of October the icy coldness of the legs ceased, giving place to an agreeable sensation of warmth. In the latter half of the month, however, the paralysis of the lower half of the body reached its highest degree, and the pain in the back was felt lower down towards the loins. For some days the patient could not feel that he sat, and had no sensation in the genitals. The government of the lower extremities was entirely lost, and when the patient was held up on both sides he dragged his legs after him as if quite inanimate; yet he could, when lying on a sofa with his legs up, stretch them forcibly out. He sat always bowed extremely forwards, and could only raise himself straight for a moment. Oedema of the feet came on; the urine was clear and frothy, but contained neither sugar nor albumen. In the middle of November the above-described symptoms began to gradually disappear, so that in December all the functions were again normal; only some difficulty in writing remained. By the end of the year, however, this had ceased; he was perfectly well, and had gained flesh and strength.

The treatment "did not differ from that usually adopted in such cases." It may be mentioned, however, that the patient through a long period took strychnine; $\frac{1}{8}$ th of a grain, cautiously increased to $\frac{1}{4}$ th, morning and evening, every twelve days, discontinuing its use for four days.—*Hospitals Tiende, and British and Foreign Med.-Chir. Review.*

TUBERCLE IN THE BRAIN.

PROFESSOR DUCHEK has published, as the first of a series of "Studies of Diseases of the Brain," three cases of tubercle in that organ—

1. *Tubercle in the Pons*.—Besides tuberculosis of various other organs, there was found in a young man a tubercle an inch in diameter in the left half of the pons. The surrounding brain was softened, and there was exudation on the membranes. The symptoms corresponded to the well-known type of the disease. At first, gradually increasing paralysis of the right leg, later of the right arm and left facial nerve, and perhaps of the palpebral branches of the oculo-motor nerve. Sensibility was diminished in the left half of the face, and mastication interfered with through paresis of the masseter muscle. Electrical contractility was weaker in the affected muscles than in the corresponding ones

of the opposite side. Pains, spasms, and stiffness, preceded the paralysis in the extremities, paresthesia accompanied it. Consciousness remained untroubled throughout the course of the illness; death followed in a year from its probable commencement.

2. *Tubercle in the Corpus Striatum*.—A child, six years of age, was affected with chorea-like movements, at first in the right half of the face, and these soon spread to the neck, shoulder, arm, and leg of the same side. By degrees the affected parts became weaker, and at length wholly paralyzed. Contraction occurred only in the muscles of the nape; sensibility of the skin, and reflex sensibility, were increased. The perceptive faculties were normal. Micturition very difficult in the day time, involuntary at night. The intellect was latterly disturbed and clouded. The illness lasted six months. A tubercle, the size of a hazel-nut, was found in the left corpus striatum; and one, the size of a bean, in the upper wall of the fourth ventricle, and many smaller ones in the cortical part of the brain.

Duchek attributes the contraction of the muscles of the nape to the tubercle of the fourth ventricle.

3. *Tubercle in the Cerebral Hemisphere*.—A man, eighteen years of age, who worked very laboriously with the right arm, was suddenly seized with clonic convulsions of it, shortly followed by a like affection of the face and loss of consciousness. At first the convulsions recurred in the same way every fourteen days, later irregularly, even many times a-day, but without loss of consciousness. Soon tonic spasms alternated with them. For some months all symptoms of illness disappeared; but then they broke out anew, and seized also on the right foot, appearing for the most part only in one extremity; they also once transiently affected the left foot. Burning pain in the forehead preceded the attacks. Coming on again after a second pause of some months, marked paresis of the right half of the face and of the right extremities was noted. The paroxysms lasted till a short time before death, about two years after the first attack, the patient dying, tuberculous, of marasmus. On post-mortem examination there was found in the left cerebral hemisphere a wedge-shaped tubercle, which, from its broad base at the cortex and attached membranes, extended an inch and a-half downwards in the substance of the brain towards the optic thalamus; it was enclosed by a richly vascular membrane; the cerebral substance round it was of a pulpy softness. Elsewhere the brain was healthy.—*Wien. Ztsch. (Med. Jahrb.)*, and *British and Foreign Medico-Chirurgical Review*.

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLV.—MEETING IV.

3d January 1866.—Dr MOIR, *President*, in the Chair.

The President, in taking the chair for the first time, returned thanks to the Society for the honour they had done him.

I. PURULENT PHLEBITIS.

Dr Sanders showed specimens from a case of phlebitis. The patient, a woman 41 years of age, was admitted into the Royal Infirmary on the 8th December 1865, from the Maternity Hospital, where she had been confined on the 13th November. After her delivery there had been no particular symp-

toms, but she had not made a good recovery; her health did not return, and at length she was attacked with fever of a low type. When admitted into the Infirmary, she was evidently seriously ill, and symptoms of phlegmasia alba of the left leg had set in. The limb was swollen throughout, was of a white colour, and the tension was such that it did not pit on pressure. On examining the vessels, the arteries were found pulsating feebly, and the vein was felt thickened and harder than natural. The general symptoms of pyæmia were present; bed-sores became formed; the patient sunk, and died on 23d December. On post-mortem examination a metastatic deposit was found in the lower part of the right lung, partly solid, partly softening into gangrene; and there was purulent pleurisy of a low type on the same side. The most interesting point in the case was that the pelvic veins were found inflamed. On tracing down the inferior vena cava it was found healthy. The right iliac veins were also healthy. The left iliac was the seat of disease as well as the veins below it as far as the ankle. This was evidenced by the thickening of their coats. On laying them open the lining membrane was found mostly smooth, but in some places softened and as if abraded; it was coated with colourless lymph or fibrin. The vein at its upper part was occluded by a colourless clot, partly puriform, from which embolic particles would readily be detached and carried by the blood from the pervious right iliac into the vena cava. Below this, in the external iliac and femoral veins, was liquid yellow pus; at some parts of the veins were small abscesses in the external and middle coats. Some of these had burst into the vein, others had remained circumscribed. The case, Dr Sanders remarked, was interesting, as illustrating a point of considerable pathological interest. It showed clearly inflammation of the whole texture of the vein. Inflammation of the internal coat (endophlebitis) had been denied, while meso- and peri-phlebitis were generally admitted; but this was an example of a class of facts which were of great importance in clearing up the question. The uterus was found to be still enlarged; the veins also were enlarged, and in many places contained puriform clots. On laying open the uterus the interior was found coated by a fluid of reddish soft substance, and the patent openings of the veins could be seen. Some of them were filled by clots of the same puriform character as that in the iliac vein. This appearance was particularly distinct at the part where the placenta had been inserted.

II. CASE OF DILATATION OF THE BRONCHI.

Dr Sanders stated that the patient, from whom the specimen he now exhibited had been removed, had been admitted into the Royal Infirmary on 3d December 1865. He was a man of 44 years of age, with a deformity of the spine, the result of Potts' disease in his early youth. When admitted there was dropsy, the result of disease of the kidney, the urine being albuminous; there was also obstinate diarrhœa and vomiting, with great tympanitis, and a little ascites. Towards the close pus appeared in the urine. This, taken in connexion with indications of the tubercular constitution, led Dr Sanders to believe that there was scrofulous abscess in addition to the probably waxy degeneration of the kidney. A peculiarity in the patient was "clubbing" of the distal joints of the fingers and convexity of the nails, which his friends called "hazel-nut" nails. On examining the lungs, the left was found to expand freely; the right was evidently dense posteriorly. There was dull percussion, with signs of cavities, gurgling, and bronchophony almost amounting to pectoriloquy. As there were no signs of active pulmonary disease, Dr Sanders thought it probable that these signs depended upon the presence of bronchial dilatations or fistulous cavities, the result of former tubercular disease. At the post-mortem examination the appearances found agreed very well with what had been observed during life. The kidneys were enlarged and pale, the principal degeneration was fatty, although the Malpighian bodies and vessels were in the early stage of the waxy condition. There was a scrofulous abscess in the right kidney, and scrofulous disease of the pelvis, extending a few inches down

the ureter. The liver and spleen were slightly waxy. There were tubercular ulcers in the small intestine. The left lung was hypertrophied, and displaced the heart somewhat; the right was diminished in size; a good deal of the tissue of its upper part was healthy, mixed with a deposit of disseminated tubercle. The anterior portion of the lower part was healthy; the inferior and posterior portion was condensed. On cutting into it a number of round cavities were brought into view. These were of the size of a walnut, a hazel-nut, or smaller. They were generally spherical, were lined with a smooth membrane like a mucous membrane, and their walls consisted of fibrous tissue. In the part of the lung where they were found there was no vesicular texture, the pulmonary tissue was entirely fibrous. Dr Sanders remarked that this was a specimen of what is called cirrhosis of the lung, with dilated bronchi. The dilatations were some unilocular, some multilocular; all communicated with bronchi or with each other. It was interesting to notice that sometimes the bronchus led directly into a cavity, sometimes communicated with it by a small lateral opening. Some of the cavities seemed at first to be quite closed, but in every case a small opening communicating with a bronchus was found. In the interior part of the lower lobe, where the pulmonary tissue was generally healthy, there were simple bronchial dilatations. The bronchus could be traced, and was found to become thinner and to expand into a sort of fusiform dilatation. The middle lobe of the lung was in a curious condition. It was evidently compressed, emphysematous and atrophied, and seemed entirely composed of interesting fibres like the tubercular tissue of the spleen or the corpora cavernosa. At one part it consisted of smooth fibrous tissue like cicatrix. Dr Sanders observed that the case was most interesting as showing what so-called bronchial dilatations were. Here this was clear evidence of tubercle. The mode of development of the cavities appeared to be this. Tubercle was deposited, became encysted, softened, and was expelled by expectoration. The cavity became then lined by membrane, and remained unaltered. The preparation exhibited showed this very clearly. A little piece of tubercle, the size of half a filbert, was encysted, but did not quite fill the space; at one point the cavity was empty, lined by smooth membrane, and was found to communicate with a dilated bronchus. The subject of dilated bronchi was one of considerable difficulty: some thought all were formed by simple dilatation of bronchi; this case proved that they might be formed in this way, or that they might be the remains of tubercular cavity. They might also in other cases, however, be the remains of cavities which were not the result of tubercle. Dr Sanders had observed cases of a peculiar form of broncho-pneumonia, characterized by copious purulent expectoration, often presenting a peculiar fetor, in which dulness on percussion and the physical signs of a cavity or cavities were present usually in the lower lobe of the lung. On examination after death, the bronchial mucous membrane was softened, in parts perforated as if by ulceration; the bronchi were dilated, and there were irregular cavities formed by ulceration (something like gangrenous cavities but without the gangrenous slough or fetor), with condensed tissue of the lung (non-tubercular phthisis). The kind of condensation is peculiar, not granular as in ordinary pneumonia, but denser, such as has been called "fibroid" or "interstitial" pneumonia. The cavities communicate with the bronchia.

III. URETHRAL CALCULUS.

Dr Joseph Bell showed a specimen of urethral calculus which he had lately removed. The patient had had no particular symptoms until five weeks before he applied at the hospital. At that time he began to suffer from retention of urine and severe pain near the neck of the bladder. He applied to a surgeon who introduced a small instrument, and told him he was suffering from stricture. The patient then came to the Infirmary, and, in the absence of Professor Syme, Dr Bell passed a number twelve catheter, and came upon a calculus lying in front of the prostate, which could also be felt from the rectum. As the urethra was very dilatable, Dr Bell introduced a larger instrument, passed it down to the calculus, and by a little manipulation worked the calculus forward till it

was well past the scrotum, guiding the calculus by the bougie. As it now began to catch, Dr Bell thought it better to cut down upon it, and accordingly made an incision in the middle line. There were no bad symptoms; only a drop or two of urine passed by the wound. The patient only kept his bed four days and had no further urinary symptoms.

IV. INSANITY CAUSED BY SUNSTROKE AND BY INJURIES TO THE HEAD,
WITH ILLUSTRATIVE CASES.

Dr Francis Skae read a paper upon this subject, which will be found at page 679 of this number of the Journal.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXV.—MEETING II.

29th November 1865.—*Dr WEIR*, *President*, in the Chair.

I. HÆMORRHAGE.

Dr Cairns read a paper on two cases, one on "Accidental Hæmorrhage, complicated with Convulsions," the other on "Profuse Recurrent Hæmorrhage, due to Retention of part of Placenta." The first of the two cases which I have the honour of bringing before the Society this evening, refers to a patient whom I attended about three years ago. The patient was a primipara, 23 years of age, of the lymphatic diathesis, and affected with partial hemiplegia of the right side, under which she had laboured from the period of childhood. The pelvis was somewhat narrow antero-posteriorly. She had enjoyed excellent health throughout the whole course of her pregnancy, which extended to the full period. When summoned to attend her in her confinement, I found on my arrival that labour had commenced four hours previously, and that during the last two hours there had been a constant discharge of blood. The pains were weak, had no influence on the os uteri, and recurred at regular intervals of fifteen minutes. The pulse was 80, and rather weak. A cold-water enema was administered, and a tight binder applied to the abdomen, and the hæmorrhage ceased shortly after. This was at six o'clock A.M. I then left her for three hours in charge of a pupil, and at the end of that time I found on my return the labour progressing slowly. Two hours after, the hæmorrhage recurred, when another enema of cold water was given, and the os being now dilated to the size of a half-crown, the membranes were ruptured, and the head was found presenting in the first position of Nelligan. After this the hæmorrhage again ceased, and labour went on favourably for four hours, when a convulsive fit suddenly supervened. The usual remedies were immediately employed, but without effect. There was no œdema of the limbs, and no albumen in the urine. The convulsions recurred every few minutes. At this stage I immediately called in the aid of Professor Simpson, who, finding the os sufficiently dilated, applied the long forceps, and in the course of a few minutes delivered the patient of a living female child. The placenta on being examined was found to contain a rupture, which extended from the circumference to near the centre,—a circumstance quite sufficient to account for the hæmorrhage. The patient went on favourably till the third day, on the evening of which her husband called upon me in a state of great alarm, exclaiming that his wife was dead. On my arrival, I found her in a state of syncope from loss of blood. After clearing the room of a number of people who had clustered round her bed, and admitting a supply of fresh air, she recovered in a few minutes. The hæmorrhage having ceased by the time I arrived, I simply ordered her half a glass of brandy, to be repeated occasionally until I should call on the following day. From that date there was a daily recurrence of hæmorrhage for about a fortnight, despite the employment of all the usual remedies. Feeling at my wit's end, I ultimately injected a quantity of warm

water up the vagina, and immediately thereafter administered an enema of cold water, thinking that by first producing expansion by heat, and then sudden contraction by cold, favourable results would be likely to follow. I was not disappointed, for although after the first application of this sort the hæmorrhage returned, it was very slight, and after two more applications at intervals of two hours, it entirely ceased, and the patient from that date made a rapid recovery.

The next case which I have to notice occurred towards the latter end of last June. The patient is 25 years of age, of slender habit, and the mother of three children, including the one last born, which she carried to the full period. I waited on the patient the instant I was summoned, but I nevertheless found on my arrival that she had been delivered four hours previously. Labour had come on very suddenly while she was engaged in her ordinary household duties, and the child was expelled on the floor by two pains, the one following the other in close succession. She managed to procure the assistance of her "next door neighbour," who, according to her own statement, had had large experience in lying-in cases. When I was sent for, the patient had fainted, but by the time I arrived she had recovered, brandy and water having meanwhile been administered. The pulse was regular and of fair strength. I found the under bed-clothes saturated with blood and large coagula, which half-filled an ordinary-sized chamber-pot. There was also, "per vaginam," a constant but small discharge, which gradually ceased after a dose of ergot, and the application of firm pressure on the uterus, which I found in a well-contracted condition. After seeing the patient in a safe and comfortable state, I asked the woman who had acted as accoucheur to show me the placenta. She informed me that she had disposed of it in the usual way. I then requested her to tell me candidly if it came away of its own accord, and in an entire state. She assured me it did. I am sorry to say, that I had somehow or other great doubts of her veracity, and, as the sequel will show, with good reason. The patient went on favourably, as in the former case, till the third day, when alarming hæmorrhage again recurred. It then recurred on the ninth, then on the sixteenth, and lastly on the twenty-fourth day after delivery. By this time the patient was perfectly exsanguine, and reduced to such a state of weakness that she could only speak in a whisper. On every occasion of hæmorrhage I made an examination per vaginam, but could detect nothing abnormal except on the last occasion. I then found a soft, spongy substance, protruding from the os uteri, which on removal and examination turned out to be a small portion of the placenta. From that date no more hæmorrhage occurred, and the patient gradually improved. On questioning the patient in the course of her recovery, I learned that her attendant had forcibly removed part of the placenta while the patient was on the floor, and part after she had been put to bed.

Dr Pattison mentioned that in a case of recurrent hæmorrhage which he had seen, on requesting *Dr Simpson's* advice, he (*Dr S.*) introduced the uterine sound, and thereafter removed a small clot of blood, and the hæmorrhage ceased.

Professor Simpson said that in such cases as *Dr Cairns* had referred to, it frequently happened that a portion of the placenta got crushed up into a small size, and became hard and moulded into the shape of the uterus. The first case was very remarkable, there being no albuminuria.

II. CASES OF TURNING.

Dr Charles Bell read the following paper :—Some years ago, I was hurriedly summoned to attend Mrs M., whom I found in very melancholy circumstances. She was under the care of a homœopathic practitioner, who afterwards attained considerable notoriety in this town. This person was absent when I arrived, but he soon returned. In the meantime the patient informed me that her labour came on three days before, and everything seemed to go on well, although slowly, until eight o'clock the previous evening, when the waters

suddenly broke during an examination, and her pains immediately ceased. The medical attendant corroborated this statement, and said that although the os uteri was little dilated, he was enabled to ascertain that the hand of the fœtus presented; yet with the coolest indifference he left her, and did not return until the following day, when he did nothing. The patient and her friends becoming alarmed, sent for me about five P.M., when I made an examination, and found the left hand and funis in the vagina. She had had no pains since the waters broke: it is therefore probable that the gush of waters had brought down those organs. The os uteri was almost completely dilated, and the uterus was firmly contracted round the child. I expressed my surprise at the woman's being allowed to remain so long without assistance, and proposed immediately to interfere; but to my astonishment the doctor would not believe that there was any danger, and, as the patient was his, he would not allow of her being interfered with, as nature was quite sufficient to deliver her. I immediately proposed that Professor Simpson should be called in. To this he also objected; but, seeing the urgency of the case, I went for Professor Simpson, who at once agreed with me, and suggested that the patient should have 100 drops of morphia and two grains of opium, after which the uterus seemed slightly to relax. I had, however, considerable difficulty in introducing my hand, from the firm contraction of the uterus; but I at last succeeded in bringing down the left foot, and in turning the child, which was dead, and so decomposed that it was impossible by acting on the body to place the head in a right position for delivery. Professor Simpson therefore suggested and performed the operation of decapitation. This operation, however, did not facilitate the delivery of the head, which we endeavoured to do by means of the forceps, without success. At last Professor Simpson succeeded in fixing the blunt hook in the orbit, and after much traction, in which both he and I used considerable force, the head was delivered.

The woman made a good recovery, without having a single bad symptom, and she was able to be out of bed in three weeks.

The next case resembles the above in many particulars.

Mrs F., a Maternity patient, informed me that she had had severe labour-pains during the night, which continued to return regularly for some hours, when the waters broke, but that she had no return of pains for some time. She had had one child at the full time, and one miscarriage. On examination, per vaginam, I could not reach the os uteri in consequence of the high position of the womb. I could not therefore ascertain by this means the presentation of the child; and as there was no appearance of labour coming on, I did not think it justifiable to introduce the hand into the vagina; but I examined the abdomen carefully, and from the thinness of the parietes, I was enabled to ascertain that the fœtus seemed to lie in a sort of semicircle; the head occupied the left iliac fossa, the feet were in the right hypochondrium, and the nates towards the ensiform cartilage. I was in hopes that when labour fairly came on, that the head might slip into the pelvis. This favourable result did not, however, take place. I was sent for the following morning at five A.M., as labour seemed to have come on. I found the pains strong and frequent; and on making an examination, per vaginam, I found the left hand beyond the vulva. Immediately I resolved to turn the child, and with this view I gave her 45 drops of morphia, which were ejected, and produced no effect on the pains; she had 30 drops more with the same result on the pains; she then had 40 drops of Battley's sedative, which in a quarter of an hour were followed by 40 more, making about 200 drops of opium within two hours. The pains were so far subdued at last as to allow me to introduce my hand into the uterus, which was rendered very difficult and painful in consequence of the os firmly contracting round the arm, producing the most excruciating cramp. The diagnosis I had made the previous day, with regard to the position of the child, was fully corroborated by the intro-uterine examination. I laid hold of the right knee, and brought it down with great difficulty, arising from the strong contractions of the womb, which frequently produced severe cramp in

my hand. Even after the foot was in the vagina, there was great difficulty in effecting the evolution of the child, as it was firmly grasped by the womb, more especially by the cervix and os; and I question if it would have been effected had it not been for the able assistance of the late Dr Ziegler, who, I believe, the Society will agree with me in thinking was one of the most accomplished accoucheurs of his time. He attempted to introduce his hand into the uterus, finding that I could not effect the evolution, but he did not succeed; he was enabled, however, to push up the shoulder, which allowed turning to be effected, and I delivered the child, which was dead. The placenta was immediately expelled, and there was no hæmorrhage.

As this poor woman was in miserable lodgings, she was sent to the Infirmary soon after her delivery, after which I lost sight of her; but I believe she got well.

Remarks.—I have been induced to bring these cases before the Society, not only because they appear to me to be worthy of your notice as being successful examples of the result of one of the most fatal operations in midwifery, performed under very unfavourable circumstances; but in order that I may have your opinion in regard to several practical points which they bring under review. Turning is one of the most ancient operations in midwifery, having been suggested by Hippocrates and Celsus in the case of dead children, and first by Ætius and Paulus Agineta in the case of living children; but we are principally indebted to Ambrose Paré and his pupil Guillemeau, and other French authors, for introducing it into more general practice in modern times. Although statistics prove that it is frequently had recourse to, the results of the operation are far from favourable, as, according to the researches of Churchill, one in every fifteen of the mothers die, and one in three of the children. The question naturally arises, Whether does this great mortality proceed from the character of the operation, or from the unfortunate delay in its performance? I am disposed to think that the latter is generally the true cause of death, both to the mother and the child. Therefore no delay ought to be allowed in performing the operation after the necessity of its performance is ascertained. But however unfavourable the circumstances of the case may appear in cross presentations, and presentations of the upper extremities, the results of the above cases seem to justify the attempt to turn before proceeding to any more serious operation, or leaving the case to nature.

There can be but one opinion as to the early treatment of the first case, which is a specimen of the utmost ignorance and inhumanity. But there may be some question as to its subsequent management; I refer more particularly to the operation of decapitation. On reviewing the case, I feel persuaded that no advantage was gained by this act; and were a similar case to come under my care now, I should be induced to adopt a different course, under the impression that however weak and yielding the neck of the fœtus might be, it might still aid me in bringing the head within the reach of the forceps or the cephalotribe, and failing these it might lend some assistance in the operation of craniotomy by retaining the head within the pelvis.

In regard to the treatment of the second case, I cannot divest myself of the idea that I committed an error in not introducing my hand into the vagina on my first visit, and ascertaining the position of the child by this means, even although there were no pains and no indication of labour coming on immediately, and at once proceeded to dilate the os uteri, and turned before the violent contractions of the uterus came on, which forced down the hands of the fœtus.

III. A COMPLICATED CASE OF ARM PRESENTATION. BY DR ANDREW INGLIS.

On Wednesday, the 25th of October last, I saw a patient at the request of one of the pupils of the New Town Dispensary, as he thought the presentation abnormal. The woman, who was about her full time, had been crushed in a crowd two nights before, and had been in pain ever since. The liquor amnii had escaped some hours before I saw her. On calling, I found her complain-

ing of pain in the abdomen, but not in labour. The abdomen was hard and tender to the touch, and the uterus was closely moulded to the body of the foetus and extended higher up than usual. The fundus formed a round and prominent tumour, and below this prominence there was a strongly marked band of constriction, while another rounded tumour extended from the constriction down to the pubes. The vagina was rigid, and the os, which was closed, was so high that I only ascertained that the head was not the presenting part. This condition lasted a week without any alteration. At the end of this time labour came on, and after four or five hours of regular pains, the os was nearly dilated. At this stage I was sent for, and found the abdomen still firm and painful. The uterus remained contracted between the pains. The right arm presented, with the shoulder tightly jammed against the pubes, while the thorax seemed inclined to protrude into the pelvis. The cervix was so hard that the hand could only be pushed in a very little way. She was then put deeply under chloroform for two hours, and at the end of that time Dr Weir saw her with me. We both tried repeatedly to bring down the feet, but failed, the uterus being so unyielding. Then, as the head was still above the brim, we tried to pull down the breech, in imitation of what sometimes takes place spontaneously, but in this also we were unsuccessful. I then sent for Professor Simpson, and requested him to bring instruments to diminish the size of the child. The pains about this time began to increase and to push the foetus farther down, and, when Dr Simpson arrived, the body was projecting into the vagina, doubled backwards, with the face above the symphysis pubes, the right arm was protruding from the external parts, the thorax was in part visible, and the abdomen filled up the hollow of the sacrum. When he had examined the case, he advised that the neck should be divided if possible, and we made several attempts to do so, with Dr Ramsbotham's sharp hook, but without success, the neck being high up and nearly parallel to the handle of the instrument. Dr S. then made use of it as a blunt hook, and while I pulled down the body with it, he cut through the ribs and clavicle with a strong pair of scissors, and finally, this not having altered matters much, divided the spine. The moment the spine gave way, the body came out, and then the head with the arm attached to it. The placenta followed, and then there was a little bleeding, which stopped the moment the uterus was compressed. Before delivery the urine was bloody and the pulse high, but an hour after it, the pulse had fallen to 95. The catheter was required for two days. A fortnight after delivery she was nearly in her usual health, and now is quite well.

On examining the body of the child after it was extracted, we found that the line of division ran from the outer third of the right clavicle downwards to the fourth or fifth rib of the opposite side. In front, the sternum was divided low down, and behind, the spine was cut through at the fourth dorsal vertebra. The bones of the thorax were all divided subcutaneously, except the clavicle and a few of the ribs, which were beyond the external parts at first.

The first point I wish to draw your attention to, in connexion with this case, is the permanent contraction, or spasm of the uterus.¹ It had lasted for a week before labour began, and was the main cause of the difficulty. When the pains set in, they came at regular intervals and were in no way abnormal, but between them the spasm was as strongly marked as before their accession; and, even after the patient had been kept deeply under the influence of chloroform for two hours, the uterus was nearly as firmly moulded to the body of the foetus as before.

Such persistent action of the uterus is not very unusual. It seems never to aid expulsion in any degree, and when combined with malpresentation produces most serious consequences. In some cases we find the whole uterus apparently involved, while in others a broad band² of constriction alone is

¹ Since this paper has been written, I have heard that some French authors have called this abnormal action "Tetanus" of the uterus, and that Dr Wilson, of Glasgow, has also written an article on the same subject; but I have not, as yet, had an opportunity of seeing any notice of its occurrence, excepting what I have quoted.

² Hamilton's Midwifery, part i. p. 217 and 218.

observed. Smellie records a case¹ in which he found a series of such bands. I have met with several cases of the occurrence of the spasm long before labour, and in one I brought under your notice last year it had continued for three weeks. I have also observed it come on during labour; frequently where the head presented, and where, in fact, the contraction seemed the only thing abnormal, but oftener where such was not the case. Moreover, the spasm may come on during, or after, turning. In a case where the funis being prolapsed, I had to open the os rather rapidly in order to turn: the uterus contracted firmly and continuously immediately after the version, and the pains ceased for about four hours. Where premature escape of the waters precedes the occurrence of spasm of the uterus, it seems probable that the irritation which may follow the escape often brings on the spasm, but it certainly also takes place while the membranes are entire; thus sometimes coming to be a *cause* of premature rupture.

I am inclined to think that cases often happen where due weight is not given to this action of the uterus. In spring I reported a case where it came on during labour; and, though the limbs were in the vagina, and the head still above the brim of the pelvis, neither external nor internal efforts, nor both combined, could move the head up or the feet down. Dr M'Clintock relates a case similar in almost every respect, except that he makes no mention of the state of the uterus. He quotes, without comment, an observation made by Madame La Chapelle, as follows:²—"She says, that where the head has been the presenting part, it is very difficult to terminate the delivery by drawing down a single leg, and that this difficulty is produced by the head being pushed into the brim before the breech of the child; the buttocks acting in the same way, she says, as the knot on a cord with which we extract a cork from the interior of a bottle. When both feet, on the contrary, have been seized (this was done in my case just referred to) there is much less difficulty, she asserts, in pushing up the head and getting the child to make the turn or revolution." With regard to this theory of Madame La Chapelle, I have to state that I do not see how the breech could act the part of the knot on the cord without the operation of some other cause of detention of the head, such as contraction of the uterus. The advantage of bringing down *both* feet seems problematical. I have turned very frequently where the head presented, and have always succeeded where one leg was brought down, while, on the other hand, in the only case where both legs were brought down, it so happened that the head could not be made to recede.

The cause of this spasm or continuous contraction of the uterus is not always clear, but in almost all the cases I have seen, there was some unusual irritation either of the uterus itself or of the neighbouring parts. Thus, in the case I have referred to, where the spasm had lasted three weeks, organic disease of the kidney was present, and the waters had escaped soon after the abnormal contraction was observed; in another there was pelvic abscess; and in others a clot had formed between the uterus and placenta, there had been malpresentation, or the woman had suffered from direct mechanical injuries, such as blows, or it seemed due to the irritation caused by the operation of turning.

When this spasm has existed for some time before labour, I have tried to produce relaxation by opium or belladonna, but without apparent result. I am inclined to think, that if it is severe in such cases, the induction of premature labour may be the best treatment, if the membranes are still entire. With regard to the treatment of the spasm during labour, Smellie³ and Hamilton⁴ speak of manual dilatation as being often successful where mere bands of fibres are concerned. I have used it along with chloroform in these circumstances, and have now and then been successful. Where the whole uterus is contracted, no medicine seems to have much effect. If artificial delivery be required, and the head present, the possibility of introducing the hand where

¹ Smellie's Midwifery, vol. iii. coll. xxxvi., case vi., p. 13.

² Clinical Memoir on Turning, vol. iv., Obstet. Transac.

³ Smellie, op. cit., vol. iii., case vi., page 12.

⁴ Hamilton, op. cit., part i. p. 229.

the whole uterus is not involved ought not to tempt us to try turning, for if the child is moved at all, it is possible that an only partial revolution may be effected, and the child be thus placed in a much more difficult position than before.

In cases of malpresentation, however, we must of course try turning wherever there seems any chance of success; but this can hardly be looked for except where the contraction is confined to a portion of the uterus, for I believe that it is only where this is the case that sufficient relaxation can be brought about. Daventer, who obtained a great reputation in his time, proposed, where turning was difficult, that the patient should be placed on her knees, with the head as low as possible, so that the uterus might hang downwards. This plan, he said, would enable turning to be accomplished in all cases, however difficult, and if universally adopted would bring about the abolition of all instruments, both for extraction and embryotomy. From the fact, however, of his promising such unlimited success, his word alone is without value; but Smellie¹ has spoken strongly in favour of the practice, and illustrated it with cases where "Daventer's method" was successful after other means had failed. On the Continent also this position is often had recourse to, and in this country it has been recently recommended, and successfully adopted in cases of prolapse of the cord.

There now only remains to be considered how embryotomy is to be performed under such circumstances after turning has failed. These cases are generally divided into two sets:² one, where the neck is accessible, and where decapitation is advised; the other, where the neck is out of reach, and evisceration with turning is recommended. I have, however, been led, by my experience in the case forming the subject of this communication, to think the treatment of both these sets susceptible of improvement. The operation performed by Dr S. practically consisted in division of the spine at its most prominent part, with so much of the rest of the thickness of the fœtus as was necessary, and then the *direct* extraction of the two resulting portions separately, without any recourse to turning. The comparative ease with which all this was accomplished, and the slightness of the disturbance caused to the mother, contrasted very favourably with the evisceration (which may occupy many hours), and the subsequent turning as usually performed in such cases. Moreover, the line of division being so situated that one arm was left in connexion with the head, suggested to me that such division might be substituted for the simple decapitation at present performed where the neck is accessible, and thus save us the difficulty often experienced in subsequent extraction of the head.

With regard to the use of Dr R.'s sharp hook, I think it not out of place to mention, that if decapitation is to be done, the instrument can hardly be expected to do it in many cases. If the body fills up the pelvis, the hook cannot be pushed past it without doing injury to the lining of the pelvis. The reason is, that the shaft of it is quite straight. When Smellie first began to use the blunt hook and the crotchet, he soon found out that the straight handle was a great disadvantage, and in his third volume there are about ten or twelve cases, in reporting which he either regrets that the shaft of the instrument had not been curved, or where he states that he could never have succeeded with the straight crotchet.

Dr Simpson remarked, that the two oldest operations in such cases were mentioned by Celsus, namely, cutting through the neck, and evisceration. He mentioned a case which he had seen with Bailie Alexander, where he thought he could turn, in which the side was presenting. In the course of a few minutes the position of the child was changed, and the case terminated by spontaneous evolution. Observe, you cannot bend the spine laterally, but you can bend the back forwards. Dr Simpson spoke of the advantages of spondylotomy, where,

¹ Smellie, op. cit., vol. i. book iii., chap. ii., sect. ii. p. 208.

² Seanzon on Decapitation and Decapitating Instruments, Ranking's Half-yearly Abstract of Med. Sci., vol. xxxiv. page 249.

with the aid of a strong pair of scissors, with one blade probe-pointed, the back could be divided in cases where turning could not be effected. In Dr Inglis' case the condition of the uterus could not be called "spasm," but rather what the French call "tetanus," or continued spasm. Dr Simpson mentioned a case of a woman in labour of her seventh child, where the cervix uteri contracted round the child's neck: the forceps were tried in the ordinary way and failed; but by taking advantage of the relaxation of the uterus during the intervals between the pains, and making traction during those intervals only, he had managed to pull down the head. Dr Simpson promised to narrate the particulars of a case of arm presentation, with deformed pelvis, at next meeting, where he had performed the Cæsarean section. In arm presentations, where turning proves impossible, Cæsarean section is almost the only treatment, except spondylotomy, which is a mere clipping of the spine at the nearest presenting part, but it must be clipped quite through. Dr Lee has mentioned that evisceration takes two hours to perform.

Dr Keiller agreed with Professor Simpson as to the improvement of spondylotomy, as an operative procedure, when turning could not be effected. He (Dr K.) urged the propriety of pushing the head well up, so as to admit of turning more easily. Dr Keiller mentioned a case he had seen with Dr Finlay' of a woman who had stumbled a few days before her confinement, displaced the child, and he found it had resulted in a case of arm-presentation. The uterine stethoscope was in this case found useful to detect the fetal heart.

Dr Simpson said that there was no room to admit of turning in Dr Inglis' case.

Dr G. Weir spoke of the benefit of using long scissors to clip the spine, or a modified pair of bone-pliers might be sufficient.

Dr Simpson said that at Vienna a crotchet was used to break the neck by rotating it. He (Dr S.) said he generally cut the spine at the nearest point that could be reached.

Dr Charles Bell and Dr Keiller closed the conversation *pro tempore*, as Dr Simpson promised a paper on spondylotomy at next meeting, which would again open up the subject.

THE TRICHINA DISEASE. BY DR THUDICHUM.

RELIABLE accounts of the epidemic of trichiniasis at Hedersleben, in Prussian Saxony, show it to have been the most extensive and fatal of all outbreaks hitherto recorded. While the Hettstädt outbreak caused severe sickness to upwards of 150 persons, and the death of 28 out of this number, the epidemic at Hedersleben has already (up to 21st December 1865) produced upwards of 90 deaths; from these figures we may fairly conclude that the total number of persons attacked amounts to several hundreds. All this havoc has been caused by one trichinous pig! The butcher, having recognised the abnormal appearance of the meat of this pig, had carefully disguised it by mixing it with the meat of two healthy pigs, or added it in small pieces to larger joints of pork to make up weight. He made this confession shortly before his death, which was caused by trichiniasis contracted from his own meat. His wife also died of the disease.

The most lamentable part of the history of this outbreak is, however, the circumstance that the practitioner resident at Hedersleben failed to diagnose the disease in the early stages of the first cases, when there would have been time to seize and destroy much of the fatal pork then exhibiting in the butcher's shop. The sudden and simultaneous seizure of many persons with vomiting and purging, with griping, spasm, and collapse, effectually simulated a commencing epidemic of cholera, and opium was used freely to arrest the symptoms. When the course and fatality of the outbreak at last led to a correct appreciation of its nature, the time for prevention, mitigation, or palliation, had passed by.

Many physicians from all parts of Germany have gone to Hedersleben to study the disease. The public have been roused once more to a sense of the danger. Panic impels to unreasonable propositions, and reasonable measures are neglected. Ignorance and folly form a grotesque background.

At Berlin, a meeting of town councillors, butchers, doctors, and a sprinkling of the general public, was held shortly before Christmas. Professor Virchow addressed the meeting, and urged the necessity of instituting a microscopical examination of all pork. At the conclusion of his speech, he handed to the president a piece of smoked sausage, and a piece of meat from a pig which had been recognised as trichinous. Thereupon a veterinary practitioner, of the name of Urban, rose and combated all that science has acquired during the last five years as an unfounded illusion. "Trichinæ," he said, "are the most harmless animals in the world. It is only doctors without practice who make a noise about them, in order to create some occupation for themselves," etc. (Great interruption; the president is obliged to stop the veterinarian.) Drs Virchow and Mason demand an apology from M. Urban. Dr Mason challenges Urban to eat some of the sausage on the president's table. (Great applause.) Urban wishes to explain. The meeting calls upon him to eat. "He had not spoken of Berlin doctors ('Eat, eat!'); but of those at Hedersleben. ('Eat!') He would first see whether the sausage contained trichinæ." (Great laughter and continued shouts of "Eat! eat! eat!") Whereupon M. Urban suddenly seizes the sausage on the president's table, bites off a piece, eats it, and leaves the hall forthwith, amidst the applause and laughter of the assembly.

About five days later (on 23d December) the *Oelkszeitung* reported that the veterinarian, Urban, was ill. He was confined to his bed, and his arms and legs were paralyzed. A hope was expressed that the illness was not caused by trichinæ contained in the sausage of which he had been badgered to swallow a piece. Vain hope!

The Berlin butchers, finding their trade extinguished, held a meeting on 20th December, and resolved, with 200 votes against 9, to make arrangements for the microscopic examination of all pork. They have petitioned for the co-operation of the municipality, and desire to make the examination obligatory upon all.

The butchers in the exporting towns of North Germany also instituted microscopic examinations. Thus we learn that a butcher at Flensburg in Schleswig, who kills 15,000 pigs per annum, the meat of which is mostly exported to England, has adopted this measure of precaution. This is laudable, but no one should rely upon such examination exclusively. *All pork should be most carefully cooked before use. Trichinæ in man are prevented with certainty only by careful and thorough cooking.*

Some say, "We in England do not eat raw meat, and therefore the danger of trichiniasis does not affect us." This is an error. In at least one county the agricultural labourers do eat raw bacon. But the strongest proof of the possibility of trichiniasis breaking out amongst us at any time is the circumstance that the common tapeworm from pork, *Tænia solium*, is always infesting a number of persons throughout the kingdom. Now, if measles survive salting, smoking, and cooking (so-called), and, after ingestion, become tapeworms, *a fortiori* it is clear that trichinæ will survive these processes, because they are much better protected against their influence than measles. The trichina has been discovered in this country; cases of trichiniasis have unquestionably been observed; and instances of encapsuled trichinæ are constantly being discovered in our anatomical theatres. Only last year a subject with millions of trichinæ in its flesh was dissected in the Middlesex Hospital. Why, therefore, disguise or deny the danger? Let us prevent it. Let us be ready to meet it at all stages. Let us search for the source whence the pig receives the trichina, and endeavour to close it up.—*Lancet*.

CHOLERA IN PARIS.

THERE has not been for several days a single case of cholera in the civil or military hospitals, or in private houses, in Paris. During the last fifteen days of its decline, there was one death per day regularly registered, and several days ago this solitary case disappeared. The cholera first invaded Paris on the 15th of September, and completely ceased on the 15th of the present month of January. It continued with variable severity for four months. The last return of the officers of the Board of Health at the Prefecture of Police, and officially communicated to the Academy of Medicine, shows the general movement of the epidemic from its first appearance to its final cessation. Admissions to the civil hospitals, 2865; cases occurring in the hospitals, 707. Deaths: in the civil hospitals, 1844; in the military hospitals, 162; in private houses in the twenty arrondissements of Paris, 3837; in the rural communes since the last bulletin, 545; total deaths, 6388. During the 122 days, from the 15th of September to the 15th January, there were fifty-two deaths from cholera (in round numbers) daily in the Department of the Seine; *i. e.*, there died two persons per day out of every 100,000 inhabitants in private houses. The highest rate of mortality, 230, was on the 14th of October. After that date, the epidemic declined very slowly, and began to decrease rapidly during the first fortnight of December.—*British Medical Journal*.

A LYONS SURGEON ON ENGLISH SURGERY.

M. DESGRANGES, an eminent hospital surgeon of Lyons, lately delivered a discourse, at a meeting of a learned society of that city, with the following title—"On Public Charities in France and England." Among the topics alluded to, there is one in which the speaker has taken, it seems to us, an erroneous view of the motives of British operating surgeons. He says: "We Frenchmen value life as much for the poor as the rich; as surgeons, we have the same regard for a patient who will after recovery remain helpless, as for the artisan who, after being cured, can resume work. In England matters are not always considered in this light. In that country, tormented with incessant activity, and ever striving after utilitarian results, the value of a man is reckoned upon his productiveness; hence, when disease has rendered him useless, he is considered to have no right to refuse the boldest operative measure, meant to fit him for work, and restore his social value. Is it, therefore, surprising that this way of viewing matters, which is very illustrative of the national character, should favour the venturesome attempts which have spread far and wide the scientific renown of English surgery?"—*Lancet*.

OBITUARY.

DR DAVID JAMES SIMPSON.

ONE of the most painful duties of the journalist is to record the death of a young man of high promise, suddenly cut off at the commencement of his career. Deeply as we may lament the death of him who has already gained a reputation, there is something still more melancholy in seeing removed from us, one who, from his abilities and position, seemed certain to attain to distinction. Such dispensations are amongst the dark things of providence; yet we are called upon to believe that, though unintelligible to us, they are subservient to some great purpose.

While the medical profession and the public were still congratulating Sir James Simpson on the well-merited honour conferred upon him, they were

suddenly called on to sympathize with him on the occasion of the death, at twenty-three years of age, of his oldest son, a young physician of great talent, who had already shown proofs that he was qualified to take a high position in the medical profession.

David James Simpson was born in 1842. He passed through the usual curriculum at the Edinburgh Academy, and, when fifteen years of age, was sent to the Gymnasium at Elberfeldt, where he spent a year. On his return to Edinburgh he studied medicine at the University, and graduated as Doctor of Medicine in 1863. In the same year he passed as a Licentiate of the Colleges of Physicians and Surgeons of Edinburgh. After graduation he became one of the Resident Physicians in the Royal Infirmary, where he spent a year under Dr Sanders. On leaving the Infirmary he went to Germany, and passed a session at the great schools of Berlin and Vienna. Returning home in May 1865, he became one of the assistants of his father, in place of his cousin, Dr Alexander Simpson, who had entered upon practice in Glasgow. In November 1863, Dr David Simpson was elected Senior President of the Royal Medical Society of Edinburgh, and held that office for one year. The inaugural address which he delivered on assuming the chair of the Society contained a spirited sketch of the recent advances of medicine and surgery, and was published at the time. Dr Simpson took an active part in the debates of the Society, and was very popular, both as a member and a president. The Society, we understand, has sent, through its secretaries, a communication to Professor Simpson, expressive of its sympathy with him in his bereavement, and of the warm feelings it had entertained for the deceased. Every member of the Society is required to read a dissertation; the subject selected by Dr Simpson was "Remarks on Syphilization as a Cure for Constitutional Syphilis." It was read before the Society on the 26th February 1864, and was published in this Journal in December of the same year. In this paper, Dr Simpson described two cases which had been under the care of Professor Boeck of Christiania, and which he himself had had an opportunity of seeing; he expressed himself in favour of syphilization as a cure for constitutional disease, though opposed to it as a prophylactic, or in the treatment of the primary affection. This paper was looked upon as the first of many others which we might have expected from him. But it had been otherwise ordered. Early in January he was attacked with jaundice and albuminuria; at first there seemed no ground for serious apprehension; but latterly the symptoms assumed a graver character, becoming complicated with purpura, and he died in his father's house in Queen Street, on Sunday, the 14th January.

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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Clinical Inquiries into the Influence of the Nervous System on the Production and Prevention of Dropsies, and on the Means and Methods of Successful Treatment.* By THOMAS LAYCOCK, M.D., Professor of the Practice of Medicine and of Clinical Medicine, and Lecturer on Medical Psychology and Mental Diseases in the University of Edinburgh. PART I.

ALL the current theories of dropsies and dropsical effusions, however varied they may be as to details, can be finally referred to some simple doctrines of hydraulics and hydrostatics. Few of modern pathologists entertain the notion that the nervous system is involved, and that the morbid process may be a defect in nutrition. Professor Virchow, however, discusses it, and endeavours to show that *œdema fugax* and *hydrops spasticus, hystericus, and paralyticus* of authors are kinds of dropsical effusion which occur in the course of a dropsy due to cachectic watery blood-state,—a “*hydræmia*,”—and that they occur simply from congestion of the vessels of the part affected. When this takes place, the serum of the watery blood escapes of necessity more readily there than elsewhere where no special congestion occurs.¹ This and all other theories fail, however, to explain various circumstances observed in the origin and course of dropsies when carefully scrutinized. In especial, cases of cachectic dropsy, in which the causes are general, the limited exemption of certain localities from effusion, as well as of the effusion itself, is remarkable. Thus Andral observes, as to the order of development of anasarca in cardiac dropsy, that although the legs usually swell first, and then the scrotum and penis, in some they became anasarcaous at the same time; while, in others, the latter only became infiltrated when œdema is already manifested at many points: this—“*En vertu de ces inexplicables dispositions individuelles que nous retrouvons sans cesse dans l'étude de la pathologie.*”² So also, he noticed that in a certain proportion of cases of intermittent fever there was œdema of the lower limbs during con-

¹ Handbuch der Speciellen Pathologie und Therapie, vol. i. p. 120.

² Clinique Médicale, tom. iii. p. 537.

valescence (paraplegic anasarca), but why it came on he was at a loss to explain, for there was no disease of the heart or large vessels, or liver, or spleen. It usually disappeared in from twelve to fifteen days, with copious perspirations.¹ It is, however, when the seat and extension of anasarca are ascertained by measurement that the most striking exceptions to current theories are observed, and such as incontestably show that some hitherto unrecognised influences determine the locality and extent of the effusion. Now, I propose in this paper to show that almost all anomalies may be explained by referring them to the influence of the nervous system on the tissues affected, with two distinct and apparently antagonistic results, namely, the prevention as well as the causation of watery effusion of serum. When we consider how contradictory current theories are, how empirical, and too often how unsuccessful the treatment, and how common and distressing these dropsical maladies, such a fact must be of great practical importance; but of still greater, when the inquiry is found to throw light on all these common and fundamental morbid changes, known as congestion, inflammation, degeneration, retrocession, metastasis, and the like.

It is right, however, to explain, *in limine*, that I have already directed the attention of the profession to this important subject. In the Medical Times and Gazette for 28th December 1861, a case is reported with my remarks upon it (Case V. in this paper), in which an attack of hemiplegia was followed by the almost complete cessation of anasarca in the unpalsied limbs. Again, in the same Journal for 31st May 1862, there is a published clinical lecture by me on the Physiognomical Diagnosis of Dropsies, in which the nature and causes of metastatic anasarca and of nervous and local œdemas are fully considered. Thirdly, on 25th April 1865, I communicated a paper on the subject to the Royal Medico-Chirurgical Society of London, in which the inhibitory or preventive influence of the nervous system on dropsies was particularly set forth. And, fourthly, for some years past, I have called the attention of both my systematic and clinical class to the facts themselves, and to their practical bearings on the doctrines of so-called metastasis, and of congestion, inflammation, diathetic degenerations, and other fundamental processes. And I would further add, that anasarca has been selected for observation rather than albuminuria and effusions into shut sacs, for the sake of greater clearness and simplicity. For the same reason, the arrangement of the various kinds of dropsies which I have adopted is that of diseases of the nervous system in general. I therefore speak of centric and reflex dropsies, of general dropsies, and of paraplegic, hemiplegic, facial, and other local effusions, according as the disease extends over entire symmetrical regions, or is limited to a local system of nerves and nerve-influence.

¹ Clinique Médicale, tom. i. p. 482.—(Fièvres.)

I.—NERVOUS DROPSIES OF CENTRIC ORIGIN INVOLVING PRIMARILY THE TRUNK AND LIMBS.

1. *Hemiplegic Dropsy*.—Hemiplegic dropsy is that kind of dropsical effusion which is limited to one-half of the body, or to the limbs on one side, and if a viscus be affected consecutively, the effusion is either exclusively or predominantly unilateral. The following case is an example as reported by Dr Smart, formerly one of my clinical clerks.

CASE I.—*Cardiac Dropsy with Pericarditis in a Man aged 56; Hyperæsthesia of the Right Side of the Thorax; Anasarca of the Left Side of the Body; Effusion chiefly into the Left Pleura.*

An Irish field-labourer, married, æt. 56, was admitted under my care into the Royal Edinburgh Infirmary on 9th December 1861. About six weeks previously he was exposed to wet and cold while at work, and became ill, with a severe dry cough, shortness of breath, a difficulty in making water, and swelling of the legs. About a fortnight before admission he became much worse in his breathing, the difficulty in passing urine increased, and the swelling of the legs gradually extended upwards to the thighs and trunk.

On admission, the patient had great and painful dyspnœa when recumbent, and was ready to faint when placed in the upright position. He laid persistently on his left side, and complained of being "smothered" if placed upright or on his back. He passed urine in bed, but that which was collected was found to be slightly albuminous. The upper or right side of the thorax was painfully sensitive to touch, so that very little examination was possible; but it was observed that the œdema was limited to the left half of the face, neck, trunk, and scrotum. The tongue also was dry on the right half and moist on the left. In about five days, however, the patient had improved so much under treatment that it was possible to move him, and he was more carefully examined. The left eyelid alone was observed to be œdematous and the conjunctiva infiltrated. Œdema of the trunk was great, but limited to left side. It was also limited to left half of the scrotum and penis, and more developed in left leg and thigh. Measurements showed the circumference of the right thigh at lower third to be $13\frac{1}{2}$ inches, of left thigh at same point 15 inches; of right calf 11 inches, of left calf $11\frac{3}{4}$ inches. The motor power and cutaneous sensibility of the œdematous side seemed to be unaffected. Persistent hyperæsthesia of the non-œdematous side; mental condition marked by extreme depression and querulousness, with tendency to abstraction and somnolency. After a period of improvement, in which the urine amounted to 64 oz. daily, and free from albumen, a sudden diminution of the secretion occurred, and albumen was present. The hyperæsthesia increased, so that a slight touch gave pain; the breathing became more difficult and laborious; the anasarca became more general; a state of semi-coma came on, and death followed in a few hours. On a *post-mortem* examination, the left pleural cavity was found to contain 100 ounces of serum, the right pleural cavity 50 ounces; the left lung was greatly compressed and spleen-like; the bronchial membrane congested and red throughout both lungs, and the tubes filled with a muco-purulent fluid. The pericardium was greatly thickened and firmly adherent to the heart throughout by adhesions partly of old standing, but chiefly recent. The cardiac walls were of the normal thickness, the valves healthy. The kidneys contained a few cysts, but were otherwise healthy. Liver of natural size, slightly congested.

In this case, the hemiplegic effusion, in the first instance, was unilateral throughout. The left pleura was involved co-incidentally

with the left half of the head, neck, and thorax ; and but for the copious effusion into the pleura, the left lung would have probably been œdematous also. It was not clear whether the unilateral moisture of the tongue was accidental or not, probably the former.

NEURO-VASCULAR AREAS IN ANASARCA.—Hemiplegic œdema is usually referred to the position of the patient, and therefore caused by gravitation, whenever the fluid is present only on that side on which the patient lies ; or else it has been attributed to the obstruction of a large vein in the region of the neck or shoulder. Obstructive œdema being thus caused, the effused fluid gravitates and fills the cellular tissue of the corresponding side. Either theory, however, wholly fails to explain the distribution of the dropsical fluid in this case. I do not know, indeed, whether it has ever been shown that there is such an intimate connexion between the areolar tissue of the two sides of the thorax, and of the thorax and abdomen, that effused fluid *can* pass freely from the areolar tissue of one region to that of another. I asked my friend Mr Turner, demonstrator of anatomy in the University, to make the experiment of injecting water into the subcutaneous tissue of the thorax and abdomen of a corpse, and watching how it was diffused. The solitary experiment he was able to make in my presence seemed to favour the conclusion that the cellular tissue is continuous, and dropsical fluids may pass from one portion to another under the influence of physical forces. But, when compared with the experiments of nature, it leads to another conclusion also. Cases are occasionally observed in which there is obstruction of the upper venous trunks of the body, and dropsical effusion results. Dr Watson mentions in his lectures an example of this kind, in which the neck and face were hideously bloated, and the arms so hugely swollen with water, that they could not be brought to the sides, but the lower limbs appeared preposterously small, because they remained of their natural size. The cause was found to be pressure of a large aneurism on the superior *vena cava*, close to the auricle. Now, if there be continuity of the cellular tissue throughout the trunk and legs, the fluid in this case should have gravitated from the face and neck downwards, infiltrating through the areolar tissue of the thorax and abdomen to that of the legs, but it did not. On the other hand, when anasarca begins to show first in the legs, the cellular tissue should fill like a bottle from below upwards. And, indeed, to ordinary observation, this appears to be the way in which general anasarca advances, but in certain cases it is not so. I carefully marked the progress of œdema in a highly cachectic patient, who (it was found after death) had embolism of the left femoral and common iliac veins, and of the inferior *vena cava*. First, the left leg became œdematous, then the right, and afterwards the loins, and so upwards along the back. But when the œdema reached to the level of the seventh rib,

it ceased to advance upwards, and the thorax and arms remained free from dropsical swelling up to the death of the patient. Yet, according to the bottle-filling theory, the anasarca should have gone higher and higher. And this is not a solitary case. Hence it follows that although the cellular tissue may actually be so continuous as to allow the diffusion of water equally in all directions, when effused into it, as in Mr Turner's experiment, there is nevertheless a prior condition required for its effusion; and that seems to be either a condition of the living tissue itself or of its capillaries.

In the instances of thoracic œdema from venous obstruction, the anatomical distribution of the venous and arterial capillaries certainly plays an important part. I had a patient lately under my care in the Edinburgh Infirmary, whose face was turgid and the thorax œdematous from the pressure of an aneurism on the upper venous trunks. This revealed the fact that the cutaneous thoracic circulation has a limit not known to anatomists, being sharply bounded below by a line corresponding very nearly with that of the sixth or seventh ribs; for in this patient the whole surface of the thorax to that point was livid with distended venous capillaries, whereas, beyond, the skin was free from capillary congestion. And I have observed (now in more than one hundred instances) that a line of vascularity stretches along this level in certain persons who are of an emotional temperament, with a tendency to diseases of the vascular system. I have termed it præcordial vascularity, from its site, for in the majority the line is broken like an imperfect rainbow, and the chief fragment is seen on the left side, below the left mamma. In a certain proportion of cases, however, it runs in an unbroken curve across the epigastrium into the right infra-mammary region, in a breadth of vascularity of from six to ten lines. This capillary congestion is not due to mechanical causes,—*Firstly*, Because those who have it to any extent are invariably found to wear their clothing lightly. *Secondly*, Because the dilated vessels change colour according as the patient's mind is active or quiescent. *Thirdly*, Because the same region is not unfrequently the *first* seat of œdema in a case of general dropsy. This I term a *præcordial œdema*.

These are examples of a fact too little recognised,—namely, that there are distinct areas of vascular action and capillary distribution which have their corresponding nerve-centres (NEURO-VASCULAR AREAS), and which have limited inter-connexion with each other. It is probably for this reason that the seats of herpes zoster may be referred to the distribution of cutaneo-spinal nerves. Whether the functions of the venous radicles and of the absorbents are under this influence remains to be established. *A priori*, the question may be answered affirmatively rather than negatively; and the few experiments made as to the veins confirm this conclusion. Be this as it may, it is certain that the gravitating, congestive, and obstructive theories are wholly inapplicable to

numerous cases of anasarca. In that under consideration, only the symmetrical half of the penis and scrotum was œdematous, and yet the whole of the external genitals was equally dependent. So also one eyelid was œdematous, while the other was not. Still more restricted areas of effusion may be sometimes seen in cases of general dropsy. Thus I have shown to my clinical class two patients in the same ward, both with general renal dropsy: in the one, the prepuce was infiltrated, while the scrotum was free; in the other, the scrotum was infiltrated, while the prepuce was free. Now, in these cases there could be no question as to the fact that the force of gravity was exercised equally upon the tissue which was infiltrated and the tissue which escaped infiltration. Why, then, the difference in the extent of effusion? Inflammation removes a dropsy of the scrotum, but there was no inflammation; exposure of a part to cold will excite effusion, but all the tissues were kept equally warm. Nor is the order of anasarcaous effusion, as manifested in different and distant parts, less referable to current theories. Too little attention has been paid to this clinical fact; yet it is of considerable practical importance. It is very well shown in the following history of a case under my care:—

CASE II.—*Cardiac Dropsy, with Bright's Disease and Pericarditis; Erratic Order of Development of Anasarca; inhibited in Left Hand.* (Reported by Dr Smart.)

George S., aged 66, married, an engine-man, admitted April 14, 1863, into Paton's Ward, Edinburgh Royal Infirmary, under care of Dr Laycock. The patient, an old soldier, who had served seven years in the West Indies, states that he enjoyed good health until a fortnight ago, when he had to give up work in consequence of his *thighs swelling*. *Scrotal and preputial œdema followed*; then of the *legs and feet*; afterwards of the *abdomen and face*; and, lastly, of the *right hand*; the thighs and left hand not swelling. On admission the pulse was 80, hard, jerking, and irregular; pulsations generally visible; precordial vascularity existed; transverse cardiac dulness measured $4\frac{1}{2}$ inches; apex beat behind sixth rib in a line vertical with nipple; a double friction murmur is heard over whole of precordia, but most loudly at mid-sternum. There is also a blowing murmur, which is loudest at apex. Second sound accentuated. There exists marked dulness, with increased vocal resonance, and coarse crepitation at the bases of both lungs behind. Considerable cough, dyspnoea, and expectoration, which is bronchitic. The urine had a density of 1·013, with abundant albumen and granular tube-casts. Both lower extremities, scrotum, prepuce, and right hand (not left, which was always the dependent hand when he lay in bed) are extremely œdematous. Thorax, face, and eyelids are slightly so. The parts become affected with œdema in the order mentioned in the previous history. Other systems are normal.

Dr Laycock ordered R Tr. digitalis, ℥ss.; liq. ant. tart. ℥ij.; aquæ, ad ℥xi.—M. Sumat ℥i. ter die. A blister to be applied between the shoulders.

On the 15th April, the report states that the dyspnoea is less urgent, and œdema, albumen, and tube-casts diminished.

On the following day, to increase diuresis, which was scanty, the tincture of lyttæ was substituted for antimonial wine. The patient continued to improve until the end of April, when cardiac action became very irregular and tumultuous, accompanied with severe precordial pain and urgent dyspnoea.

Next day, profuse hæmoptysis supervened; general œdema rapidly increased; the dyspnoea became extreme; and he died on the 4th of May.

Autopsy.—The whole body was œdematous. Pericardium distended with serum, and a band of organized lymph, half an inch in length and breadth, tied the base of left ventricle to the inner surface of the pericardium. Mitral valves greatly thickened. Those of the aorta slightly incompetent. The vessel itself dilated and atheromatous. There was considerable chronic and some acute condensation at bases of both lungs. Mucous membrane of bronchial tubes congested.

In this case there was a remarkable absence of anasarca in that arm which the patient had under him, for *decubitus* was on the left side,—a fact wholly beyond the gravitating theory; but the order of development is not less remarkable, inasmuch as the thighs and genitals became œdematous before the feet and legs. It was facts of this kind which attracted Andral's attention, and which, in common with all of us, he could only consider as inexplicable caprices, because referable to no known theory of anasarca.

The case of hemiplegic anasarca just detailed was simply cardiac; but cases of renal dropsy present the same peculiarity as is shown by the following history:—

CASE III.—*Renal Dropsy in a Woman aged 40; Anasarca chiefly on the Right Side of the Face, Limbs, and Thorax; Hyperæsthesia of the Right Side of Face and Right Arm; General Convulsions, except in Left Arm and Left Side of Face; Death; Brain œdematous; Clots in Optic Thalami.*

Johanna P., aged 40, wife of a ploughman, was admitted to Ward XI. in the Royal Infirmary, Edinburgh, on February 15, 1865, to be under the care of Dr Laycock.

History.—Patient enjoyed good health until four months ago, when, after exposure to cold and wet, she suffered severe pain in the loins, followed by swelling, first of the face, then of the legs, and, lastly, the body generally. The urine was scanty and high coloured; thirst great. She had not menstruated for the last six months. On admission she complained of severe frontal headache, and pain in the right lumbar region, which did not subsequently remit much. Skin dry. The urine albuminous, with tube-casts. Anasarca was general, but most markedly on the right side. As *decubitus* was on that side, this difference was at first considered to be due to gravitation; but for seventeen days before death she lay supine; and, on observing her position when on her side, it was seen that the left arm was so dependent over the chest that if there had been any extensive œdema the hand and forearm must have shown the infiltration of gravitation. The thighs and abdomen were much swollen; but the left calf only measured 11 inches round, while the right calf, at a corresponding point, was $14\frac{1}{2}$,—difference $3\frac{1}{2}$ inches. The right wrist measured 7 inches, the left was not swollen at all, and was 5 inches round,—difference 2 inches. The left forearm was swollen, and measured $8\frac{1}{2}$ inches at a point 2 inches below the elbow, but at the same point the right measured $9\frac{3}{4}$ inches,—difference $1\frac{1}{4}$ inch. The right arm and right side of the thorax were hyperæsthetic, not so the left. The left eye and left side of face showed hardly a trace of œdema, the right eyelids were swelled like bladders. All this under such circumstances of *decubitus* that the force of gravity could have little, if any, influence in the diffusion of the watery fluid from one limb or side to the other. A week before death, the right side of the face (the dropsical side) became highly sensitive to touch, and frontal headache was complained of: two days before death, the patient had repeated convulsive attacks, in one of which she died comatose. During these attacks the left arm

and-left side of the face (the non-dropsical side) were not convulsed. The head and face were twisted towards the left side. The anasarca distention in the thighs and abdomen, and of the right side, was enormous for a few days before death.

Autopsy, thirty hours after death.—Effusion into the peritoneum, pleura, and pericardium; no important lesions in other organs except the brain and the kidneys, which latter were in the second stage of the inflammatory form of Bright's disease. The tubes of the cortical substance were full of exudation; epithelium in a state of fatty degeneration; the right kidney large, the left small, and of a peculiar form. The brain was cedematous throughout; both of the thalami contained clots,—that in the right thalamus the larger. A papilla of brain-substance (grey matter) projected like a wart from the surface of the wall of one of the ventricles.

HOW FAR DO CARDIAC AND RENAL DISEASE AND ANÆMIA CAUSE DROPSY?—In the preceding cases the well-known coincidence of general anasarca and disease of the heart and kidneys is illustrated. This is, indeed, of such common occurrence that the diseases are usually considered to stand in the relation of cause and effect. In cardiac dropsy the obstructive theory is applied, and it is argued that the anasarca is due to the venous congestion which results from dilatation and insufficient action of the right side of the heart, consecutive to fatty or other degenerations of the organ, to emphysema, and other chronic and congestive diseases of the lungs, and to obstructive valvular diseases of the left heart. It is certainly impossible to watch the progress of a case of this kind without observing that the dropsy advances *pari passu* with the pulmonary and cardiac congestion, and the conclusion naturally follows that the more pressing condition—the congestion—is the efficient cause of the dropsical effusion. But, then, it is also observed that similar cases as to the state of the heart and lungs occur, in which there is the most distressing pulmonary congestion, leading to extreme dilatation of the right heart, hæmoptysis, and death by pulmonary apoplexy, with little or no anasarca or albuminuria. What morbid condition, then, is wanting in these non-dropsical cases of cardiac disease which is so strikingly operative in the dropsical? It may be advanced that there must be a conjoined hydræmia,—a watery condition of the blood,—but then this condition does not appear to be very manifest in various cases of general dropsy connected with structural disease of the heart. On the other hand, in a certain proportion of cases of cardiac dropsy the dilatation of the heart and the pulmonary congestion are consecutive to the anasarca. In these it is argued there is anæmia or hydræmia in the first instance, which weakens the contractility of the vessels, so as to allow the too watery serum to transude; then this condition acts as an impediment to the arterial circulation, requiring a greater power of cardiac impulse to overcome it. But inasmuch as the heart is also enfeebled by the poor blood as well as the vessels, it is unequal to the effort, and dilates. Thus the anasarca is not the result but the first cause of the dilatation. Chlorotic cedema and renal dropsies

are usually adduced as illustrations of this kind of dropsy. There can be no doubt whatever that poor blood and congestion of tissues and viscera, caused by mechanical obstruction of the circulation at the centre, however arising, are general causes of dropsy and of serous effusions when certain other conditions of the blood and bloodvessels coincide; but why in the cases of both cardiac and renal dropsy under consideration was the dropsical effusion wholly or partially hemiplegic? As to both sides the general causes are the same, whether we consider the state of the blood or of the central force-pump; and the anasarca is duly manifested in the one-half of the body; but why is it absent or less on the other half? Now, clinical observation enables us to separate the cardiac and pulmonary conditions from the hæmic, and to show that hemiplegic œdema will occur when there is neither renal disease nor obstruction to the circulation in either the heart or lungs. In proof I give the following history:—

CASE IV.—*Hemiplegic Hyperæsthesia and Œdema; Glycosuria; no Disease of the Lungs, Heart, or Kidneys; probable Spinal Disease. Syphilis (?)*.

Catherine L., a dressmaker, aged 32, was admitted into the Royal Infirmary, under my care on 20th January 1860. One year previously, after exposure to cold, she had rigors, severe frontal headache, and pains which extended to the back of the head, the neck, and between the shoulders. They continued with little intermission for about four months, during which period the patient was confined to bed. She gradually recovered and resumed her ordinary employment. Three months previously to admission she again caught cold, and with precisely the same result, only now there was swelling of the right side of the face and of the right foot, in addition to the neuralgic pains. A fortnight before admission the patient got her feet wet, and all the former symptoms were now aggravated. The right side of the face was much more swollen, and was painful to touch; the left side was quite unaffected. The right knee, leg, and foot were also œdematous and painfully sensitive. On more careful examination it was observed that her complexion was sallow; that on rising from the horizontal position she supports her head with her hands to prevent the pain caused by the movement of her head and neck; that the right pupil is dilated, and the eyeball more prominent than the left; the right eyelids (upper and lower) are œdematous, and the œdema extends along the right side of the face downwards over the right parotid, and for a short distance down the right side of the neck, immediately below the angle of the lower jaw. The skin of the right side of the face is more sensitive to pressure than to simple touch, and there is tenderness on percussion over the seventh cervical and first and second dorsal vertebræ, which are also prominent. The right calf and ankle are slightly œdematous, and there is tenderness over the inner condyle of the femur, but no swelling.

The appetite was good; respiration natural; pulse feeble, 80; a slight systolic murmur at base of heart. The skin showed brown discolorations (syphilitic?). The urine of sp. gr. 1012; no albumen; sugar in considerable quantity, and also indican. Formerly menstruated every fortnight, and very abundantly, but not for two months past.

CENTRIC (NERVOUS) INHIBITION OF ANASARCA.—In the case just detailed there were certain indications of anæmia, or more probably of a syphilitic chlorosis, to which the dropsy might

be referred. But these are general causes of dropsy; yet the anasarca was not general, being restricted to a certain region of the body. Striking symptoms pointed more distinctly to a centric lesion inducing glycosuria, and specially involving one-half of the cilio-spinal region of the axis, or the nerves on one side; so that if it be admitted that the causes of the anasarca were general, then it must be inferred that they were restricted in their operation by the condition of the nerve-centres. Doubtless the simpler explanation is, that the portion of the nervous system in connexion with the anasarcaous tissues was enfeebled, while all the remainder was healthy; and such a theory certainly affords a sufficient explanation of numerous cases. But there are inequalities in dropsical effusions not to be explained by it, and as to which it is necessary to conclude that a certain morbid condition of a portion of the nerve-centres inhibits anasarca. As this fact in the pathology of dropsies has not hitherto even been so much as surmised, I shall endeavour to show how such inhibition occurs.

CASE V.—Cardiac Dropsy (Pericarditis and Valvular Lesions) in a Woman aged 30; Hemiplegia of Left Side from Embolism of the Middle Cerebral Artery of Right Side; thereupon Anasarca disappears greatly from the Right Side, increases greatly on the Left.

A female servant, aged 30, a large and well-developed, well-nourished woman, reported to be of somewhat intemperate habits, was admitted under my care into Clinical Ward XI. Royal Edinburgh Infirmary, on 23d October 1861. Five years ago, when in good health, she had shivering, followed by cough, difficulty of breathing, and palpitation. Three weeks ago or more, about her menstrual period, the cough and palpitation became more severe, her appetite much impaired, nights sleepless. Two days before admission her legs began to swell; she menstruated on the day of admission, and had severe pain in the back. She complained of pain in the region of the heart, palpitation, and urgent dyspnoea. Face congested, lips livid, superficial veins of neck turgid and pulsating. Urine scanty, not albuminous. Feet and legs anasarcaous, and slight œdema of the hands, which are of cadaverous coldness. In fourteen days the œdema had reached the trunk, the whole of the lumbar region being very anasarcaous, and in eighteen days a trace of albumen was detected in the urine. In twenty-four days the legs were so tense and painful that an incision was made over the left ankle, and numerous lancet punctures over the right, with the view of allowing the effused fluid to drain away. Shortly after, at the same date, an attack of hemiplegia occurred. The facial muscles of the right side were strongly contracted, the mouth twisted to the right side, and the tongue, when protruded, inclining to the left. Articulation indistinct; motion of left arm wholly lost; of left leg much impaired. Sensibility of left side unimpaired; pain complained of over the right parietal eminence. The œdema in the left arm now increased very rapidly; and the left leg became more distended, but in a less degree. The motion of the left side of the thorax became more and more defective; the breathing laboured; swallowing difficult. In two days the left side had lost its sensibility, and the anasarca had constantly increased. The right thigh and leg, on the contrary, became smaller and smaller, until the skin hung empty and shrivelled. She died in four days from the hemiplegia, and twenty-eight days from the last menstruation.

Post-mortem examination by Dr Haldane.—Well-marked œdema on the left side of the body. Abundant subcutaneous fat. Pericardium adherent to apex of heart by a strong fibrous band. The heart, especially the right side,

gorged with blood. Near the apex of the left ventricle was a globular clot of the size of a large filbert, of a brownish red colour, softened in the centre. The appendix of the left auricle, and the right auricle and ventricle contained similar clots. Aortic valves incompetent, thickened, two of them shortened. Adherent to the margins of each were fleshy growths. Mitral orifice contracted, of diameter $\cdot 72$ inch; the edges of the valves thickened, and a little glueing together of the chordæ tendinæ. The left ventricle was dilated. The right ventricle dilated and hypertrophied; the diameter of the tricuspid orifice 1·6 inch, its circumference 5 inches. The lungs were adherent to the pleuræ, most extensively on the left side, congested and œdematous. The bronchial mucous membrane greatly congested. Liver moderately firm and presenting the nutmeg appearance; spleen firm and congested; cœcum congested.

Head.—Membranes of the brain quite healthy, only a little fluid in the ventricles. The middle cerebral artery lying in the right fissure of Sylvius was found to be obstructed by a clot of the same kind as that observed in the left ventricle, just at the point where it divided into three branches. In one of these there was a loose, black, secondary clot fully half an inch long.

I submit that this case establishes the important clinical fact that a morbid state of the nervous system has an influence in both removing and aggravating anasarca in a case of cardiac dropsy. For while the wholly palsied left hand became rapidly more œdematous after the palsy came on, and the left leg continued swollen although fluid was oozing from the incisions near the ankle, the œdema diminished in extent in the unpalsied arm, and disappeared altogether in the unpalsied leg. Formerly, before the discovery of cerebral embolism, such a case would have been considered an example of "serous apoplexy," and the disappearance of the anasarca looked upon as the cause. In short, it would have been classed with cases of "serous metastasis." A more correct pathology enables us to say that the embolism of the right middle cerebral artery was the cause of the palsy of the left or opposite side, and it seems equally probable that for the same reason the anasarca disappeared on the right or same side. And this view explains the true nature of Cases I., III., and IV. For if we were to suppose hypothetically, what is of no uncommon occurrence, that in this case the hemiplegia had preceded the anasarca, that would have been hemiplegic too; because, while the lesion facilitated dropsical effusion on the one side, it inhibited it on the other. But a clinical history will best establish this fact in the pathology of dropsies.

CASE VI.—*Symptoms of Syphilitic Cerebellar Lesion; Right Testicle wasted; Stricture and Purulent Urine; Hemiplegia of Left Side of Face and Tongue, and of Left Arm. Œdema comes on in Left Arm and Leg.*

John B., aged 29, a plate-layer, unmarried, was admitted under my care into the Royal Infirmary, on 13th March last. Has had syphilis. Two years ago, he had stricture of the urethra, so that he could pass urine in only a small stream. At the same time he began to suffer from frontal headaches and impairment of sight, more especially of the right eye. Objects assumed curious aspects in shape and colour; for example, they appeared crooked, spiral, bent, or interrupted in their continuity; and in colour yellow or blue. Three weeks ago, shortly after supper, he was suddenly seized with giddiness, so that he had to hold on to prevent himself falling, and immediately afterwards he

vomited. Both giddiness and vomiting lasted three days. When questioned, it is found that he can only read the largest type, and has the visual illusions described; hearing and taste are also much impaired; he has much occipital headache, and he staggers as he walks, his legs feeling weak and shaky. A slight systolic murmur is heard at apex, and a loud ringing diastolic murmur over whole chest, but loudest at third left costal cartilage near sternum. Pulsations visible generally. Bowels habitually constive; has a stricture so small as not to admit the smallest bougie; urine purulent. Right testicle wasted; penis small. On 23d March, he complained of shooting pains in his limbs, and, on 29th, he found, when he awoke in the morning, that he could not use his left arm. The left side of the face was palsied, and he protruded his tongue towards the left; eyelids unaffected. He could not lift his arm, but could grasp somewhat firmly; left leg unaffected. He articulated in a slow drawling manner; complained of pain in both the front and back of the head. On 1st April, it was observed that he slept a good deal; œdema was also seen to have affected both the arm and the leg on the left (palsied) side; the right arm and leg unaffected. In a day or two, the œdema diminished a little, and subsequently considerably. The measurements on 1st April (three days after seizure), were as follows:—Left leg (œdematous), one inch above malleolus, $8\frac{3}{4}$ inches; right leg (not œdematous), at same point, $8\frac{1}{2}$ inches; left leg above the patella, 12 inches; right leg, at same point, 11 inches. By the 19th May the œdema had disappeared, except at the left ankle.

As this case was under exact observation from the first, there can be no doubt that the hemiplegic dropsy followed directly upon the hemiplegic lesion as an effect thereof. It is remarkable how few exact observations are on record of this kind. Peter Frank, however, mentions an instance in his elaborate account of dropsies.¹ A corpulent man, paralyzed on the left side, was affected with hiccup when drinking mineral waters. This was relieved by opium; but now the urinary secretion was suppressed, and the whole of the left or paralyzed side became dropsical, the effusion being strictly limited to the middle line. We have here, then, at least two classes of dropsies in which centric nerve-lesion exhibits anasarca. One in which the lesion removes the anasarca existing, by preventing further effusion and allowing the absorbents to take up the fluid; another in which, when the causes of anasarca arise, the lesion facilitates the effusion on one side and inhibits it on the other.

II.—PARAPLEGIC DROPSY.

Paraplegic dropsy, like paraplegia proper, may have its origin either in the spinal cord or in the encephalon. It is difficult to determine, however, in many cases where the morbid change first originates. In the reflex paraplegia connected with structural diseases of the kidneys and bladder there is the exact analogue of the paraplegic œdema which accompanies certain kinds—but not all—of Bright's disease. I say not all, because there is in these cases a much more complex problem to solve than is usually believed. Experimental researches have proved, on the one hand, that injuries to the crura cerebri and the base of the brain will induce albumi-

¹ "*De Curandis Hominis Morbis*," liber vi. § 749.

nuria as well as glycosuria. The former is explained by the theory of renal congestion, but the latter is obviously due to a change on the tissue processes. Now, on the other hand, morbid or excited states of the genital organs have a direct influence on the brain itself, as is proved in various convulsive, paralytic, hysterical, and cerebro-mental disorders, and probably, therefore, on its nutrition. In Case III. it is probable that there was a direct causal relation between the diseased and painful right kidney and the predominant disease on the right side of the brain; also in Case IV., between the *right* wasted testicle and the palsy of the left side, due to defective activity on the *right* side of brain as well as the hemiplegic anasarca. The paraplegic paresis in the same case was probably also in relation with the stricture, the wasted penis, and the hallucinations of vision. The subjoined case illustrates the centric origin of both paraplegic anasarca and renal congestion, and the true nature and origin of the coma which so often coincides with albuminaria and Bright's disease.

CASE VII.—*Facial and Paraplegic Anasarca; long-standing Bright's Disease; Hæmaturia, Coma, and, thereupon, Disappearance of the Paraplegic Anasarca. Death. Syphilitic Disease of Cranium.*

James M., aged 37, admitted under my care into the Royal Edinburgh Infirmary, November 6, 1861. He was an old *habitué* of the charity, and had long-standing Bright's disease with syphilis. On admission he had distressing anasarca of the whole of the lower extremities, and œdema of the conjunctivæ, eyelids, and face, but the rest of the body was free. His forehead had baggy swellings beneath the skin. Urine highly albuminous, and, finally, hæmaturia came on, with violent pains in the legs from the toes to the groin. So soon as these were felt, the anasarca of the legs gradually lessened, and finally disappeared; and the patient as gradually sunk into a comatose state, in which he died.

On examination of the body, nearly half the frontal bone had a honey-comb appearance from extensive destruction. There was a complete perforation of the skull, two inches long and one broad, filled up with thickened periosteum and dura mater. The cranium, generally, was thickened, except the occipital bone, which was very thin; the brain natural; pericardium and heart natural; chronic tubercle of the lungs, chiefly of the right; small waxy deposit in the liver; blood in the intestinal canal, but especially abundant in the cœcum; the arteries of the villi waxy; the kidneys of natural size; the Malpighian corpuscles and the arteries had undergone amyloid degeneration; the tubuli atrophied.

Anasarca of the legs is almost universally attributed to gravitation, chiefly (I apprehend), because when there *is* effused fluid in the cellular tissue of the lower extremities, the upright position increases the œdema, and the recumbent position diminishes it. But in the case just related, the œdema was great in the recumbent position, and disappeared considerably, with violent pains in the legs, evidently of a neuralgic character, and with, at least, paresis, if not palsy. The disappearance is obviously to be classed with the hemiplegic cessation of the anasarca noticed in Case V. It would be easy to explain the case as caused by uræmia, or on the theory

of a serous metastasis from the legs to the brain. But there was no unusual amount of serum in either the brain-tissue or ventricles; while, with a manifest encephalic lesion, the uræmic hypothesis is wholly gratuitous. The entire absence of any mental disorder excludes the hemispheres from consideration. The remarkable thinness of the occipital bone is not without significance as to the probable seat of the cause being at the base of the brain; but we are greatly in want of accurate observations as to the relations which local thinness and unusual density of the cranium bear to the encephalon and *its* diseases.

III.—REFLEX DROPSIES DUE TO (SO-CALLED) SYMPATHIES.

Genito-Urinary Anasarca.—In the case just detailed, the face and eyelids were differentially dropsical. This selection of place of effusion was probably in relation (as I now propose to show) to a reflex action of the genito-urinary system on the face and eyelids, and illustrates the class of reflex or sympathetic dropsies.

The connexion between disease of the kidneys and dropsy has long been recognised. It is now an established doctrine that the dropsical effusion is mainly, if not wholly, due to the state of the blood, which, in the chronic cases, is greatly impoverished by the continual flux of albumen with the urine, and in the acute is supposed to be injured by the presence of a noxious agent, such as retained urea, or a febrile poison like that of scarlatina. And when albuminuria, without tube-casts, has been the chief symptom, the nervous phenomena which have been observed to be associated with it, such as puerperal convulsions, coma, amaurosis, and the like, have been rather attributed to the albuminuria than the renal affection and the nervous phenomena to a centric nervous lesion, as the common cause of both.

Three views as to this kind of anasarca may be adopted,—1. The current doctrine that the kidneys are primarily affected; or, 2. That the nervous system is affected first, and the kidneys become diseased and anasarca developed consecutively; or, 3. That the lumbo-sacral portion of the spinal cord is disordered by reflex action of both the urinary and genital organs, and that then the renal disease, the anasarca, and the nervous phenomena follow consecutively. I will endeavour to show that the latter and larger generalization is the most in accordance with all the facts.

It will at once be admitted that the genito-urinary system has an influence upon the nutrient and functional activity of the spinal cord. As to the testes, ovaria, and uterus, this law is abundantly established; as to the kidneys it is less certain, but the fact is compatible with existing views as to reflex paraplegia. In renal affections, then, thus arising, the morbid results will be manifested first in the kidneys, then in distant organs and tissues in functional or physiological relation with the urinary and reproductive systems.

They may be shown in disorders of either the circulation or the nutrition of the tissues, but if they consist in the effusion of serum from the capillaries, then as to the kidneys it will be an albuminuria, and as to distant tissues, œdema or anasarca. I shall not discuss nervous albuminuria now, being too large a question. If, then, we would determine what local reflex dropsies arise from the action of the genito-urinary system on the cord, we must first determine what tissues are in physiological relation with that system, or sympathize, in older phrase, with it. Clearly, we must first name the external genital organs themselves; consequently, we should expect dropsy of the labia, scrotum, and penis, to be an early and very common symptom in reflex genito-urinary anasarca. The skin and tissues of the loins and thighs come next in relation, and then the lower extremities. Now, this is very much the order in which "renal" dropsy appears. In that consecutive to scarlatina, and occurring in adults as acute dropsy, the genital organs and the lumbar region are commonly the first to swell, and often with singular rapidity. Nevertheless, that a reflex action from other abdominal organs as well as the kidneys will excite dropsy there is probable. A patient already referred to, who had anasarca after obstruction of the right iliac, suffered acute pain in the bowels during the evening; in the morning his prepuce and scrotum were œdematous.

Dropsy of the Eyelids.—It is in the eyelids, however, that reflex genito-urinary dropsy is most strikingly shown, because there is no *a priori* reason of contiguity why it should be. The eyelids constitute distinct neuro-vascular areas, and their vessels are affected from two sources of innervation. In emotional persons the *upper* eyelid becomes red from copious development of the capillaries, and when such persons are affected with cardiac dropsy, the upper lid is apt to become œdematous. It is the lower eyelid which is in peculiarly close relation with the genito-urinary system. In women, during menstruation, there is venous congestion there, and in chronic uterine diseases it becomes more or less infiltrated. In men this local œdema is sometimes very striking, giving the lower eyelid the appearance of a bladder of water; according to my experience this is invariably a mark of genito-urinary disorder or disease, although it seems to accompany hæmorrhoids also; but in these cases it is probable that the prostate is affected. When there is both an emotional temper and this reflex genital action, then both lids are alike affected by œdema and other changes. Thus, in neuralgia and epilepsy in women, with uterine irritation, the entire orbicularis of the eyelids will be marked out by pigment deposit, especially if the patient be of a dark complexion; so that it constitutes an almost perfect circle of discoloration.

It is in women, during the menstrual period, that this close relation between the eyelids and the genito-urinary system is shown in dropsy. The state of the blood at that time evidently predis-

poses to dropsical effusions. Thus, in Case V., at each menstrual period the anasarca was greatly aggravated. Occasionally, however, the eyelids became suddenly affected with œdema at this period, as in the following case:—

CASE VIII.—*Bronchitis, Bright's Disease, and Anasarca, in a Woman aged 24. Sudden Infiltration of the Eyelids at the Menstrual Period.*

Ann F., aged 24, was admitted to be under my care in Ward XI. of the Royal Edinburgh Infirmary on 5th December 1863. She had anasarca of the lower extremities, but most extensively of the right leg, and of the lumbar region and thorax. The face and eyelids free. Bronchitis, with œdema of the lungs. Urine highly albuminous and numerous tube-casts. On 13th January, she began to menstruate, but the flow ceased on the 14th after exposure to cold, and on the 15th the upper and lower lids of the right eye became suddenly œdematous; those of the left eye followed on the 16th. This orbicular œdema lasted two or three days, and then wholly disappeared. At the next menstrual period the face and neck became œdematous as well as the eyelids, causing a mechanical pressure on the windpipe: the general anasarca greatly increased, and she died on the 4th March.

This monthly development or exacerbation of œdema of the eyelids is not peculiar to women, as I have seen it in male patients (young men) with cardiac and renal dropsy, and in one case the anasarca generally was aggravated at monthly periods concurrently with great diminution in the urine.

Thoracic Dropsies of Genito-Urinary Origin.—In women the neck and thorax are also under the physiological influence of the utero-ovarian system. This is shown by the change in the mammæ and the thyroid body during menstruation and pregnancy. So also, in cases of renal anasarca in women, the neck and thorax sometimes become specially the seat of dropsical effusion. Occasionally, this is so much increased during the menstrual period that there is danger of suffocation, as in Case VIII. At the same time the renal symptoms are aggravated also. But it is not in renal dropsy this is exclusively shown, for females with cardiac dropsy manifest the same order of events. Thus, in Case V., the anasarca and pulmonary œdema were developed at one menstrual period, and death took place exactly a month after, or at the next. Other so-called sympathies might be mentioned, but these will serve for illustration of the fact, that the genito-urinary system has an influence in the production of thoracic dropsies.

III.—THORACIC DROPSIES OF CENTRIC ORIGIN.

I have already indicated that in Bright's disease the eyelids, but more especially the upper lid, may be dropsical from centric causes in those persons who are of an emotional temperament, and have considerable vascularity, as a consequence, of the upper lid. The connexion of the heart's action with all our emotions, indicates that a source of nervous dropsies may be found in the emotional centres, and that consequently this fact has to be considered, in con-

sidering dropsies associated with diseases of the heart and lungs, and which may be either of reflex origin, or due to a common cause, having its seat in the cervico-dorsal spine. In determining that seat it is necessary to separate in the inquiry the cardiac relations from the pulmonary, seeing that the two organs have not only different nerve-centres, but very different relations to dropsy. Now, the connexion of certain painful affections of the heart, grouped under the term *angina pectoris*, with neuralgia of the arms, and most commonly of the left only, is well known. I have carefully traced the cause of the pains in well-marked cases, and noted that the nerves involved are the external and internal cutaneous nerves (nerve of Wrisberg), and the intercosto-humeral nerves. These nerves are branches of the four lower cervical and three upper dorsal nerves, and are therefore derived from that portion of the spinal cord termed by Budge and Waller the "cilio-spinal," by Claude Bernard the "oculo-spinal" region, and which is so named because from hence are derived motor fibrils which influence the movements of the eyelids, iris, and eyeballs. If, then, this spinal region have an influence upon dropsical effusions into the cellular tissue of the arms, and thorax, and eyelids, it will be manifested not only by causing, but by preventing them. Now, such prevention or inhibition will, I believe, be found to be of no uncommon occurrence, provided cases be properly observed. Case III. is a striking illustration of this form of differential dropsy, and also that of the engine-man, Case II., although in a less degree.

It is to be observed, also, that when both arms and both sides of the thorax are anasaruous, they are often affected unequally. A woman, aged 54, has been under my daily observation in the Infirmary during the winter trimestre. She has Bright's disease, with frequent hæmaturia and great pulmonary distress. Her legs and thighs are anasaruous; she lies on her left side and the left arm; and the left arm, left side of the thorax, and left side of the face, including the eyelid, are much distended with fluid; but the right arm, left side of the face, and left eyelid are free, except where the anasarca is greatly aggravated. On these occasions the dorsum of the right hand and the under surface of the right wrist become, in some degree, cedematous, evidently from gravitation of fluid from the upper arm. That this difference was not due to decubitus was clearly established by noting the positions in which the exempted arm was placed. The following cases is illustrative.

CASE IX.—*Cardiac Dropsy in a Man aged 40. Decubitus on the Left Side. Inhibition of Anasarca in Left Arm.*

John N., aged 40, was admitted into Ward I., Edinburgh Royal Infirmary, on 24th October 1862. After exposure to cold, hardship, and habitual drunkenness, he had palpitation, orthopnoea, and other cardiac symptoms. On admission a loud whining murmur was heard at base with the second sound, at a point between the second and third ribs, and a double murmur at apex. After a long illness general anasarca was established, with traces of albumen

in the urine. Decubitus was commonly on the left side. The scrotum and prepuce and legs were equally anasarcaous, but the arms were not, for on measurement, on 13th December (the urine amounting to 64 ounces per diem), the right wrist measured $8\frac{1}{2}$ inches round, the left only 7; the right palm was 6 inches, the left only 5; and this disproportion continued all the way up the arms. Yet the patient had constantly his left arm the most dependent. After death, on 11th January 1863, it was found that he had had adherent pericardium and mitral and aortic disease. The whining murmur was caused by a tongue-like prolongation into the aortic orifice, and the aorta was very atheromatous.

The complication of cilio-spinal neurosis ("Grave's disease") with anasarca is perhaps less rare than it appears to be, from defective observation. A case has, however, been reported by Dr Wm. Moore, in which there was exophthalmic bronchocele, with cedema and dropsical effusion.¹ In a certain class of cases the anasarca shows itself in the first instance, and exclusively, in the region of præcordial vascularity (*see ante*, p. 779), constituting præcordial cedema. This was seen in two instances (of females) in the Infirmary, under treatment during the past trimestre. In both, the first intimation of any disorder of the health was given by the feeling of tightness of the clothing in that region, and when there was as yet no swelling elsewhere. Both cases were examples of so-called "acute Bright's disease;" but although the urine was loaded with albumen, no tubecasts were observed, and in both the pulmonary symptoms were the most pressing, evidently from pulmonary cedema.

IV.—CENTRIC PULMONARY ŒDEMA AND PLEURAL EFFUSIONS.

How far visceral cedema is necessarily associated as to a common cause, or class of causes, is a very important practical question, for it is this kind of dropsy which usually terminates life. In all the fatal cases of differential cedema I have related, it is noteworthy that it coincided with disease of the thoracic viscera, but more especially of the heart and pericardium. Dr Watson observes, as to pleurisy, that when the effusion is great, that side of the thorax in which it has taken place is more or less dilated, and "that the integuments on the same side are frequently cedematous."²

How far in these cases there is obstructive pressure on veins and gravitating cedema must be determined by observation. In Case I. the left pleura was the first and chief seat of effusion, coincidently with anasarca of the left arm and left side of the thorax, and I have also observed that cedema of the lungs follows the same law. It is probable that the so-called effusive pleurisies, in which there is also a violent intercostal neuralgia, are really examples of nervous dropsies, for in these an cedema of the corresponding side has been observed. And a difficulty as to the nature and meaning of pain in pleurisy can be cleared up by this view. It is well known how often the seat and extent of the inflammation has no relation to the seat of

¹ Dublin Med. Press, 15th April 1863.

² Lectures on Practice of Physic, 4th Edition, vol. ii. p. 123.

pain; so much so, that cases are occasionally met with in which the pain is on one side and the inflammatory changes on the other, just as in Case I. the hyperæsthesia was at the opposite side to the pleural effusion. Assuming the cause of pain to be centric, the side to which the pain will be referred will depend upon the seat of the centric lesion and the sensory fibrils involved. If it be on the same side as the pleural affection, and the decussating sensory fibrils be involved, then the resulting pain and hyperæsthesia will be referred to the opposite side.

In cardiac affections of a rheumatic character, with rheumatic pain in the left arm, and in angina pectoris, it seems certain that the corresponding half of the spinal cord is affected, and that when both arms become the seat of neuralgiæ, as sometimes occurs in breast-pang, it is because the spinal affection has extended to the other half. But even in these cases the law of decussation of the sensory nerve-fibrils may determine the seat of pain; in which case the spinal affection will be on the opposite side.

V.—CENTRIC DIAGONAL ANASARCAS OF THE LIMBS.

As illustrating what particular portion of the nervous system is involved and how, I would call attention to a class of œdemas which may be termed the *diagonal*. In some cases the effusion is limited to the thigh and hip of one extremity, and the leg and foot of the other; in others to the legs below the knee, and the arms below the elbow; in others to the thighs, the legs being free; in others the whole upper extremity is anasarcaous on one side, and the whole lower extremity on the other. The subjoined case is illustrative of this form.

CASE X.—*Dropsy in a Man aged 30. Signs of Cardiac Disease; Capillary Bronchitis and Fibroid Degeneration of Lungs; Probable Œdema of Right Lung. No Albuminuria. Diagonal Anasarca.*

Robert F., aged 30, a railway labourer, came under my care on 10th March 1863. He was in a very weak state, and had great dyspnoea, incessant cough, and copious frothy muco-purulent sputum of a fetid odour. The face was very livid, the eyelids and thorax œdematous. He had moist sounds very generally over both lungs, but predominantly over the right. Decubitus on the right side, on which side the thorax was most œdematous, probably from gravitation. Tubular breathing and blowing murmur at base were noted, but the state of the patient did not allow of careful physical examination. No albuminuria. The œdema was differentiated in a remarkable manner, for while the right side of the thorax was œdematous, with decubitus on that side, the right arm was free; and while there was œdema of the left arm, which was uppermost, the left side of the thorax was free. Further, while the right leg, foot, and thigh were very anasarcaous, the left extremity was free, except the foot, which was very œdematous. The eyelids were both œdematous. He died nine days after admission, with little change in the differential distribution of the fluid.

The restriction of the œdema to the legs, the thighs not being affected, is usually referred to gravitation. Without denying (what

must be obvious to all) that when the fluid is effused it must necessarily gravitate to the most dependent point, it is more than doubtful whether the effusion itself (which is a wholly different process—is *the* process in fact) is due simply to gravitation. Dominic D., aged 47, was affected with acute anasarca, tube-casts, and albuminuria. He had in the first instance œdema of the eyelids and face, then the scrotum was involved, and, lastly, the legs below the knee became exceedingly anasarcaous; yet the thighs were not affected, the effusion being bounded by the knees. So also purpura will be restricted to the forearms and the legs. Alexander M., aged 47, with Bright's disease, had œdema of both legs, not extending to the thighs or genitals, slight thoracic œdema, and œdema of the eyelids, forearms, and hands, but not of the upper arms. He was affected with purpura, but the spots were restricted to the legs below the knee, and to the right eyelid, affected just before death.

None of the current theories of dropsy help in any degree to explain facts like these, but certain phenomena referable to the nervous system are available. I would particularly refer the reader to Dr Roberts' instructive monograph on wasting palsy, for cases in which the muscular atrophy of the limbs followed almost exactly the same order as the dropsical effusions and inhibitions in these cases. For example, the legs are unaffected when the thighs were wasted, and so with the forearm and upper arm. In a table of 105 cases, the restriction of the atrophy to groups of muscles, and therefore to special centres of innervation, is very striking.¹ An additional fact of the same kind is contained in a communication recently addressed by Professor Rermak to the Medical Society of Berlin, in which he describes a case of "*Spasmus alternans transversus*," so named because this diagonal or crossed manifestation of phenomena was observed. For example, the muscles of the left upper arm and of the right hip were spasmodically affected at the same time. Some of those who have observed cases of wasting palsy (and Dr Roberts is one of them), finding it difficult to explain the nervous origin of the disease by current doctrines, have come to the conclusion that it is not due to disease of the spinal cord, nor of the motor nerves, either at their exits from the cord (roots) or at their peripheral terminations in the muscles. The difficulties disappear, however, if the affection be referred to a trophical nervous system, as distinct from the sensory, volitional, and visceral, and having, like those systems, restricted centres and areas of action. I have elsewhere discussed the question of restricted areas of innervation, as manifested in limbs and in groups of muscles, and to this work I beg to refer.² I may add, however, that some experiments made by M. Schiff strongly support the spinal origin of wasting

¹ On Wasting Palsy, p. 74.

² Mind and Brain: or, the Correlations of Consciousness and Organization, vol. ii. part vi. chap. v.—Organology of the Spinal Cord, and especially § 972, p. 419, *et seq.*

palsy, for he found, on making hemisections of the spinal cord, that there was a diagonal production of heat in the limbs. For example, the same section induced increased heat in the thigh of one lower extremity and the leg of the other.¹

Reserving, however, the consideration of these and other points in the history of dropsies, I would submit that the foregoing cases abundantly prove, 1. That the nervous system as a whole, or else some special division of it, has a direct influence both on the production and prevention of anasarca. 2. That anasarca is produced when innervation is defective. 3. That anasarca is prevented being manifested locally when the general causes are in operation, by more vigorous because more healthy innervation of the exempted tissues. 4. That centric disease or disorder may have the double effect of facilitating the effusion in one lateral portion of the body, and preventing it in the other lateral portion. 5. That production or prevention alike follow upon changes in innervation, which are induced in the same way, and according to the same laws, as other neuroses; and, finally, 6. That it is not the sensory, motor, or vaso-motor systems which are specially involved.

The influence of the portion of the nervous system involved in dropsies upon the means and methods of successful treatment will have a separate consideration.

ARTICLE II.—*Leprosy, Ancient and Modern; with Notes taken during recent Travel in the East.* By TILBURY FOX, M.D. Lond.

(Read before the Medical Society of London.)

IN thinking over the writings of some who have dealt largely with the literature of the ancients the feeling thrusts itself very decidedly upon our notice, that the rough details of bygone time are invested occasionally (it is to be admitted somewhat unknowingly) with such importance and significance as to constitute them the very standard of truth against which there is no appeal. This is especially the case with the writings of Celsus and Hippocrates. The subject of leprosy, amongst others, which of late has occupied the attention of many *literati*, is illustrative of the point in view. Our position in regard to it is one of doubt and misunderstanding, because up to the present time we have been unable to fathom the significance of the data by which the ancients were guided, and, according to the ideas of pathology now in favour, dovetail our own with the facts of former date. Under these circumstances we have adopted a convenient humility, and chosen to fancy that we are wrong and the ancients right; moreover, that by an examination of the laws of disease, as taught in the earliest days, we shall be able to mould the existent phenomena of our own day so as to fit the explanations

¹ Comptes Rendus, tom. 55, p. 426, sqq.

thence derived. Little difference is made for variation in expression in language or mode of record between this and the oldest time; the same argument carried farther would attempt to show that little *progress* is visible.

I am ready to admit that the bold outline and crude material of descriptive medicine may have remained unaltered by the lapse of time, and that by a careful study of concomitants we shall be enabled to harmonize the phenomena of different epochs, and to explain modifications; but cannot subscribe to any opinion which tends to teach that any retrocession has taken place. But while differentiation and detail have most abundantly multiplied in the progress of knowledge, the sustainable point has been overstepped, and men have been required to withdraw certain conclusions which were at one or another time firmly held, for, in the development of the special from the general, an enormous mass of new species has been elaborated which on subsequent observation have been found to be *mere varieties*, a phase of revocation which, culminating in the Darwin theory, is the characteristic of the age.

Still, on the whole, the preponderance of trustworthy result is immeasurably in favour of the student of to-day. Progress is traceable in everything except in one particular instance, to which my remarks will now be directed—the biblical account of leprosy. This cannot be judged in manner like to other descriptions. The objects and circumstances connected with it were special; Moses did not wish to teach medicine, but gave certain laws for the guidance and preservation of the Israelitish community—laws which the *priest* should observe, and which, whilst they were perfect *quoad* the end in view, contained only just the very necessary amount of pathology, and were not technically explained. There was sufficient difference in the nature of the forms of disease included under the term leprosy implied in the total difference of treatment. Some were clean and some unclean: Moses was under no necessity of defining pathological details. In the spirit of these remarks, I would be understood as vindicating the position of Moses on the ground of modern research.

Till within the last few years the solution of points of doubt in the matter of leprosy has seemed so very distant and unlikely as to have appeared a fitting subject to test the powers of a sphinx. Recently the accumulation of observations of small moment, *per se*, has been found to possess such potential interpretation and applicability as to have cleared the way very considerably for the establishment of satisfactory data. I have recently seen a good deal of leprosy during a lengthened tour in Egypt and Syria, and desire to call attention to particulars of its nomenclature and meaning (ancient and modern), without, however, aspiring to any such classical discussion as that which appeared from the pen of Dr Belcher, in a late issue of the Dublin Quarterly Journal, but to the hope of being able to reconcile some of the

differences of opinion which have been held in regard to leprosy, using that term, to signify the elephantiasis græcorum, altogether different from the *E. arabum*, the Barbadoes leg, or, as it is termed now-a-days, *bucnemia tropica*. The term *lepra* has, by its peculiar and varied renderings at the hands of writers, been a source of no inconsiderable obstruction to the foundation of clear conceptions of diseased conditions of the surface. It has been used in four chief senses,—

As signifying (1.) a special affection, distinct from elephantiasis, and known to moderns as *lepra vulgaris* (Willan).

2. The same condition, but in the sense of its being an expression of true elephantiasis.

3. The *lepra* of Holy Writ, a disease supposed to be now non-existent, peculiar to the Hebrews, special in its nature, and different altogether from elephantiasis, and also *lepra vulgaris*. A view originally propounded by Lorry and Ousseel, and recently Dr Belcher.

4. True elephantiasis itself, unconnected with modern *lepra vulgaris*.

In the Levitical code reference is made to many eruptions; but as concerning the subject of leprosy (elephantiasis), we observe that Moses referred to three superficial forms, independently of the fully-developed disease, (*a*) the boak or dull white; (*b*) the berat cecha or dusky berat; and (*c*) the berat lebena or bright white berat. Celsus arranges these under the term *vittiligo*; the first, or boak, corresponds to his *alphos*, the second to his *melas*, and the third to his *leuce*. Celsus, moreover, separated the developed disease elephantiasis from these superficial forms; hence Celsus and Moses apparently differ, but of this more in a moment. Now the Greeks made more confusion: with them the term *lepra* arose, and it is thought was applied to the *alphos* (boak), the highest degree of lichen (psora and *alphos*),—psora coming also from the Hebrew *psorat*. In the eleventh century, Avicenna inculcated and wrote the doctrines of the Greeks, and then arose another error; when the Arabian writings were translated into Latin the term *lepra* was given very unlicensed application, as explained by Bateman, to the elephantiasis of the Greeks, and the term *morphœa* was used to the scaly eruption called by the Greeks *lepra*; hence the general use of the term *lepra* and leprosy.

Now, it appears to me that an acceptable explanation of all pre-existing contradictions may be readily given, and particularly in consequence of the important observations which have been published by Dr H. Vandyke Carter of Bombay. That gentleman has shown clearly that there is a period of eruption in the majority of cases of leprosy in India: he noticed it himself in 62 of 186 cases. The eruption assumes two chief forms, a white and a dark variety, corresponding to the *leuce* and *melas*, and known as *morphœa alba*, and *morphœa nigra*. There is resemblance often to *lepra vulgaris* (*alphos*); and in India the confusion of these eruptions is common.

This morphœa is the *baras* of the Arabs. There can now be no question as to the relation of morphœa and elephantiasis. It is the earliest sign of elephantiasis, and it is easy to understand the different positions of Moses and Celsus. The boak of Moses, which did not render a man unclean, is our *lepra vulgaris*, or the *alphos* of Celsus; the berat *cecha* corresponds to the *melas* or morphœa *nigra*; and the berat *lebena* to the *leuce* or morphœa *alba*. Moses did not use his own judgment,—he gave as he was commanded; but how curiously his teaching and Dr Carter's observations agree as regards pathology, and how each is mutually proof of the truth of the other. Celsus was not a Moses, and did not see the relation of the components of his vitiligo, that the *melas* and *leuce* were early stages of elephantiasis, and the *alphos* distinct again. Dr Carter's observations, I repeat, are opportune, and most conclusive. Josephus and others have only followed their predecessors in misplacing *alphos*; but the time had not come for its solution and separation. Dr Belcher, in his essay, refers to some remarks at p. 360 of Mr Wilson's work, where it is said that the term *mal rojo* used in Spain, calls attention to the dark red or reddish brown line of the skin, and *mal noir* to the dusky hue in some cases; and says, it may well be asked how can *mal rojo* or *mal noir* be identical with the bright white berat of Moses, with the scaly *lepra* of the LXX., or with the disease of Gehazi, who went out of Elisha's presence as white as snow. The foregoing remarks explain that *mal rojo* and *mal noir* are but forms of morphœa. Mr Wilson, I think, does not imply that they had any relation with the disease of Gehazi, or the *lepra* of the Greeks (*alphos*). To put it in another way: the pathology of our own day teaches us (1.) *that Moses' two forms of berat, the leuce and melas of Celsus, and the forms known now-a-days as morphœa alba and nigra are the same, that they are early signs of elephantiasis*; (2.) *that the boak of Moses, the alphos of Celsus, and our lepra vulgaris are one and the same disease.*

Moses did not refer to the fully developed disease,—the tubercular and anæsthetic and ulcerative alterations,—but to the earlier stages; and he shows himself a great pathologist here; he actually forestalled the observers of recent time. This is forgotten in all discussion, and is the chief reason why authorities declare that the Israelitish leprosy and the elephantiasis *græcorum* are not the same. If they compare together the *same stages* an absolute identity is visible. The results of modern research, to my own mind, give a peculiarly prophetic aspect to the Levitical code, and there cannot be a doubt that modern investigation has fully proved its truth.

And, in all this there is little ground for supposing that any change has taken place in the relative situation of the diseases themselves. The same forms existed formerly much in the same state as at the present day, and there is little, if any, reason to think that the Hebrews were affected by one special form of *lepra* of which no trace exists now-a-days.

Lorry contended for the past existence of such a form of disease, and Dr Belcher, at p. 297 of the Dublin Quarterly Journal, says, that there is abundant proof that the leprosy of the Hebrews and that of the Middle Ages were as dissimilar as wasting and hypertrophy can be,—quite true; and at p. 298, that the Hebrew leprosy was the vitiligo of Celsus. This point has been referred to. Dr Belcher, in the article referred to, does not seem to allow that *lepra vulgaris* (Willan), the *alphos* of Celsus' vitiligo, and Moses' boak are the same; in fact, this disallowance is the groundwork of Lorry's view, though I gather from the recent edition of Neligan's work that Dr Belcher has modified his opinion.

Whatever else Josephus states, he certainly most plainly states thus much, that there was no disease such as is contended for peculiar to the Jews. The supposition is unnecessary, and most confusing. But I have at hand a fact, which is altogether convincing. During the last year (1865), in my travels through Egypt, Palestine, and Syria, seeing and hearing as much as possible about leprosy, curiously enough, I found in the vicinity of the Lebanon range, that a form of disease is common, of old date, and recognised as distinct from elephantiasis: it is called *baras el Israiliy*. I declare, from the description, character, and seat of the disease, that it is nothing more nor less than *lepra vulgaris* or *alphos*. Now, here is, without a doubt, Lorry's Hebrew eruption handed down to us by the Arabs and others, under the most peculiar designation, and yet it is found to be not a special, but the well-known boak of Moses. Ay, and yet more than this, there is an eruption like it of a dark scaly aspect, which is distinguished from it, and which is regarded as an early state of elephantiasis. It is the *baras* of the Arabs. These are very important facts, to my mind, because they are so completely and peculiarly confirmatory of all that has been said.

Though we may clear up all difficulty with regard to the ancient relations of *lepra* and leprosy, yet perhaps a greater one remains in the determination of the use of the term *lepra* now-a-days. It is a quarrel about words, and admirably does Max Müller observe, "yet under a different form, language retains her silent charm; and if it no longer creates gods and heroes, it creates many a name that receives a similar worship. He who would examine the influence which words, mere words, have exercised on the minds of men, might write a history of the world that would teach us more than any which we yet possess. Words without definite meaning are at the bottom of nearly all our philosophical and religious controversies, and even the so-called exact sciences have frequently been led astray by the same siren voice;" and afterwards adds that he does not refer to a mere show of language, which may deceive, but rather "to words that everybody uses, and which seem to be so clear that it looks like impertinence to challenge them." How very suggestive and teaching. The exact circumstances under which the

use of the term *lepra* arose are not easy to determine. The Greek translators are supposed to have applied it to the *alphos*. The Greeks must have used the term *lepra* in a generic sense, for they called the boak, *lepra alphos*; and the berat leben, *lepra leuce*. At any rate, the Arabians used the term thus widely, applying it to elephantiasis, and of course probably only followed in the footsteps of the Greeks, both peoples recognising in fact the old notion of the relation of *alphos*, *melas*, and *leuce*. Even in India, at the present day, the confusion of *lepra vulgaris* (*alphos*) and the eruptive stages of elephantiasis *græcorum* is very common, and Dr Carter pointedly refers to this. In fact, these states, always from the time of Moses, have been classed together in consequence of a certain similarity in outward aspect.

However this may be, clearly leprosy can be applied to none other than elephantiasis, and leprosy absorbs *lepra*, and herein Mr Wilson is perfectly justified in his views. Let the discussion go on as it likes about the mere use of names, thus much is clear, that Mr Wilson's suggestion of the use of the word *alphos* to indicate our *lepra vulgaris* is not only the most classical but the most judicious under the circumstances. If *lepra* were claimed by no other disease, by all means retain it in its present application; but this is not the case. Leprosy is clearly elephantiasis, and *lepra* belongs to it. *Alphos* is no invention coined to supply a deficiency.

This confusion of *alphos* and true leprosy, and its expressions (*morphœa alba* and *nigra*), is apparent, as before cursorily noticed, throughout all the Sacred Writings, in the descriptions; but it is hardly a confusion, because the details of treatment are defined; it is therefore rather an alliance; and if we carefully study the concomitants, we shall observe that every fact connected with the two classes of cases only tends to prove that the diseases under notice of that and our own time are the same. In Exodus, chap. iv. verse 6, we read, "And the Lord said furthermore, Put now thine hand into thy bosom. And he put his hand into his bosom; and when he took it out, behold, his hand was leprous as snow." Here is the general or wide use of the term leprous due to the translators. We find again, in Holy Writ, that some did and some did not dwell in places apart from the people, as in the case of Naaman and Gehazi, and the four lepers, on the other hand, who reported the flight of Benhadad's besieging army; the cases of Ahaziah, 2 Kings, chap. xv. verse 5, who was smitten a leper, and "dwelt in a several house;" and 2 Chron. chap. xxvi. verse 21-19, in reference to Uzziah, who was smitten leprous in his forehead, and abode in a several house. Again, in the time of Christ, some stood afar off, and were commanded to go show themselves to the priest, whilst others were touched and cleansed. Further, we observe how the exact rules laid down by Moses continued to be observed with a degree of exactness that is really remarkable, through the mid-life of the Hebrews, at the time of Christ, and even till this very day.

(The Levitical code bears in the marginal reading of the Bible the date B. C. 1490, Ahaziah's time B. C. 800). These observances being enforced by universal consent against a disease which, as elephantiasis, is and has been well defined, and could not by any possibility be mistaken for anything else, so that the strict peculiar observance is a guarantee, under the circumstances, that the original disease has still an existence; and, moreover, when we recollect the rigidity of the continued observance, it is easy to see in this an additional argument to prove that the so-called leprosy of Naaman and Gehazi, and that of those who stood afar off, and lived outside the city, were of different natures. There is the case I have mentioned of the *baras el Israili* and the *baras* of the Arabs existing side by side, affording to-day a complete illustration of the state of things centuries ago. *It is then wholly unnecessary to suppose the existence of a disease peculiar to the Jews; their white scaly eruption, which did not render a man unclean, is our alphas.*

It may be as well, perhaps, to add, for the information of those who are not well acquainted with the subject, that the disease of Naaman was the boak of Moses, the *alphos* or, as we in general term it, *lepra vulgaris*. Let, then, *alphos* take the place of the latter in future. I pray not to be thought as forgetting the labours of early writers, Aretæus, Galen, Hippocrates, Paulus Ægineta, Avicenna, Schilling, Ousseel, Bateman, Mason Good, since I have confined myself especially to comments on Dr Carter's researches, and the clearly expressed views of Mr Erasmus Wilson; all, however, with the silent current of clinical observation, have settled, it appears to me, the dubious points in regard to elephantiasis, save the position and use of the word *lepra*,—an intrusion of the Greeks which we can well do without. Great value should be attached to the diagnosis of the eruption of true leprosy. I have lately seen a most remarkable example in a patient of Mr Erasmus Wilson, which was considered by every one who had seen it to be syphilitic. The subject was a gentleman holding an official post in India, and all the members of the medical staff reported upon the case as one of syphilitic disease; the individual was mercurialized and mercurialized again, of course without any effect of a beneficial kind.

Whilst upon the subject of leprosy, I may add one or two short notes on some few points which I have ascertained during my recent trip through the East. At Cairo, leprosy is met with amongst the Christians and the Hebrews. At Jerusalem, lepers are plentiful; there is no hospital, but a quarter of the town is set apart for them. The lepers mostly come from the surrounding villages, and are generally of Arab origin. At Nabûlus a like state of things is met with; there are some 200 lepers who live outside the town. A few lepers are found in other parts of the country, but they generally congregate at Nabûlus or Jerusalem. At Damascus there are now only four lepers in the leper hospital, two men

and two women ; the others, for there were many some years ago, were destroyed during the massacre which happened five years ago. These lepers come from around Mount Lebanon chiefly, and are mountaineers. There are other instances to be met with in the Hauran. It is said that there is no known case of leprosy in a Hebrew at the present time in Syria. Stray cases of leprosy you come across here and there in the various villages in the line of tour :—by leprosy I mean elephantiasis græcorum.

In the first place, it is clear that all these poor creatures have lived in the very foulest of dwellings, without ventilation, without any attempt at cleanliness, the same room often being occupied by animals and human beings together. The food, too, used by these folk is bad : rice, lentils, sour milk and rancid zibda, which is a kind of stock-grease or butter, made by boiling down fat and butter, with the view of keeping it eatable for some time. It appears, too, that a good proportion of the people about Damascus consume olive-oil, which is often rancid. It would appear from what I have seen of the zibda or butter, that the people like things which are tasty and strong, and this is often arrived at by the addition of garlic to the very nasty fatty compound which they call zibda.

The evidence as to fish-eating habits is not altogether satisfactory. In Cairo, you meet with plenty of stale fish,—the native Christians and Jews eat it, especially a compound of preserved or potted fish which I think they call “ fasciah.” You can obtain plenty of evidence as to the nature of this mess in one part of the city adjoining the Nile, where its preparation is carried on, in the horrible stench which quite sickens you. At Jerusalem there is plenty of stale fish in the bazaars, but it does not seem to be eaten especially by any particular class ; besides, the lepers here come from various places afar off. At Nabûlus, the habit cannot be traced, nor indeed at Damascus ; but what is pretty much the same thing as before remarked, there seems to be a good deal of rancid fatty matter used by those affected. Fish-eating habits, where the quality of the fish is bad and stale, seem to be a prominent feature in the history of lepers ; the disease, in its widest migrations and topographical habitats, has always followed and does now follow, being confined almost to, the banks of large rivers and seacoasts in the most northern and cold as well as southern and warm climes. Elephantiasis on this account has been looked upon as a malarious fever ; and where there is any exception to this rule, if there be not the evidence of actual ichthyophagic habits, yet is there of that which is pretty much the same thing, viz. : the consumption of rancid oleaginous substances. It would be well that our Indian inquirers should sift this matter thoroughly, and the analogous case of the consumption of bad rice. It is curious that in England, when leprosy was common, that there should have been a large consumption of fish ; indeed, salmon was so plentiful that a special clause

used to be inserted into the indentures of apprenticeship at Gloucester, that the apprentice should not be compelled to eat it more than twice or thrice a-day. The analogies in the medical history of leprosy, and such states as pellagra, which are in great degree dependent upon bad food, are very striking, and present interesting points for observation. It is worth observing, that lepers affirm that if they eat any oil at any time, the disease (especially the pains in the limbs) is much worse. At Nabûlus, the great cause of the continuance and extension of leprosy is the complete intermarriage of lepers which takes place, and one is really surprised that the government does not legislate in the matter. I fully believe that the interdiction of the marriage of lepers would almost, *per se*, be sufficient to eradicate the disease within the term of a century. The migration of leprosy is prevented by this one circumstance. Lepers have few children; of these latter, some escape the disease, but many become affected: this may happen at any (early or late) age, generally, where hereditary transmission is very strongly marked, between the ages of six and ten. In Syria, it is thought that if intercourse take place during the period of menstruation, that the child begotten about that time is almost sure to be leprous. In Jerusalem, it is imagined by some that connexion with animals is a *vera causa*.

The form of leprosy is the tuberculous; it kills perhaps in from four to six years, or gets on to a certain pretty advanced stage, and then remains positively stationary for the rest of life. If treated in the early period, it is said to be susceptible of cure by mercurial remediation; but the evidence (three cases) on this point is very doubtful. Males are more liable than females to be affected in the proportion of about three to one. The disease is not known to be contagious in any way or degree. It has no relation to syphilis. This seems very clear: elephantiasis existed (and indeed is mentioned in the Koran under the name of "jezzam") long before syphilis was known in Syria. The latter was imported by the French, and to this day is known as the "Frank boil,"—a circumstance having of course a very important bearing upon the question of the relation of *lepra vulgaris* (alphos) and syphilis. If it be true that syphilis did not exist in eastern climes until within recent times, then is it a very significant argument against the non-syphilitic source of lepra. There are many authorities who think that *lepra vulgaris* is an old form of syphiloderma.

With regard to the contagiousness of leprosy, only one fact has come under my notice. It was that of a European who, in a tipsy state, cohabited with a leprous woman, and who, be the explanation what it may, actually became leprous, and in this patient the disease is progressing. In the East the disease is not considered to be contagious.

I have tried in this paper to explain away difficulties, and to harmonize the various observations of differing authorities. In

conclusion, it would have been interesting to have traced out the modern relations of elephantiasis; but as this is rather a wide subject, its consideration must be attempted in a separate paper. Suffice it for the present to sum up as regards the past, that Moses clearly indicated the relation of three forms of eruption, two of which were early signs of elephantiasis, the third being similar, but distinct in nature. Celsus, not seeing any relation between these eruptions and fully developed elephantiasis, classed them together under the head of vitiligo; he, moreover, did not, as Moses did, differentiate the boak or alphos. The Greeks introduced the generic term *lepra*, particularizing each species by additional names, one of which, alphos, designated Moses' boak. The Arabians adopted the Greek ideas, and from their time to the present there has been a recognition of the view which is so completely confirmed by Dr Carter's observations, that the alphos (our *lepra vulgaris*) is a disease *sui generis*; and the two forms of eruption described by Moses, by the Greeks under the terms *leuce* and *melas*, and by moderns *morphœa alba* and *morphœa nigra*, are modifications of one form of disease, and early stages or indications of that altered state of nutrition which results in true elephantiasis. That every fact tells against the theory that the Jews were affected by any form of disease peculiar to themselves, not the least being the existence of a form of eruption of ancient date in the East, known as *baras el Israili*, recognised distinctly as alphos, and as different from another form existing side by side, the *baras* of the Arabs, or our *morphœa*.

One word, in conclusion, in reference to the Leprosy Committee of the College of Physicians. I have very good reason to know, that a large number of reports have been returned to England from consuls and other non-medical men. Now, these must contain of course the prevalent opinion and prejudices of the people, some of which are most extraordinary; and inasmuch as no strictly medical criticism and analysis has been exercised in the selection of facts, a source of very considerable error may (if care be not taken) creep into and invalidate the labours of the committee, who of course deal only in great measure with what is actually, and in fact the equivalent of hearsay evidence.

ARTICLE III.—*Notes on some Points in the Administration of Chloroform.* By JOHN SMITH, M.D., F.R.C.S., Surgeon-Dentist to the Royal Infirmary, etc.

(Read before the Medico-Chirurgical Society, 7th February 1866.)

ON the late occasion, when there was communicated the account of a death supervening on the induction of anæsthesia, it occurred to me that, instead of making any remarks at the time, I might

take an opportunity of putting together a few notes on some points in the administration of chloroform, more especially as connected with such minor operations as those on the mouth, and laying them before the Society at a subsequent meeting.

In doing so now, I would briefly advert to three circumstances appearing more prominently to present themselves for consideration in cases of this kind. These are,—

First, The locality and nature of the operation to be performed ;

Second, The amount of chloroform administered, or rather the degree of narcotism induced ; and,

Third, The peculiarities of those cases where a fatal result has taken place.

It is unnecessary here to say that the tolerance of anæsthesia from chloroform is so universal, and the comparative exemption from its imputed risks so well established, by the confidence and success with which they are ordinarily encountered, that the accession of any really serious symptom in its use may be considered as a rare exception to the general rule ; and I would premise, that any remarks now advanced are in no way animadverting on the recent case alluded to, and already communicated to the Society,—a case in which the experience and resources of the operator are only confirmatory of how little avail, in circumstances of the kind, are any known remedial measures.

First, with regard to the locality and nature of the operation :—So far back as in 1858, in a paper published in the Medical Journal for November of that year, I drew attention to the fact, that in perhaps no other operations have the circumstances of the case more to be taken into account during the induction of anæsthesia than in those performed within the cavity of the mouth. The locality itself, the severe pain sometimes necessarily inflicted, the peculiar intensity and persistence of the sensory powers of the fifth pair, the rapid and simple nature of such operations as tooth-extraction in proportion to the deep state of anæsthesia demanded, and the complications attendant on the interference, here sometimes unavoidable, with the functions of deglutition and of respiration,—all tend to embarrass the operator and increase the patient's risk.

Apart from mere difficulties, the danger most directly to be apprehended here is that of asphyxia. With the mouth open, be it ever so slightly, deglutition is prevented. An accumulation of saliva or blood, or both, thus frequently in these operations collects over the upper part of the larynx, interfering with the admission of air. Under other circumstances, by pulling forward the tongue by means of the hook or artery-forceps, the danger of suffocation may, when it occurs from the mere position of parts, be so far averted, by thus at once both mechanically opening, and, as Professor Lister has shown, physiologically exciting, retraction of

those structures impeding respiration. Where, however, a quantity of fluid is lodged here, its entrance, along with or in place of air, during a deep state of unconsciousness, where even the coughing-up of such matters seems suspended, instead of tending to diminish would increase the danger. We may, however, here supplement the pulling forward of the tongue by taking the precaution of first clearing out the fauces; and as this can be done by the finger, covered with a piece of lint or the corner of a napkin, so at the same time, and by the same means, the pharyngeal muscles may be excited to action, and the base of the tongue moved forwards, rendering the larynx patent, as in the other mode.

It will be seen, however, that the danger here is to a great extent avoidable, and that the main and all-important indication furnished in these operations is to prevent as much as possible any such accumulation of fluid at all, by interfering as little as we can with deglutition. Deglutition is unimpaired far on in anæsthesia; and as the first inhalations of chloroform excite its occurrence as well as increase the flow of saliva, it is obvious that, in placing at this early period—whatever may be done during the actual performance of the operation—anything between the jaws so as to keep the mouth open, by impeding this function, there is incurred unnecessary risk. There is only one other remark I would make in this place; it is, that the respiratory movements should not be forced or augmented in any way while inhaling chloroform, as such a practice is invariably followed by a lull or cessation of the respirations, which is always to be regarded with anxiety.

Associated with the administration of chloroform, there is another familiar source of danger besides asphyxia,—namely, the accession of syncope. The causes of this complication seem divisible into those of an avoidable and those of an unavoidable nature. And such causes as appear avoidable will be found to attach more to minor than to major operations. In this, the avoidable class of cases, syncope is not produced by, but occurs simply during anæsthesia; thus differing from another class, where, from unavoidable causes, the anæsthetic becomes so far, at least, an accessory to the occurrence of fainting.

The Society may recollect that, at the last meeting of last session, I submitted a short notice on the propriety of recognising the probably greater tendency to syncope induced by any undue reclination of the head, as occasioning pressure on the vessels of the neck. Again, whether from this or from any other cause, syncope during anæsthesia, as at all other times, is most apt to occur when the patient is in the upright or semi-upright position; and it need scarcely be here said that, in order to diminish the chances of a complication so serious, the horizontal posture should in all cases be the one adopted during the exhibition of chloroform. Such, however, is far from being the position generally chosen

for minor operations. The preponderance in these operations of death associated with chloroform is no doubt attributable in a great measure to a contrary practice, it being very generally considered not worth while in such cases to make a patient lie down. The Medico-Chirurgical Society of London, on the other hand, in their report on chloroform, show that the statistics of 2586 *capital* operations performed before the introduction of chloroform, and 1860 performed under its influence, exhibit no increased mortality arising from anæsthesia, such operations, it may be assumed, being always performed in the *horizontal* posture.

Another circumstance favourable to the accession of syncope in these, and indeed in minor operations generally, lies in the small quantity of chloroform administered, and the imperfect degree of anæsthesia induced. Too frequently in such operations the attendant is an unqualified practitioner, who in employing an agent of the kind feels beyond his province. A mere whiff or driblet is in consequence made to answer the purpose. This seems, however, a practice fraught with danger. In all cases anæsthesia, to be either satisfactory or safe, should be complete. I have repeatedly noticed patients, who have had chloroform on different occasions for such operations as came under my own department, express themselves as much less uneasy after a full dose—much less exhausted; and I may mention my own personal experience of this agent to bear out such a statement. So far it is more satisfactory; but it is also more safe. We know that one of the benefits of anæsthesia is the prevention of shock during an operation. We know also that shock tends to interfere with the heart's contractions, by the influence of the cerebro-spinal system on the cardiac ganglia through the par vagum. And this tendency does not always depend on the magnitude or importance of the operation. The operation may be a minor one, and the suffering inflicted very slight. The patient may be unconscious of pain, and yet the heart may, through the ganglionic system, be affected by the operation, and syncope result. Cases are recorded (article "Anæsthesia" in Holmes' Surgery) where, during anæsthesia, the pulse fell with the first incisions in an operation. There can be no reasonable doubt that, of two patients equally under chloroform, if one be operated on in any way worthy of the term, and the other not, although neither have actually experienced pain, yet the one awakens up in a very different condition from the other—the one has suffered more or less from shock, the other has not. I am persuaded I have seen patients during anæsthesia in this way faint immediately on a tooth's extraction. Even in another variety of critical cases, where the spasmodic action, sometimes manifesting itself in the voluntary muscles, is possibly extended to the heart, the exhibition of a further quantity of the anæsthetic, if practicable, would, judging from analogy, certainly be the indication here to be

fulfilled. Whatever other danger, then, may be incurred by a free use of chloroform, there seems every reason for believing, that the more imperfect the anæsthetic state, the more risk there is of fainting; or, conversely, the more chloroform the less shock.

After a very large dose of chloroform, no doubt, the action of the heart has been shown (Med.-Chir. Reports, *Lancet*, 1864, etc., etc.) to become diminished in force and frequency. But nearly all the deaths that have occurred, it must be remembered, have taken place not at this later stage, but at the commencement of anæsthesia, when we know that both the force and frequency of the heart's action are increased. In certain low types of febrile diseases, the pulse improves under chloroform, and remains steadily more full and even less frequent during the state of complete anæsthesia. For a considerable time back I have found chloroform to answer in a similar manner, in place of the ordinary ammonia or smelling-salts, in cases of threatened syncope. And even in the experiments of Mr Wakely and of the Society of Emulation of Paris in 1855, where animals were purposely killed by this agent, it was found that the heart did not cease for some time after the respiration had become suspended. There therefore seems little doubt that any depressing effect of chloroform in ordinary cases could act as pointed out by Caspar only after time has been allowed for its neuro-paralyzing effect both on the sensory and motive nerves, and through them on the muscles, including the hollow muscles, such as the heart. But where insufficient anæsthesia is induced, we may expect untoward complications,—such as, incidental syncope, suffocative spasm from hysteria, etc. And where these do occur during semi-unconsciousness from chloroform, both their effects and that of the agent itself are rendered doubly dangerous.

In making such statements on what may be considered *avoidable* sources of danger during chloroformization, it is by no means disputed that sudden deaths have occurred apart from any causes of the kind. But the question next presents itself, Do any peculiarities exist in the latter class of cases to account for their occurrence? That there does exist some given condition, modifying the action of chloroform in such anomalous cases seems self-evident; and as chloroform acts through its influence on the nervous system, it seems to be *there* that such modifying conditions must be looked for.

The most probable cause of death, where its occurrence under anæsthesia has been unavoidable, would appear to be some disturbance in the reflex excitability of the nervous centres, affecting the heart. This may originate in a variety of ways in different instances. In many, the previous circumstances of the case, such as the prolonged existence of suffering, may lead to a condition of the nervous centres in which reflex excitability would be dimin-

ished; and, as it has been admitted that chloroform and other anæsthetics in their advanced stages produce and consequently would tend to increase this state, we can conceive, on their employment in patients already reduced in this way, that various functional nervous affections may be produced. Again, natural differences of excitability of some parts of the nerve-centres may exist in different persons, so that the reflex movements of organic life, such as those of the heart, are liable to be variously affected by the same cause. This is well known to be the case with the heart's action in different individuals. But as any *natural* modification of excitability in this organ would be more or less permanent, it would fail to explain deaths occurring under anæsthesia in patients who have at other times been anæsthetized with impunity.

Besides the influence of disease and of natural constitution on reflex excitability, we know that it may also be altered by artificial means, such as by various remedial or poisonous agents. To this, however, little importance attaches, unless, indeed, it be that, taking into account this fact, we may perhaps yet find some means of preparing, so to speak, the reflex excitability of the nerve-centres so far against the effect of any one, by the preliminary exhibition of another agent of an opposite tendency.

In this way, then, there seems no difficulty in imagining such causes as natural constitution, previous illness, or artificial agents, impairing or altering the excitability of the nerve-centres in different individuals, and thus leading to diminished reflex action,—a condition which such anæsthetics as chloroform would, as a general rule, seriously aggravate.

It does not follow, however, that the same reflex condition should in every one predispose to the same consequences. In fatal cases it would seem as if this result were sometimes the very first phenomenon exhibited as an effect of the chloroform. This may arise from the fact that its action may be limited to one set of functions alone. If, according to recent research (Brown-Séquard, *Lancet*, 6th Jan. 1866), the nervous conductors of sensitive impressions and reflex phenomena, as well as for the transmission of nervous force to the muscles, bloodvessels, etc., are absolutely distinct from one another as regards their function, it may be assumed that one set of functions may be exclusively and suddenly interfered with before or without the others being affected at all; for example, before anæsthesia has commenced, as in cases where at an early period the disturbance seems limited to the heart's action, one set of nervous functions being thus exclusively affected. And there is no necessity that even in similar cases, such as where the state of nervous exhaustion appears equally great, the same cause should produce the same effect. According to the view that the *same* peripheric cause of irritation, acting through the *same* centripetal nerves, may produce very

various effects, including every functional nervous affection or disorder, we can easily conceive the heart affected in one patient by an agent such as chloroform which might not produce the same effect on another in similar circumstances, or even on the same individual at a different time—on the same principle as in the example of cold applied to the surface producing various morbid results.

In these and other modes, then, death may occur as a contingency associated with anæsthesia in a manner altogether unavoidable, and in which no foresight could avail. But while admitting the conclusion deducible from such data to be, that chloroform may, according to previous and uncontrollable circumstances, exceptionally increase or diminish, or it may be in rare instances arrest the heart's action, we must not overlook the fact that in such cases the chloroform has not acted independently, but only as an accessory agent among others; nor must we forget that the same conditions of the nerve-centres predispose in precisely the same manner to accidents from the operation as the disease itself for which the chloroform is required. We know that sudden death has occurred immediately before and during the simplest operations without anæsthesia at all; and we are bound to suppose that at least some of the deaths attributed to chloroform are of the same nature.

In the account of the post-mortem examinations of those cases where death has taken place during anæsthesia, the want of uniformity in the appearances presented renders it unlikely that the result in all proceeded from one and the same cause. There is wanting in them that uniformity which occurs in poisoning by any given agent. Indeed, it seems questionable whether in the human subject death has been in any case a purely toxicological effect of chloroform. That it is quite possible to kill an animal by the simple accumulation of chloroform in the system acting as a poison has been frequently demonstrated. In man, however, we know that, in almost every fatal case, scarcely any accumulation whatever of chloroform could exist; while in many cases, where no poisonous effect whatever was produced, large and prolonged doses of the agent had been administered, and its effects maintained, not only for hours but sometimes for days consecutively, without any impairment but with positive improvement of the general health.

The records of post-mortem appearances, as has been said, exhibit the utmost discrepancy. In the 50 cases collected by the late Dr Snow, the condition of the heart was very various. In nearly 20, it was either gorged or tolerably filled with blood. In others, it was empty but stained with blood, showing that probably it had been emptied only at the post-mortem examination. Dr Berend of Hanover details, among others, 10 cases, in all of which the heart was empty, flaccid, collapsed, and doubled upon itself. In other

cases from time to time recorded, as in that last read before the Society, the heart was firmly contracted. In many it was diseased, and in not a few the cause of death was found in some other organ altogether, such as the spinal cord.

Again, the opinions as to the mode of death are not less various. Dr Sibson, in 1848, attributed the fatal result to death of the heart; Dr Snow, to what he termed cardiac syncope; Dr Black, to asphyxia; Caspar, to neuro-paralysis; Sanson, to both syncope and asphyxia, etc. A conflicting mass of evidence, from which it appears impossible to derive any other conclusion than that the deaths described were due to no one cause, but had been produced in a manner widely different, according to the circumstances of the case.

As the general import of these notes, then, I would wish to show, that while we find deaths associated with chloroform naturally separating themselves into the two distinct divisions of those arising from avoidable and those due to unavoidable causes, the avoidable causes are numerous and varied, while the unavoidable are of a range very limited if not invariable in their nature. The former class tells, of course, not so much against the administration as the administrator of this agent. The remainder forms an interesting series, probably all belonging to cases whose condition, altogether apart from anæsthesia, was unsuspectedly near to death, and where the chloroform acted merely as the touchstone of their hold on life. So far as we know of its effects as a toxic agent, such of the unavoidable deaths in man as have been recorded cannot be determined as necessarily essential to chloroform, but rather as the result of certain morbid conditions in which death is apt in an exceptional manner to supervene on the influence of any disturbing cause affecting certain already disordered functions of the nervous system.

ARTICLE IV.—*Case illustrating the supposed Connexion of Aphasia (loss of the Cerebral Faculty of Speech) with Right Hemiplegia, and Lesion of the External Left Frontal Convolution of the Brain.*
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(Read before the Medico-Chirurgical Society, 7th February 1866.)

BEFORE reading the case which forms the subject of this communication, it is requisite, on account of the comparative novelty of the subject, that I should explain briefly what is meant by cerebral loss of speech, or Aphasia, and also that I should premise some remarks on the present position of those questions of cerebral

pathology which the case now to be submitted more particularly illustrates.

Instances of loss of the cerebral faculty of speech have, from time to time, been noted in general literature and in the records of medicine. One of the earliest and most familiar examples is that of Zacharias, who was struck dumb on the announcement of the future birth of a son, and remained speechless till St John received his name. The cases which have been reported medically have usually been styled simply, "loss of speech," or, sometimes, "loss of the memory of words," "loss of verbal memory." They have been readily and properly distinguished from mere paralysis of the organs of vocalization or of articulation, and they have likewise been separated from cases of silence due to deafness, melancholia, idiocy, or to defective intelligence. The peculiar features, indeed, and the combination of circumstances which constitute cerebral loss of speech are curious and characteristic, and may be briefly summed up as follows:—

1st, In the first place, the patient has, in a peculiar manner, lost the power of expressing himself by words. The loss may be complete, so that he emits no sound, or does nothing but mumble; or the loss may be partial. The partial forms are often extraordinary. Sometimes the patient can speak certain words quite distinctly, while he does not even attempt to say others quite as simple; sometimes he can only say "yes" or "no," using them often without reference to their meaning; sometimes he has forgotten all his vocabulary except some familiar oath, or some short word of his own coining, which he utters automatically on all occasions. In other instances, the patient mutilates words in various ways, pronouncing half-words only, or giving words some uniform artificial ending: or, again, he misplaces words, using them in a wrong sense; or, lastly, he omits certain classes of words altogether; thus, some persons leave out the nouns, others the verbs from their sentences. Illustrations of all these forms are well known. It is evident that these different varieties require special study, but for the present purpose they may be all classed together as examples of Aphasia, or speechlessness,—the impairment, perversion, or loss of the cerebral faculty of speech or articulate language.

2d, The second peculiarity of the loss of speech now under consideration is, that it cannot be accounted for by defective intelligence, that is, by want of ideas to express. Nothing indeed is more evident than that the patient thinks, and that he is extremely anxious to communicate his thoughts. His face and eyes appear full of expression, and he makes eager efforts to convey his meaning, and, when he fails, he betrays his disappointment, according to the nature of his disposition, either by a good-natured smile and laugh, or else by frowns or tears. He frequently resorts to signs and gestures to assist him in his difficulty: and

it is very remarkable that, in some cases, when the power of vocal speech is lost, the patient retains the ability to write distinctly what he wishes to communicate. In other cases, however, the power of written language is impaired or lost as well as vocal speech; and in the worst cases, even gestures and other imitative signs are wanting.

3d, The third characteristic of this kind of loss of speech is that it is not due to peripheral paralysis of the organs of vocalization or of articulation; for these organs are either entirely exempt from paralysis, or if some facial or lingual palsy be present as a complication, it is not sufficient to prevent, rarely even seriously to interfere with the articulate pronunciation of words.

The defect of speech, therefore, in the class of cases under consideration, is cerebral only. The patient has thoughts, and has voice and tongue and lips capable of uttering them; but some of the links in the process are missing: either that link by which the mind translates ideas into the corresponding conceptions of sound, called words, or that link by which the brain converts the words conceived in the mind into the initiative impulse by which the apparatus of vocal speech is set in appropriate action.

Two kinds of Aphasia have accordingly been distinguished. 1st, Amnesic aphasia, loss of speech depending on defective memory of words, and therefore to some extent a psychological defect; and, 2d, Ataxic aphasia, where the loss of speech is due to lesion of a supposed cerebral apparatus of co-ordination for the movements of articulate speech,—a defect in the nervous mechanism only.¹ These two kinds are chiefly distinguished by the circumstance that in the former (the amnesic aphasia) the patient has lost the ability to write as well as to speak; while in the latter (the ataxic aphasia), he retains the power to write, but cannot articulate. The distinction is important clinically; but the theory of a distinct co-ordinating centre in the brain for articulate speech suggests anatomical and physiological questions which it would be out of place to discuss here, and the discussion of which is as yet probably premature. A cerebral organ of impulse is all that theory would require to account for Ataxic aphasia.²

From this sketch of the definition, I pass to a few remarks on the pathology of Aphasia.

There is probably no physician who has not, in his own practice,

¹ Lesion of a special co-ordinating centre is M. Bouillaud's theory. M. Lordat, the distinguished Professor of Physiology in the University of Montpellier, who was himself, many years ago, the subject of aphasia ("alalia," as he calls it), ascribed the loss of articulation to the defective *synergy* (i.e. associated action) of the muscles which form the movable apparatus of speech.

² I have not yet seen a case of aphasia without verbal amnesia, and without some impairment of the mental faculty of attention; and I should be disposed to regard some degree of amnesia as probably present in all cases of aphasia.

met with striking examples of the kind of speechlessness now described. The cases are not indeed rare, but they have nevertheless been usually rather looked upon as curiosities than been either extensively or accurately studied, and their pathology has been held to be obscure and inscrutable. Within the last four years, however, attention has been particularly directed to them in France, in consequence chiefly of the observations of M. Auburtin¹ and the dissections of M. Paul Broca.² Last year (1865), the whole subject was fully discussed at the French Imperial Academy of Medicine in a series of brilliant debates, in which MM. Bouillaud, Trousseau, Parchappe, Baillarger, Velpeau, and other eminent men of the Parisian School took part. To the record of that discussion, in the *Bulletins de l'Académie*, I would refer those who may wish to examine their various opinions in detail. In this country, partly through Dr Hughlings Jackson's paper and cases in the *London Hospital Reports* (vol. 1), and partly through Trousseau's Lectures,³ a certain degree of interest has been roused, and investigations are now being directed to the subject. Meanwhile, for the purpose of facilitating discussion and inquiry, a special name has been applied to the cerebral loss of speech here described. No less than three designations, of similar meaning, have been proposed,—*Alalia* (α and $\lambda α λ η ν$), an old name revived; *Aphemia* (α and $\phi η μ ι$), invented by M. Broca; and, lastly, *Aphasia*, recommended by M. Trousseau.⁴ The last name has the advantage of being classical Greek, while the other two are of rather spurious manufacture. *Aphasia*, in the original language, means the speechlessness due to fear, amazement, or violent emotion; and it may, therefore, with the necessary modification of its meaning, be transferred to indicate the pathological condition of cerebral loss of speech. It is the term I shall use by preference.

¹ *Considérations sur les Localisations cérébrales, et en particulier sur le Siège de la Faculté du Langage articulé*, par le Dr Ernest Auburtin. Paris, 1863.

² *Sur le Siège de la Faculté du Langage articulé, avec deux Observations d'Aphémie (perte de la Parole)*, par le Dr Paul Broca: 1861. Also, *Remarques sur le Siège, le Diagnostic et la Nature de l'Aphémie*: 1863. These are reprints from the *Bulletins de la Société Anatomique*.

³ *Clinique Médicale de l'Hôtel Dieu*, or English translation by Dr Bazire, the first part of which, containing the lecture on Aphasia, has lately appeared. See also collection of cases, and an able review of the subject, by Dr Falret, in *Arch. Gen. de Méd.* for 1864, vol. i. pp. 336 and 591; and vol. ii. p. 80; also, Russel in *Brit. Med. Journ.* ii., 1864; and an interesting case and remarks by Professor Banks, in *Dub. Jour. Med. Sc.* for 1865, p. 62, etc., etc.

⁴ "Aphasia" was suggested to M. Trousseau by M. Chrysaphis, a modern Greek, and eminent scholar, and approved by the distinguished M. Littré. As the term "*alalia*" was previously in use, the necessity for the invention of "*aphémie*" by M. Broca is not apparent. Nevertheless, two other names have been proposed, derived from α and $\phi λ θ η γ ο μ α ι$, viz. *Aphthongie* (in *Gaz. Hebdom.*, April 1865) and *Aphthensis* or *Aphthensio* (in *Dub. Quart. Jour. Med. Sc.*, 1865, vol. xl. p. 486.) Broca now proposes "*Aphrasie*."

It is proper to remember that the recent animated discussions in Paris are only the revival of the excitement which attended the first introduction of Phrenology. The phrenologists had undoubtedly the merit of first directing special attention to the cases now called Aphasia. Unfortunately their observations on the subject were associated with their system of cranioscopy, and necessarily shared the neglect into which the latter fell among medical men. While the system of phrenology as a whole, however, met with few supporters, certain parts of the system were made the subject of clinical and pathological investigations of great value. Among these the researches of Bouillaud on the cerebral seat of the faculty of speech are among the best known and the most remarkable.¹ As early as 1825, this distinguished observer adduced a series of clinical cases to prove that the faculty of speech had its seat in the anterior cerebral lobes. Bouillaud founded this opinion on cases which he believed to demonstrate that, whenever the faculty of speech was affected during life, disease of the anterior lobes was invariably found after death; that disease of the other lobes had no similar effect; and that destruction of the anterior lobes never occurred without abolition of the faculty of speech. These opinions M. Bouillaud has repeatedly re-asserted (in 1839 and 1848), and he still maintains them (in 1865). They have been controverted, as is well known, by Lallemand, Andral, Cruveilhier, and others, and in this country his views have never received general acceptance. Bouillaud's observations, however, constitute the first period in the history of the special opinions relating to our subject; a period the doctrine of which is to localise the faculty of speech in the anterior lobes of the brain.

The next period occurred in 1836, when M. Dax, a physician in the south of France, read a paper at the Medical Congress of Montpellier, on the relation of loss of speech to disease of the left hemisphere of the brain.² Aphasia, it is well known, may occur alone, without any paralysis of the body; but, on the other hand, it is very frequently accompanied by hemiplegia. M. Dax propounded, as the result of his observations, the very remarkable statement, that in cases of loss of speech the lesion is invariably found in the left hemisphere of the brain, and that consequently, whenever the loss of speech is accompanied by hemiplegia, the paralysis invariably occurs on the right side of the body, and never on the left.

¹ Bouillaud, Arch. Gén. de Méd., 1825, t. viii. p. 25; also, *Traité de l'Encéphalite*, Paris 1825; also in 1839, in Bull. de l'Acad. de Méd., t. iv. pp. 282, 333, etc.; and, in 1848, Bull. de l'Acad., t. xiii. pp. 699, 778; finally, in Bull. de l'Acad. Imp. de Méd. in 1865.

² *Lésion de la Moitié Gauche de l'Encéphale coïncidant avec l'Oubli des Signes de la Pensée*. This interesting memoir has been reprinted in the *Gazette Hebdomadaire de Paris*, 28th April 1865, p. 259. It has the appropriate motto from Virgil, *Memini, si verba tenerem*.

M. Dax did not, like M. Bouillaud, limit the seat of Aphasia to the anterior lobes, but he restricted it to one hemisphere, viz. the left, to the exclusion of the right. Already, in 1836, M. Dax had collected forty cases of loss of speech with lesion of the left cerebral hemisphere, without meeting with a single instance of the coincidence of Aphasia with disease of the right half of the brain. His memoir seems at the time to have attracted little attention in the medical world, and had fallen into oblivion till the recent observations of Auburtin and Broca recalled attention to it. It refers, however, to one of the most curious points of the whole inquiry, the connexion of Aphasia with right hemiplegia, and with disease of the left cerebral hemisphere.

The third period in the progress of opinions commenced during the recent revival of attention to the subject in 1861, when M. Auburtin, a partisan of M. Bouillaud, brought forward anew that author's views at the Anthropological Society of Paris. M. Paul Broca, who was present, happened to have at the time under his care, in the General Infirmary of Bicêtre, a patient almost moribund, who had been the subject of Aphasia for twenty-one years. M. Auburtin offered to stake the truth of Bouillaud's opinions upon the result of the dissection in this case. The autopsy, which was made soon after, decided in favour of Bouillaud's views; disease was found in the anterior lobe, more especially in the left external frontal convolution and adjoining parts. M. Broca was so struck by the coincidence that he became inclined to adopt and to support M. Bouillaud's views by further investigations. (M. Paul Broca's adoption of his views is facetiously called by Bouillaud the Conversion of St Paul). In carefully and critically dissecting the brains in other cases of Aphasia, M. Broca came to the conclusion that the seat of the faculty of spoken language might be even more precisely defined than had previously been attempted. He worked out this opinion by the following stages. Apparently without any knowledge of M. Dax's previous opinions, M. Broca arrived independently at the conviction, that in Aphasia the lesion is confined to the left side of the brain; in accordance with Bouillaud's researches, he placed it in the anterior lobes; and, finally, as the special result of his own dissections, he assigned as the special seat of the faculty of articulate language the posterior third of what he calls (after Wagner) the third left frontal convolution, that is, the most external or inferior frontal convolution, where it forms the anterior margin of the fissure of Sylvius. (This is the *étage surcilier* or *frontal inférieur* of Gratiolet;¹ it is the third, external or inferior frontal convolution of Wagner, *gyrus frontalis tertius*, *s. inferior*, *s. externus*).²

¹ *Memoire sur les Plis Cérébraux de l'Homme et des Primates*, Atlas, Pl. I. fig. i. 1.

² Rudolph Wagner, *Abhandlung über die typischen Verschiedenheiten der Windungen der Hemisphaeren*. Goettingen, 1860, Atlas.

There are therefore three positive opinions, different, but not contradictory, in regard to the seat of the faculty of speech, or, what is the same thing, the seat of the lesion in Aphasia, and its connexion with hemiplegia.

1. *Bouillaud's opinion*.—That Aphasia depends on lesion of the anterior lobes, without restriction to side.

2. *Dax's opinion*.—That Aphasia depends on lesion of the left hemisphere, and, when accompanied by hemiplegia, the paralysis is always on the right side of the body.¹

3. *Broca's opinion*.—That Aphasia depends on lesion of the posterior third of the third, external or inferior left frontal convolution (*étage surcilier ou frontal inférieur* of Gratiolet).

Such are the positive opinions. I shall meanwhile omit the negative or contrary opinions, which maintain that there is no special lesion known to be connected with Aphasia, not because the negative evidence is unimportant, for in some respects it is very strong (especially Velpeau's case of the talkative barber who presented almost total destruction of the anterior lobes by tumour), but because the case I have to submit belongs to the positive side of the question, and the discussion of the entire subject would occupy more time than is at present at our disposal. I now proceed, therefore, to read the account of my case.²

Aphasia; Right Hemiplegia; Gangrene of left leg; Softening of external or inferior left frontal convolution; Softening above the posterior extremity of the Sylvian fissure.

Margaret Mackie, æt. 43, domestic servant, was admitted to the Royal Infirmary on 16th November 1865, and died there on 10th January 1866.

This patient was a pale, thin woman, with an appearance of anxiety and excitability.

Her principal symptom was defective speech and loss of the memory of words. She could speak a few words consecutively quite well, but, after commencing a sentence, she either stopped abruptly, or concluded it in a manner altogether unintelligible. She stated that her Christian name was Margaret, but she could not tell her surname. She could not state her age exactly. From the attempts she made to give some history of her ailments, it was gathered that she had been taken ill about the date of last term day (11th November), and that she had procured a powder which made her vomit, but without affording any relief. In making out what she told us, we had to guess in great part, and she signified her assent when we guessed rightly, but shook her head when we interpreted her meaning wrongly. She was greatly distressed that she could not make herself better understood. In the little history she gave, she called "druggist" "yollit;"

¹ Of course, right hemiplegia may occur without aphasia.

² From the report of the case by Mr James Ramsay, Clinical Clerk.

"retched," "rubbished;" and Duncan and Flockhart, the well-known firm of druggists, she made "Doctor and Forrit." By the assistance of her sister, we ascertained that for nearly a year before the attack her friends had observed her disposition change; she had become very irritable, easily excited, and apt to be violent in temper. On going to a new situation on 11th November, she did not feel well, but nothing was noticed till the 15th, when she fell down on getting out of bed in the morning. She lost her speech, frothed a little at the mouth, and she partially lost the power of the right hand and leg. She did not lose consciousness, and she had no distortion of the face. As these symptoms appeared alarming, her friends brought her to the Infirmary.

Besides the defects of speech above mentioned, Mackie presented the following symptoms on admission. The paralysis of the right hand and leg had greatly diminished, and was now inconsiderable. She had somewhat less power of grasp in the right hand than in the left, but the power of voluntary movement had returned to the right leg, and she complained more of weakness, loss of power, and pain in the left leg, which she could, however, move voluntarily without difficulty. The movements of the tongue, lips, cheeks, mouth, and throat, could be freely performed; there was no difficulty of deglutition. The right pupil was larger than the left; but the sight and other senses were natural. There was no distinct facial paralysis. At times there seemed some deficient power on the right side of the face, with some drawing of the features to the left, but these signs were never unequivocal.

In regard to the symptom last mentioned, the facial paralysis, I have been surprised to read in M. Broca's pamphlet the following statement (p. 20):—"Il est inutile de rappeler que les paralysies de cause cérébrale sont croisées pour le tronc et les membres et directes pour la face." ("It is unnecessary to remind you that the paralyzes due to a cerebral cause occur in the opposite side from the cerebral lesion in the body and limbs, but on the same side in the face.") In place of this being a received axiom, as he puts it, it is the very reverse of the truth. Paralyzes due to a cerebral lesion are crossed for the face as well as for the limbs (the limbs and face of the same side being paralyzed), and the trunk of the body is usually not affected. It is a matter of regret to find an observer so distinguished as M. Broca falling into a grave clinical error which the accurate experience of every day contradicts.¹ But to return to my case:—

Two days after admission, the patient complained of pain and loss of power in the left leg, which she dragged in walking. (She could at this time walk a little when assisted.) At first this

¹ In the rarer cases where the face is paralyzed on the side of the lesion, and the limbs on the opposite side, the lesion will be found in the pons varolii, not in the cerebral hemispheres. See Romberg. M. Gubler names this form "hemiplegie alterne," alternate hemiplegia.

appearance of a cross paralysis, attacking the left leg, after having previously affected the right side, surprised me, and suggested whether it were possible that there might exist a lesion in both hemispheres of the brain. But the true explanation was soon discovered. On examining the left foot and leg, sensation was found defective as well as motion; acute pain and tenderness (hyperæsthesia) were complained of in the calf and upper part of the leg; and while the thigh and calf were of the natural colour and temperature, the foot and lower part of the leg were white and of almost icy coldness. On examination, very slight pulsation could be detected in the femoral artery, and, after a few days, no pulsation whatever could be felt. The paralysis, therefore, in the left leg, was not due to a centric or cerebral nervous lesion, but to the arrestment of the supply of arterial blood in the limb; that form of paralysis from arterial obstruction which has been so graphically described by Romberg. The foot, from being white as marble, soon became discoloured; gangrene set in and spread up the limb without forming any definite line of demarcation. By the time of her death, which was chiefly due to its progress, the mortification had extended above the knee to the lower third of the thigh. In regard to the surgical treatment of the case, I had the benefit, on several occasions, of Professor Spence's advice, who recommended appropriate dressings, but was satisfied of the impropriety of any operative interference.

The occurrence of obstruction of the femoral artery plainly suggested the idea that the lesion in the brain, to which the Aphasia and the right hemiplegia were due, had been produced by embolism of the artery of the left Sylvian fissure, the consequence of which had been softening of the external or inferior left frontal convolution and the parts adjoining (in accordance with M. Broca's views), producing the Aphasia, while the influence of the lesion acting on the corpus striatum of the same side had given rise to the slight attacks of right hemiplegia. A diagnosis was proposed in conformity with these opinions, which were discussed in a clinical lecture on the case.

The heart was carefully examined on repeated occasions. Its action was often irregular; a slight roughness could be detected with the first sound, but no distinct murmur was audible. The radial pulse was 84 on admission, with occasional interruptions in the beat.

The other functions presented nothing remarkable.

With the exception of increasing weakness, and other symptoms due to the progress of the gangrene of the leg, Mackie remained nearly in the same condition as on admission. She had twice slight convulsive attacks, as reported by the nurse, but these were not observed by the resident physician, and they left no traces when she was afterwards seen at visit. Her loss of speech and of memory (aphasia and amnesia) varied at different times. When

reminded of her surname she could pronounce it quite well, and some days she remembered it without prompting; at other times she entirely forgot it. Some phrases she pronounced well, and, in fact, somewhat hurriedly and volubly, often repeating some of the last words several times. I often heard her say, at visit, "I'm no better to-day,—better—better." In alluding to the gangrened leg, she would say "Thing—thing—oh, it's not at all—not at all," but could not complete her sentence. Sometimes she answered questions quite intelligently, and with considerable command of words; at other times her only response would be Yes'm or No'm, used indiscriminately and without meaning. It is curious that she never said Yes, sir, or No, sir; probably Yes, ma'am, was more habitual to her. As her strength declined, her memory of persons and facts appeared to fail. She sometimes did not recognise her sister, although she always seemed to recognise the medical attendants. Her intelligence also was evidently weakened latterly; for although she could understand what was said, and could express her wants, and, to some extent, her ideas, yet it was plain that she did not altogether realize her situation, nor reflect upon it as she would have done if her faculties had preserved their integrity. She was not, however, in the least idiotic, but only seemed in a maze, not clearly understanding what had occurred, or was occurring around her.

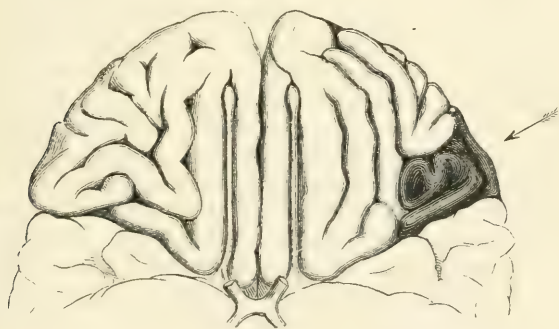
On 24th November, at my request, Mackie attempted to sign her name. She formed the capital letter "J" tolerably well, followed by some indistinct small letters. She knew that "J" was not the proper letter to commence her name, and she spontaneously tried again to write more correctly, but she still repeated the same letter; at last she gave up, intimating that she couldn't write any other letter, and she seemed much disappointed at her failure. "I can't do it—do it," was her expression.

There could be no doubt that this was a characteristic case of Aphasia, more particularly of the variety known as Amnesic aphasia, in which the loss of the memory of words is more marked than the inability to articulate, although articulation also was impaired. Mackie could pronounce sentences when she knew the words; and the evidence of her writing proved that her remembrance of written characters was as defective as her memory of vocal words.

The post-mortem examination, which excited considerable interest, as bearing upon M. Broca's views, was performed by Dr Grainger Stewart, Pathologist to the Infirmary, on 12th January, twenty-seven and a-half hours after death, the weather being cold. The head alone was allowed to be examined by the patient's friends, whose consent to an autopsy was obtained with difficulty.

The skull-cap was natural; there was some serous subarachnoid effusion. The right hemisphere of the brain presented no lesion. On carefully examining the left hemisphere, the posterior part

of the external or inferior left frontal convolution, where it forms the anterior margin of the fissure of Sylvius, together with a small portion of the adjoining orbital convolution, were observed to be collapsed, and depressed below the natural level. The flattened and depressed portions felt soft and fluctuating to the touch, like a bag containing fluid, presenting a marked contrast to the firmness of the neighbouring healthy parts of the brain. The grey matter of the diseased convolutions was smooth and intact on its external surface; but, on cutting into the softened part, the grey matter was found to be thinned off from within, and the white cerebral substance was completely softened and eroded, presenting an appearance like dirty cream. The softened part extended inwards for about three-quarters of an inch, reaching to the immediate neighbourhood of the left corpus striatum, without, however, affecting it. Under the microscope the softened portions consisted entirely of granules and granular cells, the nervous structure being entirely destroyed. The woodcut, copied from a sketch made by Mr



Ramsay, clinical clerk, at the autopsy, shows the situation of the lesion as seen upon the inferior surface of the brain. The other convolutions of the left anterior lobe were not affected, and the island of Reil and the adjoining portions of the parietal and temporo-sphenoidal lobes were healthy. Near the posterior extremity of the fissure of Sylvius, however, there existed a separate softened portion in the situation of the anterior and lower part of the lobule of the superior marginal convolution of Gratiolet (*lobule du pli marginal supérieur*, Pl. I. A. of his Atlas). Like the former softening, this could be detected externally by the sinking of the convolution, and by the soft touch and feeling of fluctuation communicated to the finger. Here, also, the superficial grey matter was not destroyed externally, but internally it was softened, and the adjoining white nerve substance was eroded and softened to the aspect of dirty cream, over an extent of about three-quarters

of an inch from before backwards, about the same area from without inwards, and about half-an-inch from above downwards. This softening presented the same appearance under the microscope as the portion previously described.

No other lesion of the encephalon was found; the corpora olivaria were normal.

The arteries of the brain were carefully dissected, but no embolism was detected. With the exception of a little thickening in the wall of the artery of the left Sylvian fissure, no alteration was present in the arterial coats. If an embolism had existed, it had been broken up or displaced, or there may have been minute embolisms in the microscopic vessels within the cerebral substance, which, however, were not looked for, in order to avoid injuring the specimen. For the reason previously stated, the other internal organs were not examined. The left femoral artery was found completely occluded by a firm decolorized clot.

This history I have considered worthy to be brought before the Society, because it is, so far as I am yet informed, the first case completed by an autopsy which has occurred in Great Britain since attention has lately been directed to the subject by the French observers. The coincidence is singular, that the autopsy should confirm so precisely M. Broca's views as to the localisation of the seat of the cerebral lesion in Aphasia. The principal softening was found in the very spot which he had indicated in his cases, and where we were, accordingly, led to seek for it. At the same time, the present case could not of itself have been considered demonstrative, since there existed a separate and considerable softening in the left parietal lobe, near the upper and posterior termination of the Sylvian fissure. Probably this additional lesion may account for the greater loss of memory seen in Mackie than is observed in other cases.

It would be an absurdity in science to lay much weight on any coincidence, however extraordinary, and I should not be hasty to draw conclusions from the case just narrated. But it is evident that the questions of cerebral action, in relation to speech, have reached that stage in which definite observations are attainable, either in support or in refutation of professed opinions. There is every reason to expect that renewed investigations may now lead to fixed conclusions, and may impart greater precision and certainty to cerebral diagnosis. The post-mortem examinations of the surface of the brain must in future be made with great accuracy, in reference to the systems of convolutions on the basis of Gratiolet's classification.¹ It is unnecessary to enlarge on the value, both

¹ For an excellent example of this mode of description by convolutions, I would refer to Dr Kenneth McLeod's elaborate paper on a case of Degeneration and Atrophy of the Cerebrum, published in this Journal, vol. x. p. 323, for 1864. I am much indebted to Mr Turner, Demonstrator

theoretically and practically, of such researches. Theoretically, the question is suggested, whether the two hemispheres of the brain, though symmetrical in form, are, contrary to common belief, different and independent in function? And practically, the mere fact of the almost invariable coincidence of Aphasia with right, to the exclusion of left hemiplegia, is of great importance. Although my reading has acquainted me with authentic examples of loss of speech with left hemiplegia (*e.g.* in Bright's Hospital Reports), yet, in the immense majority of recorded cases, the paralysis has been on the right side. Baillarger sums up 155 recent cases, of which only 10 were left hemiplegia,—*i.e.* in Aphasia, right is to left hemiplegia as 15 to 1. My own cases of Aphasia with hemiplegia during the last four years amount to 9, together with three cases which I have seen under the care of other physicians, and in all of them the hemiplegia has been on the right side of the body. The experience of my medical friends has, as yet, presented me with only one exceptional case of Aphasia with left hemiplegia. Probably to-night the members of the Society will materially enlarge my experience in this and other respects. The whole subject requires and invites fresh investigation. The new opinions proposed are so important, and differ so widely from notions commonly received and held to be established, that they cannot be accepted without the most ample and conclusive evidence. Perhaps on some future occasion I may bring before the Society some of the cases which I have still under observation; but, meanwhile, in place of adding more, I feel I ought rather to apologize for the unexpected length to which, in the course of its preparation, this communication has extended.

ARTICLE V.—*Thermometric Observations on the Fevers of Children.*

By G. STEVENSON SMITH, L.R.C.S.E., Resident Medical Officer,
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THE prominent place given to the "Thermometry of Disease" in Dr Aitken's work on the Science and Practice of Medicine, has had the effect of drawing the attention of the profession to a diagnostic agent, which hitherto in this country has not received so much notice as its importance decidedly deserves. For although more than a century has elapsed since Antonius de Haen pointed out to his pupils the value of the thermometer in the diagnosis of disease, and notwithstanding Wunderlich's efforts to impress medical men

of Anatomy in our University, for his politeness in exhibiting to me a series of specimens of brains of apes in comparison with marked specimens of human brains in his possession. This is the best way to master what, if studied on the human brain alone, is a very complex subject, *viz.* the grouping of the convolutions.

with a sense of its usefulness in all clinical examinations, it is only within the last few years that British practitioners have given this subject any serious consideration.

Like everything new in medicine, the thermometer has had to serve its probationary period of obscurity and neglect; and before being recognised as an auxiliary agent in the detection of disease, it has had to bear the searching scrutiny of that scepticism so characteristic of the age, which, while always wholesome, is too frequently retarding and obstructive.

Now, however, the practical importance of daily thermometric observations in acute diseases is admitted; and in many of our hospitals careful records are made, not of the state of the pulse or the number of the respirations merely, but of the patient's temperature also. In fevers especially, a class of diseases in which the heat of the body is so much increased, the thermometer is of the greatest assistance both in detecting the disease and in affording indications of its probable course, duration, and termination.

A knowledge of the state of the temperature is of the utmost consequence in enabling the physician to arrive at a correct diagnosis in many of the affections of children; and it is here, I think, more than in the case of adults, that the medical man will derive the greatest benefit from the use of the thermometer. For how puzzling it often is, in the first stage of a febrile affection in a child, to make out with certainty the real nature of the disease. And how frequently does an illness, which was ushered in by almost all the symptoms of fever,—a quick pulse, headache, thirst, loaded tongue, etc.,—deceive both the practitioner and the parents, and pass away almost as rapidly as it came! Every one knows the difficulty of making a correct diagnosis in many of the diseases of childhood; and as an evidence of this, patients are frequently sent into the fever wards of an hospital, whose illness turns out to be only trifling, and never goes on to fever at all. Now, it is in such cases that the temperature of the body, as ascertained by the thermometer, affords information which renders mistake impossible.

The cases in which I have made thermometric observations, and which I here report, all occurred in the Children's Hospital, and I am indebted to the ordinary physicians for permission to make these observations public.

In all of them the state of the temperature was ascertained twice every day, generally about ten o'clock in the morning and seven in the evening. Of course, in an hospital, the facilities for making such observations are much greater than in private practice, the patients being almost entirely under constant supervision and control. But a note of the state of the thermometer, even once a-day, would in most cases be sufficient to indicate how the fever was progressing.

Besides recording the temperature, I have likewise made a careful note of the daily state of the pulse and the respiration in

each case. For as the amount of heat generated in the body bears a direct relation to the activity of the respiration, and as the respiration again exercises an influence upon the circulatory system, it is of importance, while observing the temperature, to notice at the same time the number of the respirations and beats of the pulse in a minute.

Before proceeding to report the cases individually, it may be of some interest to make a few remarks regarding the instrument used and the mode of applying it.

The Instrument.—The instrument used in making my observations was Aitken's curved thermometer, made by Casella of London; and while I have every reason to be satisfied, both with its delicacy and its accuracy, I beg to offer a few suggestions in regard to its construction, which, if carried out, would, in my opinion, be very useful. I have found in using it, especially on a dark day, or in gas-light, that it is extremely difficult to make out with exactness the figures and markings on the glass. It would be an immense improvement were the numbers and degrees tinted with black, or any other colour, so that the eye could more readily distinguish the height of the mercurial column. It would also be advantageous, in making observations on young children, to have the curved portion of the instrument made considerably shorter.

How to apply the Thermometer.—In order to obtain the true state of the temperature of the body, the thermometer must be applied to some part which is not exposed to the air; and the best and most convenient place to insert the instrument is in the axilla. With a child in bed this is easily and accurately done, by making the patient lie on the back, and then introducing the bulb of the instrument under the anterior margin of the pectoral muscles, so that the mercury is surrounded by the soft parts. A better way than this, however, is to make the child lie on the side, and introduce the bulb into the arm-pit from behind. I have found that in this way the instrument is more firmly and closely embedded in the parts, and the stem passing up behind the shoulder is not breathed upon.

The thermometer should be allowed to remain in this position as long as the mercury continues to rise. From three to five minutes is the time usually required. It saves time to heat the bulb a little in the hand before introducing it.

The height of the mercury must be noted while the instrument is *in situ*.

The Normal Standard of the Temperature in Children as compared with that of Adults.

It has been ascertained, by numerous and carefully-conducted experiments made by Davy and others that in adults the normal temperature of the body is about 98.4° Fahr. In infants, according to the observations of M. Roger and Dr Holland, the

temperature is somewhat higher, when they are placed in conditions favourable to its sustenance. When examined immediately after birth, it is nearly 100° , but it quickly falls to 95.5° , and again rises in the course of twenty-four hours to 97.7° in weakly subjects, and to 99.5° in strong ones. Between four months and six years of age M. Roger found the average temperature to be 98.9° , and between six and fourteen years 99.16° (Carpenter's Physiology). Dr Bennett, again, in his Outlines of Physiology, states that in children the heat of the body is about 2° higher than in adults. As the ages of the patients on whom the following observations were made range from three to ten years, I think we shall be pretty nearly correct if we assume the normal standard to be 99° . In disease the temperature has been known to rise as high as 112° , and to sink as low as 67° .

Three Cases illustrative of the Value of the Thermometer as a Diagnostic Agent.

CASE 1.—M. M., æt. 8, was admitted on 5th January 1866. She was said to be suffering from fever, her illness having commenced five days previously with rigors and headache. On admission, the tongue was slightly coated, and red at the tip; pulse quick, but there was no rash. The thermometer soon removed all doubt, and enabled us to say with certainty that the child had no fever, and she was consequently sent home.

M. indicates morning, E. evening.

			Pulse.	Resp.	Temp.				Pulse.	Resp.	Temp.
Jan.	5.	E.	108	48	99°	Jan.	7.	M.	84	30	95°
	6.	M.	108	30	98			E.	84	30	$95\frac{1}{2}^{\circ}$
		E.	96	30	98						

The gradual fall in the temperature to 95° is remarkable.

CASE 2.—R. N., æt. $3\frac{1}{2}$, was admitted January 13, 1866, said to be affected with fever; his illness having been preceded about a week previously by cold shivers, headache, and thirst. When admitted he had a foul tongue, quick pulse, and other symptoms of continued fever; but the temperature never rose above $98\frac{1}{2}^{\circ}$, and soon went down to $95\frac{3}{4}^{\circ}$, so that it became evident that he had no fever, and he was accordingly dismissed.

			Pulse.	Resp.	Temp.				Pulse.	Resp.	Temp.
Jan.	14.	M.	130	32	$98\frac{1}{2}^{\circ}$	Jan.	15.	M.	110	32	97°
		E.	96	32	$95\frac{3}{4}^{\circ}$		16.	M.	100	32	$96\frac{1}{2}^{\circ}$

CASE 3.—C. F., æt. 6, was admitted on January 2, 1866. Ill about a week, the illness having commenced with vomiting, headache, etc., and it was thought that she had contracted fever. On admission the pulse was quick, tongue foul, but, as will be observed, the temperature never rose above the normal standard, and she was dismissed quite well.

			Pulse.	Resp.	Temp.
Jan.	2.	E.	120	36	98 $\frac{2}{5}$ °
	3.	M.	96	36	96
		E.	96	36	95 $\frac{2}{5}$

Here again the unusually low temperature of 95 $\frac{2}{5}$ ° was observed.

Ten Cases of undoubted Typhus, with daily Records of the Temperature, the Respirations, and the Pulse.

In all of the following cases the eruption of typhus was distinct. Most of them were admitted about the end of the first or the beginning of the second week, but it is always a difficult matter to make out with absolute certainty the very day on which a fever begins. We usually count from the day the child took to bed, or from the day it last appeared to be in its usual health.

CASE 1.—J. J., æt. 8, was first admitted, said to be ill with fever, on December 13, 1865, but he never showed any symptoms of fever, and was sent out apparently quite well in a day or two. Very soon afterwards, on December 26, he was readmitted on the recommendation of a medical man, who thought the boy was suffering from typhus. He had a fresh colour, a moist clean tongue, and appeared to be quite well, so that the nurse imagined that the boy was scheming. The steady rise of the temperature, however, indicated that some fever was impending, and the appearance on the 29th of the typhus rash confirmed the accuracy of the thermometer.

		Pulse.	Resp.	Temp.			Pulse.	Resp.	Temp.		
Dec.	27.	E.	108	24	99°	Jan.	2.	M.	136	38	102°
	28.	M.	108	24	101 $\frac{2}{5}$			E.	152	36	102 $\frac{3}{5}$
		E.	120	32	103 $\frac{3}{5}$		3.	M.	120	36	100
	29.	M.	108	36	102 $\frac{4}{5}$			E.	115	36	102
		E.	120	32	103 $\frac{3}{5}$		4.	M.	120	36	101 $\frac{1}{5}$
	30.	M.	120	36	102 $\frac{4}{5}$			E.	108	36	100
		E.	128	37	104 $\frac{1}{5}$		5.	M.	108	36	99
	31.	M.	136	40	104 $\frac{1}{5}$			E.	96	36	99 $\frac{3}{5}$
		E.	140	40	103		6.	M.	84	32	95 $\frac{3}{5}$
1866.							7.	M.	84	32	95
Jan.	1.	M.	132	40	101 $\frac{3}{5}$		16.	M.	84	32	97 $\frac{4}{5}$
		E.	132	36	102 $\frac{2}{5}$		24.	M.	84	30	97

In this case the height of the fever was reached on December 31st, when the thermometer stood at 104 $\frac{1}{5}$ °; after this defervescence went on gradually, and the fall to the normal standard on January 5th showed that the crisis had arrived. He improved after this date, and was dismissed quite recovered on January 29, 1866.

CASE 2.—E. C., æt. 9, admitted December 21, 1865. Ill about seven days; typhus rash distinctly visible. The attack was a severe one; there was a good deal of raving for some days, and the boy was deaf and stupid.

		Pulse.	Resp.	Temp.			Pulse.	Resp.	Temp.
Dec. 21.	E.	120	28	103 $\frac{1}{5}$	Dec. 29.	E.	114	28	100 $\frac{5}{5}$
22.	M.	132	34	102 $\frac{2}{5}$		M.	96	30	99 $\frac{1}{5}$
	E.	132	32	103		E.	96	24	99 $\frac{4}{5}$
23.	M.	132	32	102 $\frac{4}{5}$	30.	M.	96	24	98 $\frac{5}{5}$
	E.	132	36	103 $\frac{2}{5}$		E.	100	24	99
24.	M.	130	32	103 $\frac{2}{5}$	31.	M.	100	24	97 $\frac{3}{5}$
	E.	132	32	103		E.	96	20	98
25.	M.	130	32	102	1866.				
	E.	144	36	103 $\frac{1}{5}$	Jan. 1.	M.	80	20	96 $\frac{4}{5}$
26.	M.	120	34	102 $\frac{3}{5}$		E.	72	20	96 $\frac{2}{5}$
	E.	130	34	101 $\frac{2}{5}$		M.	72	24	97 $\frac{3}{5}$
27.	M.	120	28	103	11.	M.	72	24	97 $\frac{2}{5}$
	E.	108	36	101 $\frac{2}{5}$	24.	M.	72	24	97
28.	M.	108	32	101					

This patient made an excellent recovery, and was dismissed on January 29, 1866.

CASE 3.—W. M., æt. 10, admitted 17th January 1866. Ill five days; typhus rash well marked.

		Pulse.	Resp.	Temp.			Pulse.	Resp.	Temp.
Jan. 17.	E.	132	24	103 $\frac{2}{5}$	Jan. 25.	M.	120	36	100 $\frac{3}{5}$
18.	M.	120	36	103		E.	124	36	103
	E.	120	24	104	26.	M.	120	30	101 $\frac{3}{5}$
19.	M.	120	36	103		E.	108	30	101
	E.	132	36	103	27.	M.	96	30	99 $\frac{3}{5}$
20.	M.	124	36	102		E.	108	30	100 $\frac{2}{5}$
	E.	124	36	102 $\frac{4}{5}$	28.	M.	100	30	98 $\frac{4}{5}$
21.	M.	120	40	102 $\frac{2}{5}$		E.	90	30	99 $\frac{1}{5}$
	E.	120	34	102 $\frac{4}{5}$	29.	M.	84	30	96
22.	M.	120	36	101 $\frac{2}{5}$		E.	84	30	97 $\frac{3}{5}$
	E.	120	36	103 $\frac{3}{5}$	30.	M.	84	24	95 $\frac{3}{5}$
23.	M.	132	36	102		E.	60	24	97
	E.	132	36	102 $\frac{4}{5}$	31.	M.	72	24	97 $\frac{3}{5}$
24.	M.	124	36	102	Feb. 2.	M.	84	24	97 $\frac{1}{5}$
	E.	120	36	103 $\frac{3}{5}$					

In this case there was epistaxis, pain, and gurgling in the belly, and almost constant noisy delirium for three days. On the 28th January, the temperature fell to 98 $\frac{1}{2}$ °, and he became much calmer and better. On the 29th, he vomited everything he swallowed, and seemed much worse, but as there was no rise in the temperature we were assured that nothing serious was going to happen. He made a good recovery.

CASE 4.—J. M., æt. 6, sister of the above, admitted 20th December 1865; ill about six days; copious typhus rash on face, hands, and legs.

		Pulse.	Resp.	Temp.			Pulse.	Resp.	Temp.
Dec. 20.	E.	136	28	102 $\frac{1}{5}$	Dec. 23.	M.	120	32	101 $\frac{1}{5}$
21.	M.	144	30	101 $\frac{2}{5}$		E.	140	36	103 $\frac{4}{5}$
	E.	132	33	103	24.	M.	126	30	101 $\frac{3}{5}$
22.	M.	144	32	101		E.	140	34	103 $\frac{2}{5}$
	E.	144	30	100 $\frac{4}{5}$	25.	M.	120	40	100 $\frac{4}{5}$
						E.	120	36	101

		Pulse.	Resp.	Temp.			Pulse.	Resp.	Temp.
Dec. 26.	M.	120	30	99 $\frac{2}{5}$	Dec. 29.	M.	84	20	97 $\frac{2}{5}$
	E.	120	30	100 $\frac{2}{5}$			E.	84	24
27.	M.	96	34	98	31.	M.	96	24	6 $\frac{4}{5}$
	E.	96	32	99					
28.	M.	100	28	97	1866.				
	E.	96	28	98 $\frac{4}{5}$	Jan. 1.	M.	96	24	96 $\frac{3}{5}$
						4.	M.	96	96 $\frac{2}{5}$

It will be seen that in this case the temperature fell on the 14th day of the fever, viz. the 27th December, to 98°. On the same day patient had a long sleep, and when she awoke she was much better. She improved daily, and was dismissed recovered on 20th January 1866.

CASE 5.—J. M., æt. 4, admitted 15th January 1866. Ill five days. Typhus rash came out on 17th January.

		Pulse.	Resp.	Temp.			Pulse.	Resp.	Temp.
Jan. 15.	E.	132	46	103 $\frac{3}{5}$ °	Jan. 22.	M.	108	42	98 $\frac{1}{5}$
16.	M.	144	42	103 $\frac{4}{5}$		E.	108	36	99 $\frac{4}{5}$
	E.	138	36	102 $\frac{1}{5}$	23.	M.	120	36	99 $\frac{4}{5}$
17.	M.	144	42	102 $\frac{3}{5}$		E.	120	36	100 $\frac{3}{5}$
	E.	156	46	103	24.	M.	120	30	99
18.	M.	156	48	101		E.	108	36	100 $\frac{2}{5}$
	E.	144	48	102 $\frac{2}{5}$	25.	M.	120	36	99 $\frac{3}{5}$
19.	M.	144	48	101 $\frac{2}{5}$		E.	112	36	100
	E.	140	48	101 $\frac{3}{5}$	26.	E.	96	36	98 $\frac{3}{5}$
20.	M.	144	48	101 $\frac{4}{5}$	27.	E.	96	36	97 $\frac{2}{5}$
	E.	144	48	101 $\frac{3}{5}$	28.	E.	96	36	97 $\frac{1}{5}$
21.	M.	132	36	102 $\frac{2}{5}$	30.	E.	80	24	95
	E.	132	36	101 $\frac{1}{5}$					

Here the temperature fell to 98 $\frac{1}{5}$ ° on the 22d, but it afterwards rose, on several occasions, to 100°; indicating that the course of the convalescence was irregular. The child, however, made a good recovery.

CASE 6.—J. B., æt. 5, admitted 5th January 1866. Ill about seven days. Typhus rash on face and legs.

		Pulse.	Resp.	Temp.			Pulse.	Resp.	Temp.
Jan. 5.	E.	144	36	101°	Jan. 11.	M.	144	36	100°
6.	M.	144	48	100		E.	144	36	102
	E.	156	34	102 $\frac{2}{5}$	12.	M.	132	36	100
7.	M.	132	36	100		E.	144	36	99 $\frac{1}{5}$
	E.	144	42	102	13.	M.	132	36	98 $\frac{4}{5}$
8.	M.	132	42	101	14.	M.	124	28	97 $\frac{2}{5}$
	E.	152	40	103 $\frac{4}{5}$		E.	120	28	97 $\frac{4}{5}$
9.	M.	152	32	102	15.	M.	120	28	95 $\frac{2}{5}$
	E.	152	36	102 $\frac{3}{5}$	16.	E.	108	24	98 $\frac{1}{5}$
10.	M.	144	48	101 $\frac{4}{5}$	21.	E.	96	36	95 $\frac{2}{5}$
	E.	132	36	102	Feb. 2.	E.	84	24	95 $\frac{3}{5}$

On the evening of the 12th January, it will be seen that, although the pulse numbered 144 beats in the minute, the temperature was down to near the normal standard 99 $\frac{1}{5}$ °. This indicated the turn,

for the boy did well ever after, and was dismissed recovered on 5th February. It will be observed that even at night, on two occasions before he left the hospital, the temperature remained so low as $95\frac{3}{5}^{\circ}$.

CASE 7.—E. M., æt. 7, admitted 20th December 1865. Ill eight days. Typhus rash distinct on hands, forearms, and thighs.

			Pulse.	Resp.	Temp.				Pulse.	Resp.	Temp.	
Dec.	20.	E.	152	42	104 ⁰	Dec.	26.	M.	108	40	99 ⁰	
	21.	M.	156	41	102 ³ / ₅			E.	108	40	100	
		E.	156	42	103 ³ / ₅		27.	M.	108	34	99 ² / ₅	
	22.	M.	132	42	101 ² / ₅			E.	108	36	99 ⁴ / ₅	
		E.	144	42	103 ¹ / ₅		28.	M.	108	34	97 ¹ / ₅	
	23.	M.	144	44	102			E.	108	32	97 ³ / ₅	
		E.	156	52	102 ⁴ / ₅		31.	M.	92	32	97 ² / ₅	
	24.	M.	144	38	102 ² / ₅	1866.						
		E.	132	42	102 ² / ₅	Jan.	2.	M.	84	24	98 ¹ / ₅	
	25.	M.	108	40	101			9.	M.	96	24	95 ¹ / ₅
		E.	108	37	100							

Here, again, the temperature which up to the 26th December, the fourteenth day of the fever, had always been above 100° , fell on that day to 99° , and although the pulse kept up for a day or two longer, still the boy improved from that date. He left the hospital quite well on 15th January 1866.

CASE 8.—E. C., æt. $8\frac{1}{2}$, admitted 21st December 1865. Ill about eight days. Copious typhus eruption on face, hands, trunk, and legs.

		Pulse.	Resp.	Temp.			Pulse.	Resp.	Temp.		
Dec.	21.	E.	132	28	103 $\frac{3}{5}$	Dec.	26.	E.	96	36	98 $\frac{0}{5}$
	22.	M.	132	28	101 $\frac{4}{5}$		27.	M.	96	34	97 $\frac{2}{5}$
		E.	144	28	103 $\frac{3}{5}$			E.	84	36	96 $\frac{2}{5}$
	23.	M.	144	30	101		28.	M.	84	36	96 $\frac{4}{5}$
		E.	144	30	102 $\frac{4}{5}$			E.	84	32	96 $\frac{2}{5}$
	24.	M.	132	28	101 $\frac{2}{5}$		30.	M.	84	30	95 $\frac{4}{5}$
		E.	132	30	102 $\frac{4}{5}$	1866.					
	25.	M.	120	24	98 $\frac{3}{5}$	Jan.	2.	M.	72	28	96 $\frac{3}{5}$
		E.	120	36	101 $\frac{4}{5}$		3.	M.	72	28	95
	26.	M.	96	36	96 $\frac{4}{5}$		6.	M.	72	28	97

On the 26th of December, the fourteenth day of the fever, the temperature fell suddenly from $101\frac{4}{5}^{\circ}$, at which it stood on the previous evening to $96\frac{4}{5}^{\circ}$, no less than five degrees; thus indicating that the crisis had arrived. After this date the child improved every day, and went home quite well on 11th January 1866.

CASE 9.—M. B., æt. 8, admitted 26th December 1865. Had been ill about eight days. This was also a case of true typhus.

			Pulse.	Resp.	Temp.				Pulse.	Resp.	Temp.
Dec.	26.	E.	120	42	103 $\frac{3}{5}$	Dec.	28.	M.	108	40	101 $\frac{0}{5}$
	27.	M.	120	42	101 $\frac{2}{5}$			E.	100	24	102
		E.	108	30	103		29.	M.	108	36	100

		Pulse.	Resp.	Temp.			Pulse.	Resp.	Temp.
Dec. 29.	E.	96	36	100°	1866.				
30.	M.	96	30	97 $\frac{3}{5}$	Jan. 3.	M.	88	24	95 $\frac{3}{5}$
	E.	110	28	98 $\frac{2}{5}$	6.	M.	84	24	97 $\frac{2}{5}$
31.	M.	104	24	97 $\frac{4}{5}$	16.	M.	84	24	96
	E.	104	28	98	24.	M.	84	24	98

It is rather strange that in this case the fall of the pulse preceded that of the temperature; for while on the evening of the 29th December the pulse was 96°, the heat was 100°. The boy did well and was dismissed quite recovered on 29th January 1866.

CASE 10.—E. M., æt. 4, admitted 25th January 1866. Ill about a week. Typhus rash on face, hands, and body. Patient was very ill, restless, delirious, and very deaf for some days.

		Pulse.	Resp.	Temp.			Pulse.	Resp.	Temp.
Jan. 25.	E.	144	40	104°	Jan. 30.	M.	132	44	100 $\frac{2}{5}$
26.	M.	132	40	102 $\frac{2}{5}$		E.	132	44	100 $\frac{4}{5}$
	E.	144	40	102 $\frac{2}{5}$	31.	M.	108	40	100
27.	M.	144	42	102 $\frac{3}{5}$		E.	132	40	101 $\frac{1}{5}$
	E.	142	40	102 $\frac{3}{5}$	Feb. 1.	M.	108	40	99 $\frac{1}{5}$
28.	M.	144	44	102 $\frac{1}{5}$		E.	96	38	98 $\frac{4}{5}$
	E.	150	44	102 $\frac{3}{5}$	3.	M.	100	36	97 $\frac{3}{5}$
29.	M.	132	44	101 $\frac{3}{5}$	4.	M.	96	36	97
	E.	136	40	102 $\frac{3}{5}$					

This girl improved immediately after the temperature went down to 99 $\frac{1}{5}$, and made an excellent recovery.

These are all the cases of typhus fever on which thermometric observations have yet been concluded, and they may serve to show the general range of temperature of that disease in young persons.

Of late, typhoid fever has not been very prevalent amongst children; at all events, comparatively few cases have been admitted to this hospital. I give the temperature, etc., of one case which proved fatal about the twelfth day of the fever.

Temperature, Respiration, and Pulse, in a Fatal Case of Typhoid Fever.

M. S., æt. 4; admitted 16th January 1866. Ill six days.

		Pulse.	Resp.	Temp.			Pulse.	Resp.	Temp.
Jan. 16.	E.	132	36	104 $\frac{1}{5}$	Jan. 19.	E.	144	48	103 $\frac{3}{5}$
17.	M.	132	36	103 $\frac{1}{5}$	20.	M.	144	40	101 $\frac{1}{5}$
	E.	120	36	104		E.	144	46	103 $\frac{2}{5}$
18.	M.	132	40	102	21.	M.	168	46	104 $\frac{1}{5}$
	E.	132	40	103		5 P.M.	168	46	104 $\frac{2}{5}$
19.	M.	138	46	103 $\frac{2}{5}$					

Child died at 7 P.M. of the 21st.

It is of interest to observe that before death there was a considerable rise in the temperature. It would have been very difficult to have diagnosed this to be typhoid fever by the eruption alone, for it was very scanty, and by no means characteristic. But there were

the pea-soup evacuations, and a tympanitic distention of the belly ; and the post-mortem examination revealed changes in the mesenteric and Peyerian glands. There was likewise effusion into the chest.

The next case shows the range of temperature in a scarlet fever patient, in whom the eruption was copious, and the tonsils ulcerated. The rash first appeared on the 22d.

Case of Scarlatina, with Daily Records of the Temperature, Pulse, and Respiration.

			Pulse.	Resp.	Temp.				Pulse.	Resp.	Temp.
Jan.	22.	E.	150	36	104 ⁰	Jan.	28.	M.	92	30	97 ⁰
	23.	M.	144	36	103			E.	108	30	98 ² / ₅
		E.	144	36	103 ³ / ₅	29.	M.	96	30	97 ⁴ / ₅	
	24.	M.	144	36	102 ² / ₅			E.	90	30	98 ² / ₅
		E.	144	40	103 ¹ / ₅	30.	M.	96	36	97	
	25.	M.	120	36	100			E.	108	36	98 ⁴ / ₅
		E.	140	36	102	31.	M.	96	24	98 ¹ / ₅	
	26.	M.	120	36	99			E.	90	24	98
		E.	120	30	99 ¹ / ₅	Feb.	1.	M.	96	24	98 ² / ₅
	27.	M.	96	30	97 ³ / ₅		3.	M.	84	24	98 ⁴ / ₅
		E.	108	30	97 ² / ₅						

Desquamation began on the seventh day from the appearance of the eruption. There was no renal complication, and patient made a good recovery.

Of course, these cases, the particulars of which I have here recorded, are far too few to enable me to make any correct generalization as to the range of the temperature in the fevers alluded to. Nor do I intend to attempt to draw partial or premature conclusions ; but convinced that "the foundation of all knowledge must be a careful and extensive acquisition of facts," I offer the results of my observations as a contribution to the thermometry of disease in children.

It is of great interest to remark that in *all the ten typhus cases the temperature fell considerably below the normal standard during convalescence* : and in seven of them it was so low as to be under 96° Fahr.

The greatest heat observed was in Case 1, where on two occasions the thermometer stood at 104⁴/₅°.

In the typhoid case the greatest heat was 104³/₅°, while in the scarlatina patient the maximum was 104°.

The lowest temperature occurred in the typhus cases 1, 5, and 8, during convalescence, when the heat of the body was only 95°.

There is just one other point I would beg to direct attention to, as it is of importance in connexion with the diagnosis of typhus from typhoid fever. In Dr Aitken's recent work he thus alludes to the different modes of defervescence in the two diseases : "The difference between the two (typhus and typhoid) is rendered still more

striking by their respective modes of defervescence. In typhus the defervescence shows no gradual remissions, on the contrary it is invariably sudden." And in his typical ranges of temperature, he represents the fall in typhus to be as much as five degrees, when the crisis occurs.

Now, in only one of my cases (No. 8) was the defervescence either so sudden or so great as this. In it the temperature, which, on the evening of December 25, was $101\frac{4}{5}$, fell on the following morning to $96\frac{4}{5}$,—a fall of five degrees. But in most of the other cases the decline of the temperature to the normal standard or under it usually took place gradually, the different steps downwards scarcely ever measuring more than one or two degrees.

ARTICLE VI.—*Observations on the Arrested Twin Development of Jean Battista dos Santos, born at Faro in Portugal, in 1846.* By P. D. HANDYSIDE, M.D., F.R.S.E., Lecturer on Anatomy at the Royal College of Surgeons. *Illustrated by Woodcuts.*

THE profession is indebted to the Editor of "The Lancet," and to Mr Ernest Hart, of St Mary's Hospital, London, for a very interesting report of an examination of this remarkable double monstrosity in an adult.¹ Attention having been thus directed to this perhaps unique variety of abnormality by excess, it may not be unimportant to record some anatomical peculiarities presented by it, yet hitherto unnoticed, so far as I am aware; and farther, I shall venture to express a difference in opinion relative to other features, on which the able author of that report has made his observations.

When the subject of these remarks presented himself, on the 18th of December last, at the Royal College of Surgeons, Edinburgh, with his prominent and broadly cicatrized *umbilicus*, double *penis* and *scrotum*, and apparently double lower extremities, the idea suggested itself to me that from the umbilicus, possibly from the supra-sternal fossa, downwards, the existence of a twin production might be elicited.

I. *Abdominal Region*.—In seeking to trace development, as alone it can be done during life, the eye rests, *first*, on the broad base of depressed integument caused by the anormal umbilical *raphé*. This wide mesial groove extends along the depressed *linea alba*, from the sunken ensiform cartilage, as low as the *pubes*; it is traceable, between the well-developed *recti* muscles, as a groove of above a quarter of an inch in depth, almost four inches in breadth; and the edges of it begin, on a horizontal line with the lower edge of the umbilicus, to converge towards the pubic spines. The *umbilicus*

¹ That narrative, with four illustrative woodcuts, is published in the No. of that Journal for July 29, 1865.

is situated fully an inch nearer the pubes than is usual in the male sex; and it projects from its base, at the depressed *linea alba*, for



half an inch forwards, so as to bulge a quarter of an inch in advance of the abdominal wall. Its base is two inches in diameter; while its sides and summit are irregularly nodulated, by means of two symmetrical depressions on its sides superiorly, and a horizontal groove on its lower half; and, withal, it is covered with a glistening stretched cicatrice, yet without any indication of umbilical hernia. This condition, then, of the parts, suggests the idea of the attachment to its summit, originally, of a double umbilical cord; and the photographic representation, annexed to these remarks, may be observed to present the special features that I have now described. Upon coughing, no farther deficiency, nor any relaxation, are apparent in the abdominal or inguinal regions; neither, under this impulse, can any displacement, or vibration of bowel, be perceived on any part of their parietes, nor can more than two testicles be felt; and farther, on digging the fingers well into the hard and resisting *linea alba* in the hypogastric region during expiration, there cannot be discovered any abortive osteal element indicative

of the lodgement, in the supra-pubic depression, of traces of the existence of a second pelvis.

II. *Chest, Neck, and Head.*—Proceeding to trace that mesial groove along the *sternum* to the neck, its depth is here fully one inch, its breadth nearly two inches, and its sides are rounded convexly forwards owing to the prominence of the cartilages of the true ribs. The *scrobiculus cordis* below, and the *jugular fossa* above, are thus both absent; but there is no deficiency in the vertical conjunction of the osseous elements of the *sternum*. No abnormality is apparent on other parts of the neck, nor on the head or face. The thoracic, like the abdominal viscera, in relative position and size, appear normal.

III. *Spine.*—On examination of the head and trunk being made from behind, no deviation from the ordinary structure is observable. The mesial space between the *os coccygis* and the *anus* is depressed, deeply cleft, and somewhat tense; in a manner resembling the longitudinal mesial groove on the fore-part of the trunk.

IV. *Anus, Bladder, Urethra, etc.*—The orifice of the bowel is situated unusually far forward, being about three inches anterior to the *os coccygis*, and immediately at the posterior edge of the point of attachment of the middle lower extremity. On introducing the forefinger as far as possible, and exploring slowly the parietes of the *rectum*, nothing unusual presents itself; and no information can thence be obtained as to the special arrangement of the parts contained within the *pelvis*. Refusal being still given to the proposal to sound the bladder *per urethram*, we are shut out from another source of information in regard to the anatomy of the *pelvis*. We have reason to conclude, however, from the answers given to inquiries put to Dos Santos, that he possesses a double bladder, the two communicating probably through an imperfect *septum*; that he has separate urethral muscles to each canal; and that he is furnished with only a single *levator ani*, and single vesical, vesicular, and prostatic plexuses of nerves and bloodvessels.

V. *Scrotum.*—The *scrotum* on each side is large, and its tissues seem natural. It is subdivided, by the usual *septum*, into two very unequal compartments; the external one on each side being large, and lodging a very large testicle of the usual form; while the internal, or mesial compartment of each *scrotum*, is small in size, and corrugated, though it is a quite distinct, pendulous, and flaccid bag or pouch. These latter are, respectively, of one inch and a quarter, and one inch and a half in length; and are stated to have lodged, in early life, two additional testicles, that, in boyhood, retired within the abdomen.

VI. *Penes.*—From the upper and fore-part of each *scrotum* hangs a *penis*, of large size, and of normal form. Each *penis* has, on its own side, precisely the same relations to the pelvic wall; the right *crus* of the right *penis* and the left *crus* of the left *penis* having, each, its usual origins, by *crura*, to the ascending *ramus* of *ischium* and descending *ramus* of *pubes*; the inner half of each *penis* be-

longing to the undeveloped twin. The mesial, or adjacent, *crura* of the two *penes* are equally well marked, though the incorporation and fusion together of the two mesial or intermediate *ossa pubis*, with their subjacent rami (including probably the adjacent *ossa ischii* and adjacent *acetabula* of the twin *pelvis*), makes the tracing of them a little difficult. It seems, nevertheless, pretty clear that the origin of these intermediate *crura* is derived from a round thick fibro-cartilaginous process, which is found depending from the fore-part of the expanded pubic arch of the pelvis.

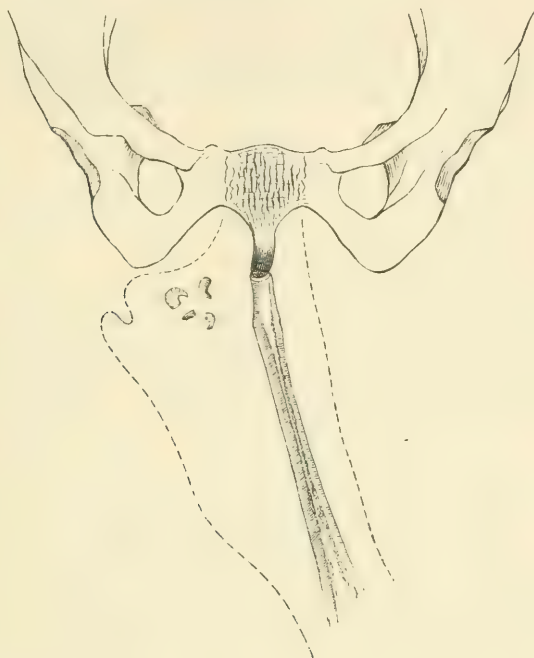
VII. *Interpubic descending process, representing the missing halves of a twin pelvis.*—This remarkable cartilaginous process presents to the touch a scabrous surface; it is about one inch in diameter, and three inches in length; and it is slightly incurvated backwards, ending inferiorly in a true arthrodial articular surface for the upper end of the united *ossa femorum*. The pelvic attachment of this fibro-cartilaginous process appears to be one of an amphiarthrodial nature, in connexion probably with the existence of a broader block than usual of the dense interpubic fibro-cartilaginous plates; and, in fact, to be an extension of these layers downwards, resembling the prolongation in tough cartilage of the extremity of a rib. The width of the pelvis, measured between the anterior superior iliac spines, and the very obscure yielding to torsion in either direction of this descending fibro-cartilage, appear to favour the view now advanced of the derivation and representative character of this remarkable structure.

Mr Hart's view of the "osteal congener of the true *penis*" being represented by the descending pelvic process just described, creates a necessity for the expression of my dissent from it, on the ground, that as there exists nowhere in Dos Santos any appreciable trace of a *third* foetal development, no more can we, in my opinion, be justified in concluding that in this dense process—forming, as it does, an integrant part of *his skeleton*—is to be recognised the rudiment of a third male organ.

This mesial projection, then, may be held to represent—situated, as it exactly is, in the place of the absent halves of the twin pelvis—the coalesced *ossa pubis*, including their proper ascending *rami* of *ischia* and *alæ* of *ilium*. In other words, this process may be regarded as the congener, and combination perhaps, at once of a breadthened and elongated cartilaginous *symphysis*, the missing halves of the subpubic and triangular ligaments, and the flattened and prolonged surfaces of the *acetabula*, without, however, any apparent trace of the *teres* ligaments;—all these fused together to fulfil the double office the *ischium* so largely performs, that of affording a firm attachment to what in this case are the mesial *crura penis*, and the rudimentary two extremities to be presently described.¹

¹ The annexed scheme or diagram may serve to illustrate this view:—A dotted outline represents the contour of the third limb; with a deep cleft, representing a rudimentary second *anus*, at its angular upper end.

VIII. *Perinæum*.—The *perinæum* proper is absent; the entire space between the anus and the posterior edge of the scrotum being occupied by the pendulous supernumerary limb.



IX. *Middle Lower Extremity*.—The superficial area of skin between the anal aperture and the two *scrota*, corresponding to the offset of this third limb, amounts to two and a-half inches in mesial length, and the space occupied transversely by its attachment is nearly two inches. It hangs loosely; is much bent at the knee; while the posterior surface of the thigh, and the entire leg and foot project forwards between Dos Santos' true limbs in such an inclination that the leg and foot rest in front of the inner surface of the lower two-thirds of his left thigh. This position of the abnormal limb—the most convenient one for Dos Santos in a sitting or lying posture—was probably impressed upon it *in utero*, and during infancy was apparently maintained as he lay on his back or side; while, subsequent to the age of progression, to avoid its unseemly protrusion, as well as the inconvenience of its loose dangling attachment above, he has been in the habit of wearing it slung or braced up firmly along his *right* thigh during the time he preserves the erect attitude.

The entire membral appendage is capable, without pain or strain, of very free rotation at its arthrodial attachment; sufficient indeed to permit of its being turned to either side about two-thirds upon

its axis.¹ Under such free manipulation—more readily effected behind the person of Dos Santos whilst he stands erect—and with the forefinger inserted fully into the *rectum* for exploration, I satisfied myself that it is through the medium of the free arthro-dial articulation, already referred to, that the middle extremity is suspended; and, moreover, that this articulation is a *congenital* formation, resembling, in structure and extent of mobility, the joint that exists between the lesser head of the *os humeri* and the shallow circular cup on the *radius*.

Projecting backwards from the posterior surface of the upper third of this middle limb, there is an obtuse enlargement of about four or five inches in elevation, directed upwards towards the cleft between the buttocks, and the skin covering the summit of which is remarkably depressed by an irregular cicatrice. On freely handling, and as it were kneading, the entire enlargement, it is found to lodge in the interspace between the cicatrice and the upper end of the *ossa femorum*, a congeries of about four separate and irregularly-shaped osteal elements, which may be taken to represent (as suggested to me by Dr Sanders) “the sacral bones” of the undeveloped twin—the elements probably of a second *os sacrum*. The value to the anatomist of the cicatrice alluded to may be regarded as great in determining the proper relation of parts, and important in the construction of any hypothesis on the uterine history of this double monstrosity. Consequently, in the absence of farther means of information than are at present available, it appears to me that in this irregular cicatrice we may recognise, as probably correct, the opinion which Mr Hart has advanced—in connexion with “the true anal aperture” being “deviated by about half-an-inch from the median line;”—namely, that “the puckered cul-de-sac in the skin over the clubbed and enlarged upper posterior extremity of the thigh is suggestive of an abortive second anus.”

The *ossa femorum* I regard as plainly double, and fused together longitudinally. Their lower ends, laterally, present the well-marked contour and osseous markings of the normal flat external condyles. The *patellæ* are absent and wholly unrepresented. From the two *malleoli* bearing the elongated character of the lower ends of the *fibulæ*, I am satisfied that both of these bones, enclosing the *tibie* of both lower extremities, are well represented; and Mr Hart's second and third figures show admirably what this able surgeon so well describes as “really a coalescence of two feet, more or less perfect.” No evidence, however, nor indication, of fracture, dislocation, false joint, torsion forwards, or other injury, are, in my opinion, capable of being detected along any part of the limb.

In testing the accuracy of the explanation that I have ventured to give,—that the contracted form and irregular disposition of the middle extremity have been imparted to it so as best to conduce

¹ The second and third figures in Mr Hart's paper express in part this extreme degree of mobility.

to the comfort of the individual, an explanation which coincides also with the pathological state of the parts,—I would again recommend that an examination of these parts should be undertaken from a position *behind* the erect person of Dos Santos. Accordingly, the contracted anormal limb should be rotated, at the subpubic arthrodial joint, so as to bring into view the back of the thigh upon the same vertical plane and parallel to the back of the two normal thighs. On this being done, we shall find little difficulty in immediately recognising the presence of contracted hamstrings, with contracted double tendon of Achilles, and double sole of foot; parts which subtend the back of the abnormal thigh. We shall then see, in Dos Santos' additional limb, a marked instance of *congenital contraction of the hamstrings, combined with an extreme case of congenital Talipes equinus*.

In viewing these features, it may be borne in mind that in all extreme cases of talipes equinus, owing to the shortened sural muscles and tendons,¹ the sole of the foot is so much extended at the ankle-joint that its plantar aspect is set at an obtuse angle with the calf of the leg. When, in addition to this, the flexor tendons of the knee-joint also are contracted, so that the region of the calf is set at an acute angle with the back of the thigh, then the lines of these two angles together seem to complete nearly three sides of a rhomboid, as on the margin; in which we find the sole of the foot running up parallel to the back of the thigh, and the dorsum of the retracted foot presenting its surface backwards,—that is, on the same vertical plane with the spinal column. Accordingly, on a close examination of Dos Santos, made while standing behind his person (as already suggested) a very acute angle—of 40° —may be noticed at his contracted knee-joint; an obtuse angle—of 158° —at his over-extended ankle-joint, as measured immediately over the insertion of the two united *tendines Achillis*; while his two contracted plantar *fasciæ*, united edge to edge, form, between the four well-marked eminences of the balls of the heel and great toes, one deep longitudinal groove or recess.

Apart from the remarks already made, presumptive evidence in favour of the *congenital* character of these contractions in Dos Santos' third limb is afforded, in the absence of every trace of the *patellæ*; bones which are not deposited earlier than the third year of life.

X. *His general characters*.—It may be added, that Dos Santos' is an unusually interesting case of abnormal development, in so far as none of his functions, physical or mental, appear to be at all impaired; but are, if anything, rather above par. For his time of life, even with his nationality, he manifests a strong masculine

¹ See a woodcut at page 11 of Dr Little's work on Club-foot, representing a preparation in the museum of the London Hospital, which exhibits the bones of a *congenital talipes equinus* in a young man. Here the plantar region of the entire foot forms an obtuse angle with the calf of the leg.

development; and this is more especially visible not only in the structure of his skin, hair, muscular conformation—which is both robust and athletic,—breadth of shoulders, and strong marking of the bones; but likewise in his general manner, tone of voice, expression of countenance, mode of address, and perfect self-command.

GENERAL REMARKS.

(a.) There seems reason to conclude that the foregoing case is a *twin, not a triple development*; the teratological law on this point, derived now from a very large induction, defining very clearly the very decided characters and extreme rarity of the latter form of monstrosity. Besides this, the well-marked and prolonged second perineal raphé, and in it the existence—as an exaggerated lacuna—of the rudimentary second *anus*, affords evidence that such a cul-de-sac could not have been the point of attachment of a third umbilical cord. Moreover, the two symmetrical and well-defined depressions upon the *umbilicus* proper, seem to point to the true seat of a probably double *funis*. And, farther, the all but invariable rule in regard to double monsters—that the union of dissimilar parts is very rare, and that like parts of two unequal bodies, the autosite and the parasite, are always attached to one another¹—seems to make this case, as revealing such characteristics, speak for itself.

(b.) The *sternum* is abnormally broad, and deeply grooved, though not cleft; nevertheless its condition, as described, testifies to its being the compound of the lateral halves which in the normal state are fused into one bone. The apparently absent or much expanded *ensiform* portion denotes the same tendency to a separation in this region, at the mesial line. That this cleavage existed probably at an early period, the broad *linea alba*, cicatrice-like, appears likewise to affirm.²

(c.) There can be no doubt that the *two lower limbs* of the parasite, or secondary blighted foetus, are, in *Dos Santos' case*, *fused into one*. Regnault's case of a somewhat analogous development³ presents a twin formation, in which the two adjacent upper extremities coalesce, excepting only at the digits; and these are nine in number, the thumbs (like the great toes in *Dos Santos' case*) being united into one. J. F. Meckel⁴ also, describes an intermediate third upper extremity (evidently coalesced) ascending in a straight line. Again, St Hilaire⁵ represents one “*symèle*,” in whom two

¹ See, for example, Plates 5, 14, 15, 16, 19, 20, of Isidore G. St Hilaire's *Hist. Gen. et Part. des Anomalies*, etc., tom. iii., which exhibit the congenital attachment of corresponding parts.

² In I. St Hilaire's *lib. cit.*, Pl. 14 (fig. 3), 16 (figs. 2 and 3), and 18 (figs. 2, 4, 5, 6), all these seven figures represent double monstrosity occurring at a cleft sternum,—the first six of these in man.

³ St Hilaire, *lib. cit.*, Pl. 14 (fig. 1.)

⁴ De *Duplicitate Monstrosa* Commentarius, Halae, 1815, p. 79.

⁵ *Lib. cit.*, Pl. 5 (fig. 1.)

lower limbs are fused together, ending in a row of eleven digits; another, presenting the bones of the legs and feet coalesced,¹ and terminating in eight digits, inverted, the little toes being unrepresented; and (at fig. 3), an interesting view, on dissection, of the disposition of the vascular trunks and the distribution of the nerves in this last case.

(d.) The only approximate type to this twin malformation—as regards a *third lower extremity consisting of two limbs fused into one, and having one sole of the foot with ten distinct digits*, and presenting some points of analogy as concerns the *position of the second anus and the umbilicus respectively*—which I find recorded, is one by St Hilaire.² Consideration of this case tends, I think, to support the views already expressed in the preceding narrative. But as regards Dos Santos' *sexual malformation*, we find no similar case of it, if we except the somewhat problematical histories cited and referred to by Meckel.³ Six old writers, this author quotes, relative to so-called *double penes*; but I deem it relevant here to make one extract only:—"Illuc forsan pleraque exempla pertinent, quæ de pene vario modo duplici narrantur, præsertim ubi bini penes juxta se positi suis dicuntur. Hujus generis sunt, quæ citantur a Sini-baldo,⁴ Hannæo,⁵ aliisque minus integræ fidei medicis, quamvis per utrumque penem ad lubitum mejisse dicantur, in quibus hæc partium genitalium deformatio deprehendebatur et magnitudine solita uterque penis fuerit gavisus. Dissectio saltem talis difformitatis facta est nulla." The other four descriptions cited by Meckel, indicate merely I think varieties of *Epispadias* and *Hypospadias*.⁶

(e.) This case affords the nearest approximation to an instance of the "genre notomèle" of St Hilaire's "Monstres Doubles." In that malformation *an accessory limb is inserted in the region of the spine*,⁷ and, as is stated by him, "est encore une monstrosité inconnue chez l'homme, et très rare chez les animaux." That Dos Santos' case supplies this desideratum in the human species, I do not mean to assert, unless it be assumed that the four osteal elements in his case are the elements of a *second sacrum and spine*.

(f.) A *somewhat similar parasitic mesial bone* to that of the middle lower extremity here described—being of a *tibia-like* form, covered with periosteum, and surrounded by a number of vessels and nerves—was found by Professor Sir James Simpson⁸ within a tumour removed by Dr Richardson, of Stockton-on-Tees, "from over the middle of the *sacrum* of a child, without showing any bad symptoms afterwards."

(g.) The record of numerous instances of *contracted knees and*

¹ *Lib. cit.*, Pl. 5 (figs 2, 3.) ² *Ibid.*, Pl. 20 (fig. 1.) ³ *Lib. cit.*, p. 50.

⁴ *Geneanthropia*, lib. ii. c. 3. *Ni autem totus fallor, vesica hic adfuit inversa.* ⁵ *Obs. anat. rar. cent. iv. hist. 22.*

⁶ J. F. Meckel, *Handbuch der Path. Anat.* Leipsig, 1812, i. 650-657.

⁷ *Tom. iii. p. 270.*

⁸ See his Lecture in the *Medical Times and Gazette* for July 2, 1859, pp. 5, 6.

clubbed-foot monsters tells in favour of the view I have ventured to impress,—namely, that there lacks evidence to show that any change has come over the form and position of the third limb of Dos Santos, since his birth.¹

(h.) Dos Santos' *anormal limb exhibits little vascularity, sensibility, or voluntary movement*; it is surrounded at its neck of attachment to the perinæum by flaccid skin, loose fat, and connective tissue merely; nevertheless the probability of there existing an important communication with the spinal canal, and the fact that *its sub-public connexions* are significant, forbid the notion that this redundant middle limb might be safely removed.

Part Second.

REVIEWS.

An Introduction to the Study of Medicine; to which is appended A Report on the Homœopathic Treatment of Acute Diseases in Dr Fleischmann's Hospital, Vienna, during the Months of May, June, and July 1846. By GEORGE WILLIAM BALFOUR, M.D., F.R.C.P., Lecturer upon Practice of Physic, etc. Edinburgh, 8vo, pp. 307: 1865.

THE name of Dr George Balfour is familiar to our readers, in connexion with many interesting papers communicated to this Journal; while his accurate and carefully edited translation, for the New Sydenham Society, of the late Professor Casper's great work on Forensic Medicine has introduced Dr Balfour to another and not less numerous circle of the profession. The present contribution exhibits the same qualities as we have previously had occasion to observe and admire in Dr Balfour's writings—the same thoughtfulness, the same independence of judgment, and a like degree of painstaking research. The book is pleasingly written, and from first to last is eminently readable. It consists of seven chapters and an appendix, in which homœopathy, as indeed the general title indicates, bulks largely. To the statements contained in this appendix the author evidently attaches very considerable value, for, originally embodied in the form of a report to the late Sir John Forbes, then editor of the *British and Foreign Medical Review*, in the pages of which it appeared, nearly twenty years ago,—he has deemed it worthy of reproduction, and believes it to be “a record of permanent interest.” We are not anxious to diminish the com-

¹ St Hilaire, *op. cit.*, Pl. 12 (figs. 1, 2, and 3.)

mentation—and if we were so minded, would be little sanguine of success—which our author has thus passed on this his own earliest performance, executed “while,” to use his own words, “engaged in perfecting myself in the use of the stethoscope under the tuition of Dr Joseph Skoda;” but we are constrained to acknowledge that in so far as permanent interest or value is concerned, we take a different view of this report from its author, and have no hesitation in affirming that the excellence of his “Introduction to the Study of Medicine,” so far from being enhanced, is really impaired by the appendix. A youth fresh from the studies of the schools is scarcely the person to select for the delicate and difficult task of observing and ultimately reporting on the different means employed by different physicians in a foreign school, for the treatment of acute diseases. That Dr George Balfour was probably as well qualified for this severe duty as any other youthful physician of his own time, we are quite ready to admit; and furthermore, that he prepared and submitted to Dr Forbes an interesting report, which, with Dr Forbes’ own comments, was entitled to a place in the British Medical Review for October 1846, we will not deny; but we do affirm that as documents of scientific value Dr George Balfour’s Report of 1846, and his Appendix to the “Introduction to the Study of Medicine” in 1865, are not entitled to take any rank whatever.

Let our readers judge for themselves. There is scarcely one among them who has not seen many cases of severe, and at least a few cases of uncomplicated fatal pneumonia, and who therefore may not be prepared, as we confess ourselves compelled to do, to call in question the accuracy of the following statement. Dr Balfour is speaking of his earliest observations in the wards of Skoda, and is addressing himself to the question of the necessity or non-necessity for the employment of active treatment in pneumonia. He observes, page 3, “In particular, I found him (Skoda) treating acute inflammatory diseases, such as pneumonia, pleurisy, etc., without that leeching or general bloodletting which I had been taught to consider not as merely useful but as absolutely necessary for the recovery of the patient. This doctrine had been so thoroughly impressed upon my mind that I shall never forget my horror when I found that it was habitually and entirely ignored by Skoda, whose patients, in the clinical wards, at least, were doomed to sink or swim by natural efforts in the cause of science; nor can I ever forget *that patient* whose case first opened my eyes to the possibility of there being some truth in Skoda’s axiomatic defence of his practice, that pneumonia was a disease which tended not to dissolution but to resolution, and therefore did not require any active treatment.” The case of “that patient” is the subject of a foot-note, page 4, and to this note, which we reproduce, we particularly crave attention. “The patient alluded to ultimately recovered. Although at one time Skoda’s assistant gave him up, Skoda himself never

did so, *though only a small portion of the top of the left lung remained permeable to air*, and the case was also complicated with pleurisy. He suffered very much while the strength of the disease was on him, but when that was over and broken he recovered amazingly quickly. He was a young plethoric man, and I have never seen a more serious case of pneumonia either as to severity or extent. *The whole of the right lung and almost all the left lung were hepatized.*" Now, that the case thus recorded by Dr Balfour was a very severe instance of pneumonia we do not doubt, that it yielded to the expectant treatment employed by Skoda, and would, in all probability, have sunk under any form of antiphlogistic treatment, which assuredly, in such a case would not have been had recourse to by Alison or any observant physician of Alison's time, we are also ready to acknowledge; but we take leave to call in question the accuracy of the observation of the young physician—at the time *perfecting himself* in the use of the stethoscope,—for we say, advisedly, pneumonia does not wait, and never did wait to kill, till "the whole of the *right* lung and almost all the left lung were hepatized." We feel constrained to ask what is the use of the lungs? if an acute disease can destroy the "*whole*" of one, and that the larger lung, and "*almost all*" the other, and still life be maintained! For this clinical picture Dr Balfour has certainly drawn on his own fancy. No doubt the circumstances in which the observation was made were peculiar and even embarrassing; he had recently arrived in Vienna; his mission was an important, even a proud one, for so young a man; introduced to the practice of the already celebrated Skoda, he found that physician employing a plan of treatment very different,—although, in this respect, it appears to us that Dr Balfour has largely exaggerated the difference,—from that pursued in the home hospitals he had recently left, and so sitting down he indites a letter, under date Vienna, 4th November 1845, giving a glowing description of Skoda's practice. That description Dr Balfour has reproduced in 1865. How many of the cases observed in Dr Fleischmann's hospital, during a later period of Dr Balfour's visit to Vienna, and which form the basis of his report to Dr Forbes, were noted with as little of a scrupulous adherence to perfect accuracy we do not know, and have, indeed, no certain means of informing ourselves. We therefore conclude that the appendix to the "Introduction to the Study," though, by no means, the least learned or elaborate portion of the volume now before us, does not contain materials for enabling us with any confidence to draw a comparison between the different plans of treatment which were witnessed by its author. No doubt the late Dr Forbes thought differently. With his favourable opinion, at all times entitled to respect, Dr Balfour will probably feel quite satisfied, and be thus enabled to regard with complacency the adverse view which, reluctantly, we have felt ourselves compelled to express. We turn,

now, to offer a very brief notice of the subjects discussed in the various *chapters* of the "Introduction."

The *first* chapter is *introductory*, and its burden is the treatment of acute disease by bloodletting, historically and critically considered. This chapter is certainly the most elaborate in the book, and those of our readers not already familiar with Dr Balfour's views, from a perusal of his paper entitled "Hematophobia," will find it both interesting and instructive. *Chapter second* treats of "*Medicine as an Art*," in which the true and proper relation of the sick to the physician, as well as the function of the physician, are clearly and ably sketched. From observations made in this chapter, as well as hints which are given in other portions of his work, we are led to conclude that Dr Balfour reposes a great degree of faith in the special therapeutic action of certain remedies. Few will differ from him in respect to the operation of quinine in intermittent fever; but we apprehend that his view of the specific or antidotal effect of iron in erysipelas, and still more of actæa in rheumatism, will for the present raise a host of objectors.

Chapter third is occupied with the mysteries of *Life and Death*. The speculations of Dr Balfour in it are, to say the least, ingenious; and the illustrations afforded of his particular views full of interest as well as instruction. He has arrived at the opinion that plants are not destitute of a voluntary principle, and the consideration of this topic leads him to the important generalization, that in Nature there are but two kingdoms—the organic and the inorganic.

In *chapter fourth*, the subject of *Health and Disease* is discussed in such a manner as to satisfy us of the practical skill, as well as the extensive information, of Dr Balfour. He is particular in directing attention to the advances which have of late years been made in the effectual treatment of many maladies, by the important discoveries in the nervous system which have rewarded the labours of not a few physiologists; and, perhaps, there is no more interesting portion of Dr Balfour's work than that which treats of the various forms of disease which have not inappropriately been styled *reflex* in their nature or essence.

From *chapter fifth*, on *Inflammation*, we learn nothing new, unless it be that in a case of acute disease of the chest, "*cut short*," described at page 220, Dr Balfour found, that "over the dull portion the respiration was puerile vesicular!" If this case was one of pleurisy, which—from what follows the passage quoted—we conclude Dr Balfour to have considered it, we submit that when dulness on percussion exists in pleurisy there must needs be deficiency or suppression of the vesicular murmur; and if, on the other hand, the case was one of pneumonia, occupation of the air vesicles in the inflamed part must necessarily have prevented puerile vesicular murmur being heard *over the dull portion*; it may, however, have been audible in its vicinity. The girl who was the subject of

this case "was," we are informed, "very well pleased with her treatment," and contrasted it favourably for her then medical attendant, Dr Balfour, with that pursued on a former occasion, fourteen years previously, when, suffering from a "*precisely similar attack*," she was bled freely from the arm, besides having other remedies, and only recovered after a fortnight's tedious illness. For ourselves, we must say that the materials for comparing the two attacks which this girl suffered are not supplied. On the contrary, the diagnosis of the latter attack is rendered very uncertain by Dr Balfour's own statement; accordingly, we decline committing ourselves to the condemnation of her former physician's practice, while we have no hesitation in approving the simple remedies employed by her latest attendant.

We have heard of *Gavarret*, but who is *Gavasset* quoted by Dr Balfour at page 189? Some other mistakes in proper names are calculated to offend the reader not only of this, but of other chapters: none more so than *Abercromby* for *Abercrombie*.

Two very short chapters, *sixth* and *seventh* respectively, embrace the subjects of *fever* and *chronic diseases*, and to these Dr Balfour has confessedly left too little space to do anything like justice. We are startled, however, by an announcement at page 226, so altogether erroneous as to make us wonder that a physician living and practising in Edinburgh could have committed himself to so serious a misstatement, "there is hardly anything but enteric fever to be seen now-a-days in Edinburgh." Let us recommend Dr Balfour to pay a visit to the fever wards of the Infirmary, where he will find that, as has been the case for many years, typhus still continues to be a familiar tenant; but both there, as well as in the well-known habitats of the old town, and also in the new town—to a greater extent than usual within the last few years,—typhus has this winter prevailed. Dr Balfour's argument concerning the non-pythogenic character of enteric fever, founded on the great prevalence of the disease, and the diminution of "pythogenic material to be found about the wynds and closes" of Edinburgh, thus receives its quietus, for the premises on which he builds his argument are as manifestly as they are absurdly erroneous.

In taking leave of Dr Balfour, from whom we have seen reason to differ on several topics, we are glad again to express our opinion of the value and general excellence of his book. We trust that Dr Balfour will accept our criticisms in the same friendly spirit as that in which they are offered. Our attention has been strictly limited to what is Dr Balfour's, in noticing Dr Balfour's work, preferring this mode, not always an easy one, to dealing with the author's *too* frequent quotation of the opinions of others, a plan by which, no doubt, his book is made to assume a more learned aspect, but it is a learning which confuses rather than interests the reader.

On Flooding after Delivery, and its Scientific Treatment, with a special Chapter on the Preventive Treatment. By LUMLEY EARLE, M.D., Obstetric Surgeon to the Queen's Hospital, and Medical Officer to the Hospital for Sick Children, Birmingham; Member of Council of the Obstetrical Society of London, etc. etc. London: Robert Hardwicke: 1865.

AT first sight a new book on Post-partum Hæmorrhage seems uncalled for and useless, as it is generally supposed that all that can be said upon the subject has been already said, and can be found in any work on Midwifery. It is true that the subject is, as a matter of course, always treated of more or less fully in obstetric lectures and books; but the fault we have usually to find in the ordinary handling of the subject is, that, for fear of inculcating bold measures on the student, the other extreme is taken, and the treatment made too much to consist of a graduated routine in which mild measures take the initiative, and the remedy which in many severe cases ought to be employed *first*, and as a substitute for which every other remedy is useless, and worse than useless, because wasting time, is generally placed last on the list. Boerhaave, in the preface to his Aphorisms, quoted by Dr Watson, says he knows of nothing which can be fitly termed a remedy, "*quin solo tempestivo usu tale fiat.*" Treatment which is quite proper in some cases is utterly useless in others. Again, in systematic works on midwifery, and often in lectures, many little practical points are omitted, because of their seeming littleness, which the student, on commencing practice, has either to pick up from some observant friend, or to find out for himself by experience. Similar ideas seem to have impressed Dr Earle in publishing his treatise, and we congratulate him on the success of the undertaking. The whole subject is laid out by him with admirable clearness and precision, and in an eminently practical manner, each condition of flooding having its own diagnosis and treatment. After going over, in the first chapter, the general signs and symptoms of flooding, Dr Earle, in the next chapter, describes the "*Preventive Treatment.*" This is a most useful chapter, and if the precautionary measures there inculcated were always adopted, many fewer cases of post-partum hæmorrhage would occur. The difference in the average number of such cases in the hands of different practitioners can be accounted for in no other way. In the next chapter, Dr Earle gives a good account of the various remedies which are employed in different cases. Then follow the causes of flooding, with the diagnosis and consequent treatment of each. These our author divides into,—I. The more common causes occurring before the delivery of the placenta or membranes; II. Those occurring after the birth of the placenta; and, III. The rarer causes of flooding. In the first class he includes (1.) Partial separation

of the *not* morbidly adherent placenta; (2.) Partial separation of the morbidly adherent placenta; (3.) Irregular contraction—hour-glass contraction; (4.) Inversion of the uterus; (5.) Disruption of the placenta; (6.) Retention of the membranes. In the second class we have (1.) Inertia of the uterus; (2.) Distention of the bladder; (3.) Clots in the uterus; (4.) Clots in the vagina; (5.) Cough—generally sympathetic; (6.) A large placenta; and (7.) Exertion. In the class of rare causes we find (1.) Rupture of the os uteri; (2.) Rupture of a thrombus of the cervix; (3.) Sloughing of the vagina; (4.) Rupture of the perinæum; (5.) Thrombus of the vulva; (6.) Inflammatory ulceration of the cervix; (7.) Purpura; (8.) Constipation; and (9.) Polypus, or fibrous tumour. The concluding chapter supposes the flooding checked, and describes the after-treatment.

Having now given a general summary of the contents and plan of the book, we shall notice a few points of special interest; and, first, distention of the bladder as a cause of flooding. As far as our obstetric reading goes, this part of Dr Earle's book is quite original. The substance of it was, if we are not mistaken, first published in the form of a paper in the Transactions of the London Obstetrical Society. We have not ourselves met with a case of flooding from this cause; but it is well to be on our guard, as immediately *after* the birth of the child the possibility of such a condition existing is apt to be overlooked. Of course, as Dr Earle remarks, cases of draining which stop on the woman raising herself on her knees are not to be set down under the head of which we are speaking, as in such cases the draining is likely kept up by the presence of clots in the vagina, which drop out when the patient raises herself to micturate; but our author says, "I have now met with seven cases of flooding which were distinctly set going and kept up through the injurious influence of a distended bladder upon the lately parturient uterus." The passage which follows we make no apology for quoting, as it serves also as a specimen of the clear, practical, and graphic style in which the book is written. "The bladder, when distended, from its intimate connexion with the lower fourth of the uterus, readily displaces or dislocates that movable organ. It may displace the uterus in four directions,—either backwards, as in retroversion; upwards, or to one side, as may be noticed after delivery; and downwards, if the bladder should be prolapsed. After delivery, the two most common displacements of the uterus from a full bladder are directly upwards, or towards one or other iliac fossa. The hæmorrhage results from the uterus not being able to contract perfectly, either on account of its being displaced, or through the sympathetic influence which exists between the two contiguous organs. A distended bladder is probably one of the causes of *primary* post-partum hæmorrhage only; for, although it might induce in a few cases a slight degree of secondary hæmorrhage, I have gen-

erally noticed that if the uterus once contracts firmly, and keeps so for several hours, subsequent distention of the bladder merely displaces it, but does not affect it so as to set up flooding." . . . "We cannot tell readily, by an abdominal examination before delivery, whether the bladder is full of urine, because, from the pressure of the gravid uterus, the bladder gets flattened from side to side, and the abdominal cavity is so uniformly distended by its various contents. The diagnostic signs of a distended bladder after delivery are more conclusive. We can at any time study them by telling a patient lately confined not to empty the bladder for ten or twelve hours, and we shall then be able to examine at leisure the effect of a distended bladder upon the uterus. The proper position of the uterus after delivery is the hypogastric region; but when the bladder is distended, the uterus is displaced, and its place filled up by the bladder. We find the uterus either still keeping its central position, and pushed directly upwards as high as the umbilicus, or even above, or we find it lying towards one or other ilium of the pelvis. The fundus of the uterus reaching as high as the umbilicus, or even extending above that point, would not of itself be sufficiently diagnostic, for it might be due to simple hypertrophy of its structure, or to its cavity being distended by some foreign mass. The characteristic point is, that, in distention of the bladder, the continuity of the uterus into the pelvis is suddenly lost. When the bladder is empty, if we pass our hand over the uterus from the fundus downwards, we feel it dipping into the pelvis at the symphysis pubis, but not so when the bladder is full; we then feel a soft fluctuating tumour intervening between the hard structure of the uterus and the pubes. If the elevation of the uterus were due to hypertrophy of its structure, or the distention of its cavity by clots or placenta, we should still feel the hard uterus passing into the pelvis at the symphysis; whereas, in distention of the bladder, we lose the hard structure of the uterus at some distance from the pubes, the space intervening being occupied by the soft fluctuating bladder. If the urine be now removed, the uterus, whether it were high up before, or turned to one side, will be found to have descended and taken up its usual central position in the hypogastric region, and the fluctuating tumour will have disappeared. The patient also often complains of feeling a great weight in the supra-pubic region, or expresses a desire to make water. If we place the tips of our fingers on the soft mass between the uterus and pubes, and give with them a sudden jerk, the patient will generally flinch and say that she wishes to pass water. The flinching and desire to empty the bladder is caused by a wave of fluid being urged against the sphincter vesicæ." The treatment of course is the catheter, and several cases are related by Dr Earle in support of his observations.

His advice as to the necessity of introducing the hand as a first instead of a last remedy in certain cases of flooding is in our opinion

most judicious. We could wish we had more space to refer to several other points treated of, such as the evil of overlooking, as is frequently done, a considerable amount of slow draining, and allowing the nurse and patient to look upon such as not exceeding what is proper. However, we cannot enlarge upon this, but shall confine ourselves to a few words on one other point of practice, which by many may be considered heterodox, viz., the time one ought to wait before removing the placenta when adherent, there being *no* hæmorrhage going on. We entirely agree with Dr Earle in thinking it a mistake to wait longer than half an hour. Nothing is to be gained by waiting longer; on the contrary, the difficulty of introducing the hand on account of the uterine contractions, and of manipulating when it is introduced, increases in proportion to the time we wait, to say nothing of the probability of irregular contraction supervening. This last condition—irregular contraction—is, we are convinced, more frequently caused by the ineffectual efforts of the uterus to expel an adherent placenta than by anything else.

Dr Earle in this book has exhausted the subject of post-partum hæmorrhage, and has rendered a subject interesting in itself still more so by the addition of cases illustrative of every variety of flooding. We can with confidence recommend its perusal to all medical men, but more especially to junior practitioners and students, who will find it of the greatest assistance to them in the management of this dangerous complication of labour, and who will, we feel sure, thank our author for the numerous practical “wrinkles,” to use a slang phrase, which he has not thought too insignificant to notice. We need only further add, that in externals the book is very tasteful.

Lectures on Clinical Medicine, delivered at the Hôtel Dieu, Paris, by A. TROUSSEAU, Professor of Clinical Medicine in the Faculty of Medicine, Paris, etc., etc. Translated and edited, with Notes and Appendices, by P. VICTOR BAZIRE, M.D. Lond. and Paris, etc., etc. London: Hardwicke.

ON the original publication of the *Clinique Médicale de l'Hôtel Dieu de Paris*, we laid a pretty full analysis of it before our readers, and expressed the high sense we entertained of its value. So much was the work appreciated, that a large impression was soon exhausted, and in the beginning of last year a second edition was published. The new edition consisted of three volumes instead of two, and contained additional chapters on a large number of interesting subjects. Dr Bazire comes forward as the translator of this new edition; and the first part, now before us, contains seven lectures. In performing this task, Dr Bazire confers a

great benefit upon the medical profession in this country. Trousseau, though a thorough Frenchman, possessing the characteristic genius and *verve* of his countrymen, has points which are calculated to make him specially appreciated among ourselves. He has a great respect for English physicians, and is well acquainted with our medical literature; he is not carried away by theories, but is an eminently careful and practical observer; and he is an eclectic, so that he brings before us at once all that he thinks most valuable in French, and, indeed, in modern medicine. Accordingly, we know no foreign author whose works are more likely to be appreciated here than Professor Trousseau's.

Dr Bazire, in his translation, does not follow the arrangement of subjects adopted by his author. In the original, the subjects first treated of are the Exanthemata; but Dr Bazire, considering that the arrangement of the lectures was merely arbitrary, has "selected for earlier publication those lectures that are likely to excite the most interest at the present time." Accordingly, the first part contains the lectures on the following subjects:—Venesection in Cerebral Hæmorrhage and Apoplexy; Apoplectiform Cerebral Congestion, and its relations to Epilepsy and Eclampsia; Epilepsy; Epileptiform Neuralgia; Glosso-laryngeal Paralysis; Progressive Locomotor Ataxy; and Aphasia. The subjects are certainly of great interest, and are treated by Trousseau with eminent ability. We would particularly direct attention to the lecture on Aphasia, which gives an admirable account of what is as yet known regarding this very interesting condition.

For the mode in which Dr Bazire has executed his task he deserves unqualified praise. His translation, while perfectly faithful, is very readable, containing just enough of French idioms, or rather of the French turn of thought, to impart additional piquancy to it. But Dr Bazire has not rested content with a mere rendering of the words of his author into English; he has added some valuable notes and cases, especially on the subjects of Progressive Locomotor Ataxy and Aphasia, and brings down his information to the very latest date. The work is well got up, and the price very moderate; the first part, containing 276 octavo pages, being published at the price of four shillings. We trust that the subsequent parts may be issued at regular and, if possible, short intervals. If this be the case, and if, as we fully anticipate, their translating and editing be conducted with the same care as that exhibited in the volume before us, we venture to predict for this work a decided success.

Outlines of Elementary Botany. For the use of Students. By ALEXANDER SILVER, M.A., M.D., Assistant to the Professors of Materia Medica and of Medical Jurisprudence in the University of Aberdeen. London: Renshaw: 1866.

IN this little volume Dr Silver has given a very clear outline of a subject which the medical student too often looks upon with peculiar dread. Botany by the many is regarded as a bugbear, partly on account of the large number of new names, and the supposed difficulty of the terminology; partly on account of the large size of the text-books usually put into their hands. From the latter reproach Dr Silver's work is perfectly free. A neatly got up 32mo volume of 350 pages, containing a very fair number of illustrations, is rather calculated to attract than to repel. Dr Silver modestly says that his manual is intended not as a substitute for, but as an introduction to, the larger standard works. It would, of course, be absurd to compare it with these, but this by no means detracts from the positive merits of the work, which contains a large amount of valuable information arranged in a clear and distinct manner. The work is divided into five parts. The first treats of vegetable morphology; the second of physiological anatomy or histology; the third of vegetable nosology of pathology; the fourth of systematic botany; and the fifth of geographical and geological botany. As a specimen of Dr Silver's style we extract a short passage from the chapter on vegetable pathology:—

“As to the cause of the potato disease, many opinions prevail, all holding that a fungus is developed in the diseased tuber, but differing as to whether this is a cause or a consequence of the disease. The fungus is termed *Botrytis infestans*, and attacks all parts of the plant very rapidly, beginning with the leaves, which assume an appearance like those acted on by sulphurous acid or other irritant gases. Timber, whether alive or dead, when exposed to damp in a still atmosphere, is attacked by what is termed the “Dry Rot.” The wood begins to decay, and thus furnishes a soil for the fungi, which are soon developed. The fungi are chiefly *Merulius vastator*, and *Polyporus destructor*. As soon as these begin to develop, their spawn spreads with great rapidity, and deprives the wood cells of their contents, so that the wood becomes brittle. When wood is allowed to dry properly, and exposed freely to the air, it is not so likely to be attacked. To preserve wood from this, various substances, such as corrosive sublimate and creosote, have been employed to coagulate the albumen contained in it. Chloride of zinc has also been used for the same purpose. Dodder, a phanerogamous parasite, often attacks flax and clover, and is very injurious to them.

“The disease which attacks wheat, known as Earcockle, is caused by small vibriones which make their way into the interior of the grain, which they entirely destroy, and replace by a cottony substance. Scalding water destroys the vibriones. Many insects are destructive to vegetation—some, like the locust, destroying everything before them, others selecting one particular plant for their ravages. A species of *Cecidomya* attacks wheat, and inflicts great injury on it. Aphides or plant-lice, are found on almost every variety of plant, and increase with enormous rapidity; fortunately, they are destroyed in great numbers by the little beetle called the “Lady-bird.” Cochineal is produced

by a Coccus which attacks the Cactus plant, and galls by a Cynips or Diplolepis, which deposits its eggs on the oak. Besides these, an almost infinite variety of insects infests vegetables, and many prove very destructive, though few are so much so as the larvæ of a beetle called Elater, the well known and dreaded "Wire-worm." This animal is cylindrical in shape, of a yellowish colour, marked with rings, and having a hard skin. A crop of white mustard appears to have the power of destroying them, but crows are their prime enemies."

A valuable feature in the work is that under the heads of stem, leaves, fruit, seeds, etc., lists are given of all the officinal vegetable substances contained in the British Pharmacopœia.

We can cordially recommend Dr Silver's work either to those students who are for the first time entering upon the study of botany, or to those who are desirous of refreshing their memories upon a subject, the details of which, unless learned practically, are very apt to slip out of the mind.

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLV.—MEETING V.

7th February 1866.—Dr MOIR, *President of the Society*, in the Chair.

I. SPECIMENS OF TUMOURS.

Professor Spence exhibited three tumours removed from patients at present under his care. The *first* was a tumour of the upper jaw removed from a young woman. It had projected externally and into the mouth. When examined it was found to be hard and firm, and quite defined posteriorly; though extending up to the floor of the orbit, it was evidently circumscribed. The only suspicious circumstance in the case was, that the growth had been rather rapid; the tumour having only existed for two years, or rather less. Still, as the woman was healthy, and the growth was quite circumscribed, Professor Spence had determined to remove it. The operation was performed by an incision down the side of the nose, and another from the angle of the mouth. The tumour completely occupied the antrum; and as the malar bone was involved, the greater part of it required to be removed, as well as the superior maxilla. The removal was effected easily enough, but the growth split at one point; the splitting showed the fibrous structure of the tumour, which was rather softer than had been expected. The tumour had been examined subsequently; and though for the most part fibrous, rather too many proliferating cells had been found in it. The result, so far, had been satisfactory; and at all events the tumour was less likely to recur than one of the ordinary soft growths of the antrum. The *second* was a tumour of the lower jaw, and had been removed last week. The tumour arose close to the neck of the lower jaw, and involved the descending ramus. It projected into the throat, and extended to the pillars of the fauces. It was of distinctly cystic character, and another unfavourable circumstance had been the rapidity of its growth, having only existed for four months. Several of the

teeth had fallen out, and there was bleeding from the gums. The rest of the bone was healthy. At the commencement of the operation, as it was quite possible that the swelling might have been a periosteal abscess, Mr Spence made an exploratory puncture, but, as he had expected, only a bloody sanies was discharged. The jaw was then removed; the only point of difficulty was, that as the masseter and internal pterygoid were involved, the coronoid process was not easily felt, and it was found that a small portion of it had given way. So far the case had gone on well; the operation had at least given temporary relief, and, as the growth had been so rapid, had given the patient his only chance. The third specimen was an illustration of the difficulty there sometimes was in deciding as to the nature of a case. The patient had been sent to Mr Spence by Dr Laurie of Musselburgh. At that time there was a slight swelling in front of the tibia, of a soft and elastic consistence. The patient, a girl, had a strumous appearance; still, there was something about the appearance of the limb and the swelling which made Mr Spence pronounce that the growth was malignant. The case was watched for a week or two, and the diagnosis was fully confirmed. Still, in this case as in the last, an exploratory incision was made into the growth, when blood gushed out. The limb was then amputated, Mr Spence following his usual practice in such cases, of not operating through the continuity of the bone, but at the joint, as there was thus a better chance of the tumour not returning. The growth occupied the whole thickness of the bone, but none of the fibres of the tibialis anticus were involved though stretched over the tumour, and it was also separated from the tibialis posticus and the flexor; but the head of the soleus could not be detached from the covering of the tumour.

II. LACERATION OF THE BRACHIAL ARTERY.

Professor Spence showed this preparation as an illustration of the arrest of hemorrhage by the tearing and twisting of an artery. The patient's arm had been torn off, and a piece of the brachial artery, three or four inches long, was hanging out; the median nerve being more resilient than the artery, a still larger piece of it was left. In the brachial artery the twisting could be well seen, which had had the effect of completely arresting the bleeding.

III. URINARY CALCULUS.

Professor Spence showed a calculus which he had recently removed from a boy. It had almost the shape of a dumb-bell, and from its peculiar form some had supposed that it had for its nucleus a foreign body. This it would be seen was not the case. It was an ordinary uric acid calculus, coated with phosphates, with another phosphatic one attached to it.

IV. AMPUTATION AT THE SHOULDER-JOINT FOR GANGRENE OF THE ARM.

Mr Annandale showed the bones of a patient's forearm. The preparation only illustrated an ordinary fracture of the upper third of the ulna. Still, the case was of some interest. The patient, a gentleman thirty-five years of age, some days ago got his arm jammed between the buffers of two railway carriages. When his medical attendant saw him, he found considerable swelling of the limb, and recognised a simple fracture of the forearm. The arm was put up in splints, but the patient complained of much pain. Things went on well for a week, when gangrene appeared and spread up the arm. Mr Annandale was asked to see the patient in consultation, and yesterday found gangrene extending from the hand up to the axilla. The patient was very delirious. Mr Annandale recommended amputation at the shoulder-joint as the only chance of saving life. The patient was placed on a mattress, and on examining the arm with a view to planning the incisions so as to avoid the gangrenous parts, Mr Annandale was surprised to find that the head of the humerus was dislocated downwards and forwards. This circumstance, accordingly, was taken into account in the performance of the operation. The first flap was taken from the external and anterior part of the arm, and the incision exposed the joint.

The capsule was then opened; it was found very difficult to displace the disarticulated head of the bone, which was firmly wedged below the coracoid process.

V. CALCAREOUS DEGENERATION OF THE TUNICA VAGINALIS.

Mr Annandale showed a specimen of this condition. The patient had had a hydrocele for twenty years. Latterly it had become much firmer, and had, accordingly, been removed by Professor Syme. It would be seen that in the wall of the tunica vaginalis were large flakes of calcareous matter. It was rare to meet with it to such an amount.

VI. AMPUTATION OF BOTH LEGS.

Mr Annandale showed the photograph of a patient from whom he had recently amputated both legs at the knee-joint, on account of injuries sustained by the falling of a heavy piece of metal upon them. He had followed Mr Carden's method, and the results, both on this and on other occasions in his own and in Mr Syme's practice, had been most satisfactory.

VII. EXCISION OF THE KNEE-JOINT.

Dr P. H. Watson showed two specimens of excision of the knee-joint. The *first* was from a lady who had left her friends from some temporary alienation, and was staying in lodgings. She had thrown open her window, had fallen out, and sustained a compound fracture of the patella. Some portions of the bone had been driven into the femur. Excision of the joint had been performed about twelve days ago, and the case was going on most satisfactorily. The *second* preparation was from a case where disease of the articulation had existed for some time, and which latterly seemed about to terminate in suppuration. An abscess had formed in the popliteal space, and had been opened. On indications of acute suppuration of the joint setting in, Dr Watson recommended excision of the joint, and the operation was accordingly performed yesterday. In the cavity of the joint a quantity of a curdy purulent matter was found. With regard to the after-treatment, Dr Watson stated that in all cases of excision of the knee-joint he applied a splint on the posterior surface of the limb, and an iron-rod in front; a bandage steeped in plaster-of-Paris was then applied, and in this way the limb could be slung from the very first day.

VIII. PRIMARY EXCISION OF THE WRIST.

Dr P. H. Watson showed the bones of the wrist-joint which he had lately excised. The patient, a boy, was firing a small cannon on New-year's night, using a piece of a broken file as a ramrod. The cannon was accidentally discharged, and the ramrod passed through the wrist, the end bulging up the skin on the farther side. On withdrawing the ramrod and passing the finger into the wound, Dr Watson found that the os magnum was broken, and the articulations much injured. He accordingly excised the wrist-joint, employing two lateral incisions, taking care to avoid all the vessels.

IX. LACERATION OF THE BRAIN.

Dr P. H. Watson showed a specimen of laceration of the brain. The patient, while on his way home in a state of intoxication, had fallen down, or been knocked down, sustaining an injury of the head, and was brought to Chalmers' Hospital in a state of profound coma. He never recovered consciousness, and died in three days. On the right side the hemisphere was extensively lacerated, and a large clot lay between the brain and the dura mater. Posteriorly, there had been hæmorrhage from the lateral sinus, which had extended to the base of the brain and medulla oblongata.

X. URINARY CALCULUS.

Dr P. H. Watson showed a urinary calculus which he had removed from a boy nine years of age. Two years ago, the boy had a first epileptic attack, and

he had since had intermitting strabismus. Whether the epilepsy and squinting had been due to the presence of the calculus Dr Watson could not say, but it would be interesting to notice whether there was any improvement in these symptoms.

XI. MET-ALBUMEN IN HORSE'S URINE.

Dr Arthur Gamgee showed a specimen of horse's urine, which contained one of the rarest of the albuminoid, or rather gelatiniform bodies, discovered by Professor Scherer of Wurtzbourg. Two years ago, Dr Gamgee had had sent to him samples of urine from diseased horses. They had all a slimy or gluey consistence. No coagulation was produced by heat or by acids, but an albuminoid substance (met-albumen) was detected. When nitrate of the sub-oxide of mercury was added, and the urine boiled, a red coloration was produced; when alcohol was added a flocculent precipitate was thrown down. Dr Gamgee had met with several cases of the same kind in all the horses that had been suffering from disease of the kidneys. The urine now exhibited to the Society had been sent to Dr Gamgee from London; from its appearance he suspected that it contained albumen, and on testing it, it was found that it did. The Society would observe its peculiar gluey consistence.

XII. ANEURISM TREATED BY GALVANO-PUNCTURE.

Dr John Duncan showed a large aneurism in which galvano-puncture had been performed to arrest hæmorrhage. The whole arch of the aorta was much dilated, and the aneurism projected through the upper part of the sternum. During life the sac gave way externally, and the hæmorrhage, which would have proved speedily fatal, was arrested by galvano-puncture. It would be seen that large clots had been formed in the sac.

XIII. CASE OF APHASIA.

Dr Sanders exhibited a patient suffering from aphasia. He had this day presented himself at the Infirmary. The disease had existed for about thirteen years, and consisted of hemiplegia of the right side, and loss of speech. The loss of power of the right side and of speech had at first been complete, but had here been partially recovered. There was, however, still some loss of memory of words.

XIV. ON SOME POINTS IN THE ADMINISTRATION OF CHLOROFORM.

Dr John Smith read a paper on this subject, which will be found at page 804 of this number of the Journal.

Dr Andrew Inglis had paid great attention to the subject of chloroform. There was one point which he considered of great importance: if administered too suddenly, deglutition and respiration were impeded. This was avoided by giving the chloroform gradually—holding it at a distance at first. If, during the excited stage, the tongue was retracted spasmodically there was no use for artery-forceps, as the tongue could easily be pushed into the floor of the mouth with the finger, and if the chloroform-cloth were taken away at the same time, the respiration would go on again. If, on the other hand, it should fall back passively at a later stage, it could also be made to fall forward or to the side, or could be pushed out of the way with the finger. By thus turning the head the saliva would also run out, while, if the artery-forceps were used under either of these conditions, deglutition was totally interrupted, and an opportunity afforded for the saliva to run down into the larynx; besides, forceps were liable to lacerate and injure the tongue.

Professor Spence could not agree with Dr Inglis in his last remarks. While it was of course essential to clear away the fluid from the mouth in the first instance, there was nothing from which he had seen more good, or had seen more lives saved, than pulling forward the tongue. It should be remembered that not only was the tongue pulled forward, but the epiglottis as well, so that air was admitted into the larynx. As to the mode of death, he had seen two cases terminate fatally: the one a patient of his own, the other of a colleague.

In both the pulse ceased before the respiration. The latter case he had had the best means of watching; the patient had been a somewhat dissipated man, and there was some difficulty in bringing him under the influence of chloroform, which had been administered pretty freely. The breathing became stertorous, when an assistant said that the pulse had stopped. Professor Spence was holding the leg, and immediately put his finger on the posterior tibial, but it had ceased to beat. The patient was still breathing, but the chest was quite stiff, and all means employed to restore him proved ineffectual. The man evidently died from failure of the heart's action. Mr Spence's own fatal case was one where he had amputated the penis; the operation was performed very rapidly; the breathing was a little stertorous; on looking for the vessels, in order to secure them, none bled. The man was still breathing, but though all means were tried he did not recover, but died of syncope. On post-mortem examination only some traces of fatty degeneration of the heart were found. In both cases the patients were breathing when the pulsation ceased in the arteries; the men were deeply under the influence of chloroform, but were not getting it at the time. Mr Spence had lately seen a case which was very nearly fatal. It was a case of amputation at the ankle-joint. On proceeding to secure the vessels none bled. When the lad was examined he was seen to be pale; Mr Spence drew forward the tongue, and this, with artificial respiration, undoubtedly saved his life. With reference to the use of the artery-forceps, Mr Spence might observe that it was quite sufficient to pinch the tongue, and that it should never be torn or lacerated.

Dr Gillespie stated, with reference to the case last mentioned by Professor Spence, that he was present at the time of the operation, and felt great alarm at the state the man was in. He had no doubt the result would have been fatal had the tongue not been pulled forward. The pulse and respiration had stopped; and even when the tongue was pulled forward, five or ten minutes of artificial respiration were required in order to restore him. At the same time Dr Gillespie thought the artery-forceps were used much too often, for he considered it cruel to employ them unless it was absolutely necessary. Still in some cases, such as the above, there was no choice. Dr Gillespie quite agreed with Dr Smith as to the danger in some cases of not giving enough of chloroform. Some patients, particularly young hysterical girls, were very apt to faint when submitted to a little pain, if not thoroughly unconscious. He had lately taken a lady to Dr Smith to have a tooth extracted; when returning to consciousness she fainted, and there was a good deal of difficulty in restoring her. He was afterwards told that she was very subject to fainting-fits. She fainted in the cab on her way home, and again when put to bed. This was just one of the cases where a fatal result might have occurred. In Dr Gillespie's own unfortunate case the pulse stopped before the last breath, for after the heart had ceased the patient gave several prolonged respirations. It was more dangerous to give chloroform in such cases than even when there was organic disease of the heart. He had lately administered it quite successfully to a man eighty-four years old, who had very extensive atheroma of the arteries. Dr Gillespie remembered a girl coming to the hospital to have a whitlow opened. At that time he was not so well aware as now of the danger of administering chloroform to a patient in the sitting posture. The patient was sitting, a little chloroform was given, and before she was quite over he plunged in a lancet; the patient fainted, and there was the greatest difficulty in rousing her. Ever since he had administered chloroform in the recumbent posture.

Mr Annandale could not help thinking there were two principal points to be attended to, 1st, How to administer chloroform; 2d, Are there any symptoms which should prevent us from administering it in certain cases? With regard to the first point, he had no doubt that the mode of giving it, employed in Edinburgh, was the safest. A little chloroform was poured on a towel, this was brought carefully in contact with the mouth and nose, and the state of the respiration was carefully watched. He could not agree with Dr Inglis that the tongue should not be drawn forwards. If the respiration became affected Mr

Annandale thought the tongue should be drawn forwards. As to the position of the patient, he thought it immaterial whether this was sitting or lying, so long as the state of the breathing was attended to. He had given it often in both attitudes without any bad effects. As to the second point, Mr Annandale was afraid there were no symptoms which could point out to us the cases where chloroform might be dangerous. Fatal cases would occur, but he did not think there were any symptoms which contra-indicated its use.

Dr Andrew Wood thought there could be no more important inquiry than into the cause of deaths from chloroform. Many of these were, no doubt, unavoidable, some were not; the effect upon the public of such cases was to alarm them, and bring chloroform into unmerited discredit. From his own observation, he had little doubt that many of the fatal cases were due to the manner in which chloroform had been administered. All his experience led to this, that there was a large amount of danger in the too rapid administration of chloroform, from an undue haste to obtain its anæsthetic effects. He had also observed, and been pained to observe the way in which large quantities of chloroform were poured upon a towel folded four or five times. This was, in fact, a large compress over the mouth, and was a source of double danger, for not only was the chloroform administered too rapidly, but the towel over the mouth was a source of danger, by preventing the patient from inhaling enough of air. If a fatal result occurred under such circumstances, he maintained that chloroform was not responsible for it. *Dr Wood* believed that a great improvement on this method was to place a thin towel or handkerchief over the face previously well greased, and to drop the chloroform gradually on the surface furthest from the face. This was the safest and most effectual plan, and that, too, in which there was least expenditure of chloroform. On two occasions, some time ago, he had been present in cases of labour. On the first, chloroform was administered in the usual way, in a thick towel. The case was a tedious one; and a large amount of chloroform, from six to eight ounces, were employed. The second case was equally tedious, but here chloroform was administered by means of a handkerchief, and only half-an-ounce of chloroform was employed, and with more satisfactory results than in the other case. Ever since, he had adopted the latter way of giving chloroform. As to the position of the patient, *Dr Wood* could not agree with *Mr Annandale*, that it was indifferent whether he was sitting or lying. If there was anything at all in what *Dr Smith* had said, there was a danger from syncope, and this was more likely to be avoided by making the patient assume the horizontal posture. As to the employment of chloroform itself, we must in this, as in all other cases, be content to purchase a great blessing by some risk. While, however, he believed that some fatal cases must occur, he was satisfied that a great many had proved fatal from the way in which chloroform had been administered. He believed that nothing had tended more to advance Surgery than chloroform. So far as he himself was concerned, he would not submit to a painful operation without it.

Mr Annandale believed that most fatal accidents had occurred where artificial apparatus had been employed for introducing air at the same time as chloroform. The result was not surprising, for most of them had the effect of intensifying the chloroform instead of diluting it.

Dr Argyll Robertson observed that the means to be employed to recover a patient, when in a state of syncope, required more investigation than it had yet received. He was surprised that no allusion had been made to one special means,—namely, electricity. This, he believed, had been comparatively seldom employed. The subject was one regarding which there was great diversity of opinion, especially as to the method to be employed. The ordinary interrupted current had usually been continuously employed; the effect of this was to produce spasm, and this, in such cases, was not likely to be beneficial; it was from its intermitted application that he thought good results were likely to be obtained. The point was one which deserved special investigation. In some cases it might be well to apply the current to the abdominal muscles, in some to the

chest, in some to the phrenic nerve; and it was an important point to determine, in particular cases, which was the best.

Mr Annandale agreed as to the importance of the question suggested by *Dr Robertson*; there might, in some cases, be a danger of electricity preventing the heart's action.

Dr Dyer alluded to the state of the mouth when chloroform was administered; he had often seen a large cork introduced between the teeth, to keep the jaws apart, when a tooth had to be extracted. In these cases deglutition was invariably attempted, but without success. *Dr Dyer* had seen patients suffer violently from attempts at deglutition and threatened suffocation. He thought it was bad to introduce any such thing into the mouth during chloroform inhalation. The papers had lately been full of the narrow escape of a "lion-tamer," who had put his head into a lion's mouth and had been bitten by the animal. *Dr Dyer* believed that the bite was merely the result of an attempt at deglutition, while the jaws were held forcibly open. Had the bite been the result of ferocity, the animal would, no doubt, have killed the man, especially after it had once tasted blood.

Mr Hepburn thought that the great risk in administering chloroform was the strength of the vapour inhaled. It had been said that most fatal cases occurred where instruments for diluting the chloroform vapour with air had been employed; with this he could not agree. Of course, the instrument employed must be a good one. He believed *Clover's* to be the best. During the administration of chloroform he had always kept a thin piece of india-rubber between the jaws, and had never found any bad effects from it. Patients, it was well known, in general kept their jaws firmly shut while under the influence of chloroform.

Dr Moir quite agreed with the observations of *Dr Andrew Wood*. He was not at all afraid of administering chloroform; having given it constantly since its first introduction, and having scarcely, if ever, declined to employ it. There was, however, one thing he must mention, more especially as there were several Infirmary surgeons present. On a few occasions, when present at operations in the Infirmary, he had seen it administered, and had been really frightened at what he had seen; at least he would have been afraid to give it as he had seen it given there. A thick doubled towel was placed over the patient's mouth, and in many cases had produced stertorous breathing, so marked that he would have been terrified had the patient been his own. Within the last few months he had seen (not in the Infirmary) an assistant give chloroform by pouring it upon a cloth, folded four or five times, and then applied close to the mouth. *Dr Moir* felt satisfied that if he had not interfered, the patient would have died of asphyxia; the face was purple, the breathing was stertorous, and the man struggling for breath. Seeing this, he was surprised that there were not more deaths. As to himself, he had never seen any harm result from its use; nor had he ever once found it necessary to pull out the tongue, as described by former speakers, though he had always put his patients into a state of complete anæsthesia. For some years the only plan of administering it he had adopted was to hold a strong muslin handkerchief over the patient's face, so as to allow the inhalation of plenty of atmospheric air, and to drop the chloroform slowly upon the handkerchief. When the breathing became stertorous he immediately stopped, and resumed its use from time to time to keep up the desired effect.

Professor Spence could not allow *Dr Moir's* remarks to pass unnoticed. The results in the Infirmary here had been less fatal than in any other place. The reason was obvious. The result was more likely to be fatal when the patient was not put thoroughly, completely, and rapidly under the influence of the chloroform. When it was inhaled rapidly, the patient went over rapidly; and when it was given as he had seen it, by dribbling, there was far more risk of asphyxia. There had been no cases in the Infirmary fatal by asphyxia; in the only two fatal cases which had occurred, death had been due to syncope, and in both the chloroform-cloth had been previously removed.

Dr Smith remarked, in reference to keeping the mouth open artificially during the giving of chloroform in dental cases, that he had long ago pointed out that it was not only hazardous but quite unnecessary; for when enough was given it was easy to open the mouth; it was only when enough had not been administered that the jaws were clenched; any one who had experience in chloroform during capital operations knew this. Both in these and in minor cases it was an error to commence operating at such a time.

XV. CASE OF APHASIA, WITH RIGHT HEMIPLEGIA AND LESION OF THE THIRD LEFT FRONTAL CONVOLUTION OF THE BRAIN.

Dr Sanders read a paper on this subject, which will be found at page 811 of this number of the Journal.

Dr Haldane spoke of the great interest of the subject, and gave the particulars of four cases of aphasia in connexion with right hemiplegia, which he had at present under observation.

Mr Turner thought the subject of peculiar interest in connexion with the attention which had lately been paid to classifying the cerebral convolutions. The old indefinite descriptions of localities would no longer be sufficient, and it would be necessary to define with accuracy the situation where lesions were met with.

Dr Sanders alluded to the assistance afforded by comparative anatomy in studying the cerebral convolutions. In the human brain they seemed at first very complicated; and he had been much assisted by *Mr Turner* showing him the brains of several apes, in which the convolutions were arranged on a much more simple plan.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXV.—MEETING III.

13th December 1865.—*Dr CHARLES BELL*, Vice-President, in the Chair.

I. ABDOMINAL BANDAGES.

Dr Keiller read a paper on "Abdominal Bandages," and exhibited a variety of the most approved forms. *Dr K.* specially directed the attention of the Society to a bandage which he had constructed and found to fit well, and to give ample support without causing undue pressure.

Dr Charles Bell thought that, in dealing with bandages, simplicity in construction and facility in application were the main considerations.

Dr Keiller said that some women required more support than others, especially those that had born several children. Among the bandages now exhibited would be found a varied selection of simple forms.

Dr James Sidey showed the full dress of a native Kafir, which consisted of a dried imitation fig-leaf. He stated that at seventeen years they were circumcised.

II. SPECIMENS OF MORBID PLACENTÆ.

Dr Andrew Inglis showed five specimens, and said:—

The first two were true cases of fatty disease. They were quite white, not only on the surface, but in the substance also. The lobes were strongly marked, and the whole placenta was quite soft and friable. In both cases the patients were syphilitic. The disease had involved the whole placenta, and the children were both dead. Labour came on about the eighth month. He also showed a specimen of cretaceous deposit on the surface of the placenta. On looking at it with the microscope, the chalky matter was found deposited around the loops of the bloodvessels. After the child was born the placenta was detached, but did not come away. On putting the hand into the uterus, it was found quite uncontracted, but there was no bleeding. The placenta was lying loose and quite flat in the uterus, but,

when it was doubled up, the uterus contracted and expelled it. In this case, also, both mother and child were syphilitic. The child was alive, large, and well-formed. The next two were instances of clots formed between the uterus and placenta. The patients both had shown symptoms of loss of blood during pregnancy, and expelled during labour a quantity of black matter, which seemed to be decomposed blood. There had been great pain and feeling of distention at the point where the placental bruit was loudest. In one case the symptoms appeared early and repeatedly, and there could be seen consecutive layers of clot covering a great part of the placenta. At the time of delivery in both, there was also (besides what was adherent to the placenta) a large firm clot expelled by itself.

III. BLIGHTED OVUM.

Dr James Sidey exhibited a blighted ovum, born at the seventh month; the patient had been threatened with a miscarriage once previously.

Dr Keiller said it was what is generally called a mole pregnancy, where the ovum had died.

Dr Charles Bell agreed with *Dr Keiller*, but thought this was a specimen of tuberculated ovum, which had been well described by Sir James Young Simpson, and of which we had had several specimens brought before us.

Dr Sidey said his patient had menstruated slightly at each period.

Dr Charles Bell had known many cases where these blighted ova had been retained for many months.

Dr Sidey had one case lately where it remained for nine months, and he was sent for by the patient for what she considered her confinement, and an immense mole was discharged.

Dr Cairns related a case where he had seen a mole discharged before a full-timed child.

Dr Charles Bell remarked that it must have been a case of twin pregnancy, in which one ovum had been blighted.

IV. INDUCTION OF PREMATURE LABOUR.

Dr Keiller made some remarks on the course of treatment to be recommended in the case of a patient who had lost seven children (stillborn), and who was now again pregnant, and, although seven months, he (*Dr K.*) could not now hear the fetal heart. She was of usual size, with the placental bruit quite clear. There was a general diffused fulness not circumscribed. Should premature labour be induced was the question to settle, and he (*Dr K.*) purposed doing so, provided the child was not already dead.

Dr Young said we cannot depend on the statements of patients as to the foetal movements. There was no more difficult question to decide than when premature labour should be induced. In one case he had lately, he (*Dr Y.*) said, at seven months, it was too soon, at another time too late, and, on the last occasion, labour came on spontaneously about the seventh month and the child did well.

Dr Sidey recommended the patient to be left alone, as there was no deformity.

Dr Keiller said that premature labour had been induced twice in this case, but all the children had died apparently from diseased placenta.

V. HISTORY OF A CASE OF OVARIAN TUMOUR. BY DR JAMES YOUNG.

I have brought this case under the notice of this Society, not as presenting anything new, either in the nature or treatment of ovarian tumours, but merely that I may give you a succinct account of one of those cases which have occupied the attention of the profession for some years with so much interest. The following case I watched from its commencement, in all its stages, till it ran its fatal course, after nearly seven weary years.

Mrs F., æt. 40, was confined of her second child on the 1st of August 1858. Her labour was easy, and presented no abnormal feature; her recovery was favourable till the tenth day, when she complained of a slight pain in the

right iliac fossa, but which was speedily relieved; and I discontinued my attendance. Some few weeks elapsed, and I was again sent for, when she told me she had distinctly felt a small swelling (as she called it) in her right side, and that she had felt it first on the fourteenth day after her confinement, four days from the commencement of the pain she had complained of, but likewise stated that it caused her no inconvenience. I ordered her to use the tincture of iodine externally, and the iodide of potassium internally, to avoid all undue exertion, and to pay the strictest attention to her general health. Six months after her confinement, I found the tumour somewhat of the size and shape of an egg.

She nursed her child for about twelve months, and menstruated at the expiry of the thirteenth month (1st September 1859). The uterine functions continued till September 1860, when menstruation was suspended. On the 1st of January 1861, she sent for me, and drew my attention to her increased size. On examination with the stethoscope, I heard the usual bruit of pregnancy; the ovarian tumour was considerably larger; but in the circumstances I advised that she should not interfere, or adopt any method of treatment, till after her confinement. The presence of this tumour neither during her pregnancy nor at her confinement caused any annoyance or difficulty, and she was easily delivered of her third child on the 6th of June 1861. The tumour was at this time easily felt through the abdominal parietes, and was of considerable size, and entirely confined to the right iliac fossa. On the 9th of June, she was seized with a violent attack of acute peritonitis, which yielded to the usual active treatment. The case now went on well, and in fourteen days she was completely convalescent. Professor Simpson saw her in the end of June, and carefully examined the tumour, which, since her confinement, had grown considerably. She was ordered bromide of potassium, and the iodine ointment externally, which was continued for some weeks.

In August of the same year my patient was able to be removed to Cheshire, by which change her general health was much benefited. After her return in October I saw her, and found the tumour still increasing, surrounded now with a large accumulation of ascitic fluid, and which was becoming most distressing to the patient, so much so that she could not lie down in bed. Dr Simpson and Mr Edwards saw her with me on the 1st of November 1861, and recommended paracentesis without delay. I accordingly tapped her on the 8th of November, to her inexpressible relief.

I regret I have no note of the precise quantity of fluid drawn off; but at this time, as well as at future tapplings, it ranged from three and a-half to four and a-half gallons. The patient recovered well during the first ten days, when she was seized with an attack of vomiting with cramp. Professor Simpson and Mr Edwards saw her on the 18th of November, and her case was almost considered hopeless, the pulse being very weak; her lips and face assumed a most cadaverous hue. The vomiting continued constantly for ten days, during which time she was nourished with enemata of beef-tea and wine.

On the 1st of January 1862, she was able to be out.

The history from this date has no feature of any importance, till we find her again complaining of pain and breathlessness about the beginning of 1864. On the 10th of April of that year, Professor Simpson saw her and tapped her, to her great relief. The ascitic fluid surrounding the ovarian tumour proved to be clear and thin, and measured about three and a-half gallons. She recovered slowly but favourably. Several adhesions were now easily felt, and distinct bands could be traced stretching across the abdomen. Observe, please, the period between the tapplings—8th November 1861 and 10th April 1864—nearly two years and a-half. The third operation was necessary on the 29th of October of last year, there being an interval of only six months and a-half. A fourth tapping was demanded on the 2d December, being an interval of only thirty-five days. On each occasion the operation was postponed as long as possible, and the quantity of fluid at each time was always about the same as at first. Her recovery was very protracted. Ovariectomy was spoken

of after the second tapping, but Professor Simpson hesitated, on account of the numerous adhesions, combined with the patient's extreme debility. About the beginning of this year, Mrs F. expressed an earnest wish that further measures might be had recourse to, so as to give her a chance of life, as she felt strongly persuaded in her mind that she would not survive long. On account of her extreme solicitations, I asked Dr Keith to perform ovariectomy. Early in January he carefully examined the ovarian tumour, but considered the case extremely hazardous on account of the adhesions, and the extreme debility of the patient. On the 11th of February last, the operation of ovariectomy was performed in presence of Dr Keith, Dr Sidey, my father, and myself. The operation was performed in the usual way, and I regret to add that the patient died thirty-one hours after; but as the case will be mentioned by Dr Keith, in his published list of cases of ovariectomy, I forbear further mention. The tumour was solid, and weighed from fifteen to twenty lbs.: there were no cysts, except one or two, very small, on the anterior surface. This proves, therefore, that the fluid which collected in the cavity of the peritoneum was purely ascitic, secreted from the whole peritoneal surface,—indeed, the red congested state of the viscera makes this point certain.

This case is referred to by Dr Thomas Keith (see this Journal for January 1866).

Dr James Sidey mentioned a case which he had, where seven complications existed, namely, anasarca from albuminuria, blindness, slight hæmorrhage, footling presentation, prolapsus funis, turning, and placenta adhering.

Dr Black introduced a patient, where Professor Simpson had operated on a large nævus (size of a walnut), destroying it with *potassa fusa*, while under the influence of the new anæsthetic chloro-carbon.

SESSION XXV.—MEETING IV.

10th January 1866.—Sir JAMES YOUNG SIMPSON, Baronet, in the Chair.

Dr Moir rose and said,—Gentlemen, I have been requested by several members of this Society to perform a very agreeable duty to night, one, however, which I would willingly have devolved on some other member—not certainly from any disinclination, but because I feel that I do not possess those oratorical powers which would enable me to do full justice to the subject. You are all aware that within the last few days, the Queen has been pleased to confer the rank of Baronet on our distinguished townsman and fellow-practitioner, the President of this Society. As to the propriety and justice of this step, I have not heard one dissentient voice among all with whom I have had any communication on the subject. On the contrary, both among the public and on the part of the press, it has been hailed as a just tribute to the distinguished merits of that highly gifted gentleman. Great, however, as the honour undoubtedly is, there are circumstances which materially enhance its value, and must make it doubly gratifying to Dr Simpson and to all his friends. In the first place, it was quite unexpected. I believe that, till Dr Simpson received the letter intimating the appointment, he had no idea that such a thing was in contemplation. Secondly, it was unsolicited. We know very well that such honours are conferred sometimes as the reward of political partisanship or support; that at other times memorials are got up stating reasons why so-and-so should be made a knight or a baronet. In this case there was no need for any such measures. Dr Simpson's life is his memorial—a memorial which has been accepted and honoured by Her Majesty in the most handsome way, the letter conveying the honour stating that it was conferred in recognition of Dr Simpson's high position as a medical practitioner, and especially for the benefit conferred on humanity by the discovery of chloroform, through the instrumentality of which not only had human suffering under severe operations been greatly alleviated, but even life had been saved, operations having been rendered possible which, without chloroform, could scarcely have been per-

formed with any hope of success. And, lastly, Dr Simpson was the first medical practitioner in Scotland upon whom such an honour had been conferred. We have long felt that sufficient acknowledgment has not been made of the services of the medical profession, either in its civil or public capacity, by the Government of the country,—we hope that a more enlightened policy has been inaugurated, and that this honour conferred on Dr Simpson is one of the first-fruits. So much for the honour itself. As for the Individual on whom it has been bestowed, it is not needful in Edinburgh, and especially in this Society, to say much. The Edinburgh Medical School has long stood pre-eminent as an Obstetric School, owing very much to the distinguished talents especially of the previous and present Professors of Midwifery in this University—the late Dr Hamilton, my respected teacher and friend, and my friend and fellow-student, the present Professor of Midwifery, the more distinguished of the two. For where is there a part of the civilized world where the name of Dr Simpson is not known; and from which patients have not come to place themselves under his care and to obtain the benefit of his advice? As to his talents, his unselfish nature, or his perfect willingness to assist in every way with his experienced counsel, I need say nothing here. In this Society we have constant opportunities of receiving instruction from his own lips; and I am satisfied you will all agree with me that we have benefited very much, have become better practitioners, and been enabled to be more useful to our patients, in consequence of the instruction we have received from him in this place. I am sure, therefore, that we are all highly gratified with the recognition his eminent talents and services have received at the hands of Her Majesty, and that we feel that, in this honour conferred on him, honour is also conferred upon ourselves. I know I am only expressing the feelings of every member of this Society when, in your name, I offer Sir James Young Simpson our congratulations upon the high and distinguished honour conferred upon him, and our earnest wishes that he may be spared yet many years to enjoy it, and—what he values still more—to advance the knowledge, the interests, and the usefulness of our profession; and that the dignity may descend through a long posterity.

Sir James Y. Simpson—Gentlemen, I must say, I feel very much taken by surprise by this movement of yours. I wish only that I had had some inkling that such a procedure was intended, so that I might have thought what I could adequately say, in acknowledgment of the very kind terms in which my friend Dr Moir has spoken of me, and the very kind manner in which you have received his remarks. When I received the letter to which Dr Moir has alluded, and was reading it over, I at first simply wondered why Earl Russell—whose handwriting I knew—wrote to me in flattering terms of the advancement of medicine and surgery, and the benefits accruing from the use of chloroform; and when at last I came to the gracious offer from her Majesty, with which the letter concluded, no one could have been more astonished than I was. My first consideration was whether I was entitled to accept it; and for a time I felt startled and almost ashamed that I should be selected as the medium through which a high and royal honour was to be conferred on the medical profession in Scotland. But after debating the matter with myself and others, I came to the conclusion that on the present occasion it was my duty to accept, and gratefully to accept, the honour which Her Majesty was pleased to bestow upon me, however much that honour exceeded and surpassed altogether any deserts of mine. There have occurred subsequent circumstances which have very much enhanced to me this complimentary mark of Her Majesty's approbation. In particular, I have been very specially rejoiced in finding that you, my fellow-practitioners here, and others throughout the country from whom I have received many communications, should have felt such sincere gratification that I, as their representative, have been honoured in this way. The very cordial ratification of the step, by all classes of my fellow-citizens, has also added greatly to its value in my eyes. Then I have an elder brother, who has ever been a father to me, who is a hundredfold prouder of the distinction than I am myself;

and, I confess, that one of my principal pleasures in it has consisted in witnessing the deep and quiet joy which it has given to him. Perhaps I may, myself, be excused for looking on the honour as rendered greater not only by the fact stated by Dr Moir, that it is the first time it has been conferred upon a medical man practising in Scotland, but because it is the first time also, I believe, it has ever been bestowed upon a Professor in a Scottish University. I hope, however, neither this nor any other circumstance will, in the least degree, trammel me in trying to assist at the meetings of this Society, or in any other way seeking the advancement of midwifery, medicine, or surgery, and promoting the best and highest interests of our common profession. There are, I believe, six living medical baronets: two of them pure physicians, two of them surgeons, and I think we, the Fellows of this Obstetric Society, have reason to be satisfied that there are now two others of them obstetric practitioners. From Sir Charles Locock, the acknowledged head of our own department of the profession, I have had the gratification of receiving a warm letter of congratulation, which I esteem very highly, because I esteem the writer so very highly. And from the bottom of my heart I beg to return you, my colleagues and brother practitioners, my most earnest and fervent thanks for your congratulations and good wishes.

I. CASE OF CÆSAREAN SECTION.

Sir J. Y. Simpson described the following case:—

Mrs —, an Irishwoman, twenty-four years of age, had a child thirteen months before this last labour. That child was premature, and presented naturally. It was extracted piecemeal after she had been three days in labour. Three doctors were with her then, and told her that if she should ever become pregnant again she must have labour brought on at the end of the sixth month.

On Monday, 9th October 1865—her full time having arrived—the liquor amnii came away. On Tuesday, Wednesday, and Thursday of the same week, she felt occasional slight pains, which had scarcely any effect on the os uteri. On Thursday evening, Dr Peacock dilated the os uteri with the fingers to nearly the full size. No part of the child could be felt till the dilatation was nearly complete; and then Dr Peacock touched a soft body high up above the brim. During the Thursday night an arm came down into the vagina. Dr Peacock and Dr Balfour, of George Square, tried to turn the child after the arm came down, but found it impossible, as the brim was too small to allow them to pass the hand by the side of the protruded arm.

I saw her about ten o'clock on Friday morning. The arm was swelled and protruding from the vagina. The pulse was very quick and weak, and the patient much exhausted. On making a vaginal examination, I found the promontory of the sacrum projecting greatly forwards, like the size of a closed fist. I found, further, that the posterior lip of the uterus was deeply fissured, and rent in a transverse direction; and I feared at first that the lacerations extended through all the tissues of the part into the cavity of the peritoneum, as the projecting sacral promontory could be felt through it, as if only a thin layer of tissue intervened. On carefully, however, and cautiously examining the bottom of the rent with the finger, I could not trace it anywhere actually passing into the peritoneal cavity. I attempted, as Dr Peacock and Dr Balfour had done, to pass my hand into the uterus, but the effort to do so proved quite fruitless. The patient's house was some miles in the country; and after sending into town for a set of obstetric instruments, I made several useless efforts to reach the neck of the child with the blunt hook. I then tried, equally unsuccessfully, to slip up the cephalotribe, with the view of compressing and breaking up the chest and spine of the infant. Finding all these attempts to fail utterly, I then thought that the poor patient's only chance—and that an exceedingly slight chance—was the removal of the infant by the Cæsarean section. But by the time some preliminary arrangements were made with this object, the woman had greatly sunk in strength, and her pulse was scarcely perceptible. Indeed it was doubtful to Dr Peacock, Dr Black, my son, and

myself, whether it was perceptible at all. The patient was chloroformed, the Cæsarean section easily performed, and the placenta and child removed. The placenta was affixed to the anterior wall in the course of the line of incision. One drawback followed. The wound in the walls of the uterus continued to remain open, and pour out a considerable quantity of blood. To restrain this hæmorrhage, I was obliged to put two or three stitches of iron wire into the walls of the uterus itself, which seemed at once to restrain the dangerous effusion of blood. Before sewing up the abdominal wound with iron wire, the peritoneal cavity was cleared of blood, and, by the hand passed behind the uterus, it was ascertained that the rupture felt in the uterus had not passed at any point into the peritoneal cavity.

When the patient recovered from her chloroform sleep, we were all pleased to find that her pulse was now perceptible, and capable of being counted,—much stronger, in short, than it had been before the operation; and she expressed herself gratefully for the relief given to her. She was placed upon small doses of opium. In the evening, Dr Peacock found her pulse about 125, and of moderate strength. She had then no abdominal pain. Next morning some vomiting supervened, and the efforts connected with it displaced two of the external stitches. During this—the second day after the operation—the patient continued wondrously well, her pulse being found by Dr Peacock as low as 115. On the third day, at noon, I found the pulse at about 110, and the patient looking and feeling marvellously well. But in the course of the afternoon the heart's action increased much in rapidity. Great tympanitis supervened, and she sank and died on the following or fourth morning, about nine o'clock, or sixty-seven hours after the operation. Unfortunately, all efforts to get a post-mortem examination of the body were unsuccessful.

In addition, *Sir J. Y. Simpson* remarked, that in this case the Cæsarean section seemed the only practicable mode of delivery, if the patient were not to be allowed to die undelivered; and if she had been operated on sooner she might have had a chance of recovery. Besides the frightfully exhausted state of the patient, the position of the placenta was here a great drawback to the operation, and was probably the cause of the subsequent bleeding; for hæmorrhage from the sides of the wound in the uterus does not occur frequently in such cases of Cæsarean section. In this instance the oozing was along the whole cut surface, the edges of the wound being turned over or outwards. The pelvis may have been about two and a-half inches in size, if so much.

Dr Charles Bell said that malpresentations were more frequent in premature cases, and he was satisfied that one great cause of mortality in the Cæsarean section was the delay in performing it, by which the constitution became exhausted, and unable to bear the shock of the operation.

Dr Andrew Inglis said that Cæsarean section used to be practised in cases of arm presentations many years ago.

Sir J. Y. Simpson, in allusion to the question of the state of the patient at the time of operation, as bearing upon the chances of her recovery, referred to the case of a woman who was once ripped up by the horn of a cow in the Queen's Park, Edinburgh. She recovered from this rough and unprofessional form of Cæsarean section probably from not having been previously exhausted and worn out.

Dr Keiller here mentioned, in regard to the internal use of the wire ligature, the case of a woman he had to-day seen, who had been vomiting stercoraceous matter for some days, but had not made him (*Dr K.*) aware of a swelling in the groin. On examination, *Dr K.* discovered a strangulated hernia. She was removed to the hospital, where *Mr Spence* operated. He passed a wire suture through the bowel, in the hope of forming a fistula.

With regard to the case of Cæsarean section alluded to by the President, *Dr K.* thought that the hæmorrhage must have been caused by the position of the placenta giving an unusual vascularity to the anterior or cut wall of the uterus. If the placenta be implanted on or near the posterior wall, there is comparatively little danger from hæmorrhage, and hence the advantage of

ascertaining, by means of the stethoscope, the site of the placenta previous to incising the uterus. The amount of hæmorrhage was generally found, as stated by the president, to be wonderfully slight in Cæsarean section of the uterus.

Dr Main stated that he knew of a woman who had a swelling (hernia) opened by a medical man for an abscess,—fæces were discharged; the wound, however, healed, and the patient recovered.

An interesting conversation followed on the advantages of several forms of uterine pessaries.

Sir J. Y. Simpson then brought under the notice of the members a recent case of anencephaly combined with spina bifida.

SESSION XXV.—MEETING V.

24th January 1866.—*Dr BURN, Vice-President*, in the Chair.

I. CASE OF PLACENTA PRÆVIA. BY *DR CAIRNS*.

The patient in whom this serious complication occurred is about 34 years of age, and the mother of eight children, including the one last born. She aborted once. All her labours, with the exception of the last, were natural and of short duration. She announced to me the fact of her last pregnancy about the second month, and in doing so burst into tears,—saying, “that she was certain something unusual was going to happen, because she felt altogether different from what she had done on former occasions.” She went on, however, quite favourably to the seventh month, with the exception of feeling considerable uneasiness from an eruption on the labia, which happened about the fifth month, and of which I was apprized by her husband, who requested me to prescribe something to allay the irritation from which she was suffering. On informing him that I could not do so without first ascertaining the nature of the eruption by making an examination, he replied that “he would do his utmost to prevail upon her to allow an examination to be made,” but added, that “he was quite sure she would not give her consent.” He was right. She would not give her consent, and the eruption was accordingly allowed to take its course. Well, about the seventh month, as I have said, on happening to call one day, she stated that a considerable quantity of blood had escaped by the front-birth on the day previous. I at once divined placenta prævia as the cause; but remembering that she would not submit to an ocular examination a short time before, I abstained from proposing a tactile one, and merely enjoined her to maintain a recumbent posture to the utmost of her power; and, in order to secure the co-operation and influence of her husband, I explained to him privately the serious nature of the case which I suspected; but, being a person of very active habits, she persisted to the last in actively superintending her ordinary household duties. From the first occurrence of the hæmorrhage I made it a point to see her as frequently as possible, and to urge upon her the absolute necessity of laying herself up. All my entreaties, however, were of no avail, even although she acknowledged, on my making inquiry, that the hæmorrhage repeatedly recurred, though, as she said, in trifling quantity. Nay, she even had the hardihood to take a long drive in a cab only two days before her confinement, which event took place in the seventh month and a-half of her pregnancy, and which was heralded by the sudden escape of the liquor amnii during the night, followed by copious hæmorrhage. When this event happened I was immediately summoned. On this occasion no objection, of course, could be made to an examination, which was accordingly done, when I found several coagula of blood in the vagina, and a soft spongy mass surrounding the entire os uteri. As yet, no pains had occurred, except mere “twinges,” as the patient expressed it, and which had no influence on the os. I plugged the vagina, and on finding that no hæmorrhage of any consequence recurred, after waiting some time, I then left, thinking that, as the membranes had ruptured, true labour-pains would supervene in the course of a few hours. After waiting a whole day, however, they did not do

so, and the hæmorrhage having returned, and feeling that I could not manage the case in justice to both mother and child without assistance, I called in the aid of Sir J. Y. Simpson, who ordered a tight abdominal bandage to be applied, and a drachm of the ergot of rye to be administered every half-hour. These measures had the effect of soon superinducing good labour-pains, and, after the lapse of two hours, the os was sufficiently dilated to admit of version, which he performed, and delivered the mother of a male child, which exhibited very faint signs of life; but after persevering in the application of the usual remedies for about twenty minutes, I succeeded in fully establishing the respiration, Sir James meanwhile devoting his attention to the mother. The latter, though quite pulseless and almost exsanguine after delivery from loss of blood, made a good recovery, and the child, which is now four months old, has, up to this date continued to thrive remarkably well.

Dr Cochrane thought that the hæmorrhage was allowed to continue too long before there was any interference.

Dr Burn considered it excellent practice in such cases to plug the vagina both before and after delivery.

Dr Keiller was in favour of plugging the vagina till the os had dilated to permit of turning. In *Dr Cairns'* case he thought the placenta had been only partially over the os.

Dr Pattison mentioned a case to the Society where the vagina was plugged after rupture of the membranes, and which did well.

Dr Keiller suggested to *Dr Charles Bell* that the plug might be removed from time to time, to examine the patient.

Dr Burn instanced a recent case where the plug did admirably.

Dr Keiller said that the plug should be removed if the patient showed any evidence of syncope, and the case carefully watched.

Dr Bryce stated that he had seen only two cases of placenta prævia—one was complete and morbidly adherent—mother did well. The other case was a partial one, with considerable hæmorrhage. *Dr B.* returned the detached portion, the pressure of the child's head completely arrested the bleeding, and the child was born alive.

Dr Keiller said, when the head came down it acted as a plug in preventing hæmorrhage. He thought that *Barnes'* practice of separating part of the placenta was a good one in some cases; generally the hæmorrhage ceased.

Dr Thomas Balfour owned that he felt a difficulty in accounting for the result of *Barnes'* treatment; he was aware that if we separated the whole placenta the hæmorrhage must cease.

Dr Keiller suggested that it might be accounted for by the contraction of the fibres of the uterus.

Dr Burn would expect blood to escape from the open vessels.

Dr Keiller said that clots gathered and plugged the vessels.

Dr Murray mentioned a case of placenta prævia which he had seen at the seventh month, where he ruptured the membranes. When the os was considerably dilated, the head came down, and twins were still-born.

Dr Keiller thought that the grand aim in such cases was not to delay, get the os dilated, and turn the child, with which remarks *Dr Charles Bell* agreed.

Dr Burn said he had seen a case lately where the placenta was expelled three hours before delivery.

Dr Charles Bell said there were cases recorded in which the placenta had been expelled first, and the child born alive.

Dr Keiller spoke of a case he had seen when in Dundee, along with *Dr Nimmo*, where the placenta was expelled, and the child born alive.

Dr Cochrane had seen one case where the placenta was first expelled, but the child was dead.

Dr Charles Bell suggested the placing of the placenta in hot milk.

II. UMBILICAL HERNIA.

Dr Stephenson remarked that this affection had rarely if ever proved of

serious import in children. He had never heard of a case where it had become strangulated, but occasionally the tumour does become distended and causes uneasiness to the child. Although not dangerous, it was not to be neglected. In the treatment, he was of opinion that bandages, whether elastic or not, with or without pads, were of little use. The method which he was in the habit of adopting was a simple cork-pad, cut and adapted to each case, placed in the centre of a piece of leather, and the whole spread with one of the ordinary plasters. This form of support he had seen worn for two months without renewal, and he had never seen it cause any uneasiness or irritation. Its advantages were simplicity and cheapness, can be worn constantly, even when the child was bathed every day, and does not require to be renewed so frequently as the similar method proposed by M. Guersant, where only diachylon plaster was used.

Dr Keiller considered a plaster apt to prevent the contraction of the umbilical opening.

Dr Charles Bell was in the habit of using a nutmeg cut in two, and placed in a plaster.

Dr McCowan related a case he had recently seen of strangulated hernia, where four inches of omentum had passed the ring.

Dr Cochran recommended a graduated truss as the best remedy in this affection.

Dr Keiller had been in the habit of using straps of adhesive plaster, which generally proved sufficient without any pad.

Dr Wilson agreed with *Dr Keiller*, especially if it was a case soon after birth.

FROM ABROAD.—THE STOMATOSCOPE—RESPIRATORY THERAPEUTICS—POPULAR LECTURES ON MEDICAL SUBJECTS.

A NEW instrument, to be termed the *stomatoscope*, was exhibited last week to the Paris Surgical Society by its inventor, Professor Bruns, of Breslau. A platinum spiral wire (enclosed in a boxwood cup, to prevent the transmission of heat), brought to a red heat by the passage of an electric current from two of Middelдорps' elements, is placed in the mouth behind the teeth. The light reflected by a very small mirror is sufficiently intense to render the jaw transparent, so as to allow of the vessels proceeding to the roots of the teeth, the smallest specks of caries, etc., etc., becoming visible. By reason of the transparency, even the labial coronary artery may in some subjects be seen at the level of the commissure, and its course followed. The instrument is therefore likely to form a useful means of exploration in dental affections.

M. Sales-Girons, encouraged, we suppose, by the favourable reception which his plan of administering pulverized liquids in a limited class of affections has met with, seems disposed, after the manner of over-sanguine inventors, to convert it into a hobby, to be ridden on all occasions. In a note accompanying a memoir sent into the Academy of Medicine, entitled "Respiratory Therapeutics," he attempts to generalize this plan of treatment, declaring that the bronchi, and not the stomach, are the proper channel for administering the principal medicinal substances. It is true he has only one solitary case to bring forward at present, but his object is to call attention to the subject.

"If there is a fact," he says, "which may be affirmed by the best proofs of modern physiology, it is certainly the preference which should be given to the respiratory passages over the digestive organs as the means of administering medicinal agents. Organic susceptibility, excellence of tissue, a position in the primary centre of hæmatosis, a special contiguity to the blood-globules, a superior aptitude for absorption, cleanness of surface,—all these advantages of the bronchial mucous membrane contrast with the opposite conditions of the gastric membrane as a means for the good administration of medicines. It is true that the respiratory passages do not present the facilities of introduction

and dosage enjoyed by the digestive canal; but, if a cure can be better operated through the former, the indication is furnished, and it will be attained by the means of art and practice. Until better contrivances can be devised, we offer the pulverization of liquids as a means of introducing medicinal substances into the bronchi, as all those of which we now have to treat are soluble in liquids. Until we have clinical proofs, I am aware that the best physiological warrants are but favourable presumptions; but, if these presumptions will induce clinical researches, my end will be answered, for, persuaded that sooner or later the recommendations of physiology will lead to favourable results, my ambition counts only in hastening these on. I have only one case at present—that of an obstinate intermittent fever treated and cured by a pulverized solution of quinine."

The popular lectures on scientific and literary subjects, which have so long formed a feature amongst our own educated classes, have recently taken root in Paris, and have spread through the chief towns with something like enthusiasm. As might be expected, many of these have been characterized by great ability, and their delivery is likely to be of utility. This last remark cannot be predicated, however, of a series now in course of delivery by M. Marchal de Calvi, who, we feel certain, will never find imitators in this country, at least amongst persons of the same stamp as himself. An accomplished orator, and holding himself a high position in the Paris Medical body, he has been addressing immense audiences of ladies and gentlemen in the *Salle Valentino*, upon medical topics, taking cholera, small-pox, vaccination, etc., for his subjects. How difficult such a task must be to accomplish with tact, delicacy, and efficiency before a mixed assemblage of this kind need not be explained, any more than the necessary incompleteness and uselessness of the results which must, after all, attend its execution. If this were all, we might pass the matter by as a mere foolish undertaking; but, when it is stated that the orator has condescended to flatter in place of combating the prejudices of his auditors, and amidst salvos of applause held up what he chose to consider the shortcomings of the profession to the gaze of a mass of persons totally unfit to entertain an opinion on the matter, one can only regret that the right of holding public assemblies in France, which has been so often withheld for so many useful objects, has been granted on this occasion for one so destitute of utility and so fraught with mischief.—*Medical Times and Gazette*.

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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On Amputation at the Knee.* By JAMES SYME, F.R.S.E., Surgeon in Ordinary to the Queen in Scotland, Professor of Clinical Surgery in the University of Edinburgh, etc.

ABOUT two years ago, Mr Carden of Worcester communicated a method of performing amputation at the knee, which he had employed long and extensively with great success, but which was rather coldly received, probably from being at variance with preconceived ideas on the subject. So long as a good muscular covering of the bone was deemed essential for constituting comfortable stumps for the thigh, the proposal of a single integumental flap naturally excited an unfavourable feeling; and as there is reason to believe that it still continues to render the operation unpopular, instead of being regarded as one of the greatest improvements in modern surgical practice, I beg to offer the following testimony in its favour.

When I began to amputate at the ankle, and found the great advantage of dividing the bone through its cancellated texture, it naturally occurred that the same consideration was applicable to the knee, and that, when circumstances permitted, amputation should be performed here rather than through the thigh, with its dense shaft and medullary texture. But, unfortunately, not being then aware of Mr Carden's plan, I formed a covering for the bone by cutting it from the calf of the leg, which proved very inconvenient, and so counterbalanced the benefit anticipated, that this operation soon fell into disuse. Mr Carden, pursuing quite an opposite course, made a semi-lunar incision in front, from side to side, with its convexity nearly over the tuberosity of the tibia, and reflected the flap of skin thus formed, so as to expose the muscles above the patella, where what remained of the limb was divided transversely. The popliteal artery, and any of the small branches that required ligature having been tied, the ample covering of integument was brought down to its place, where, being secured by sutures, it lay without any tendency to retraction, or requiring the restraint of bandages, while the dependant opening afforded a free vent for the

discharge of matter. No trouble was experienced in the after-treatment, and the stump proved eminently serviceable, since the skin over the bone, instead of becoming thinner, acquired additional thickness, so that the patients could rest upon it just as they do after amputation at the ankle.

But the advantages of this operation are not limited to its facility and satisfactory results in the event of recovery, since its great claim to respect and confidence is the safety that attends its performance. This I believe mainly depends upon the dense bone and medullary texture not being concerned; but, however explained, certainly presents a most remarkable contrast to the danger which attends amputation of the thigh. Professor Lister of Glasgow, who, I believe, has the merit of having introduced Mr Carden's operation into Scotland, drew my attention to this most important circumstance, and in a letter which I had from him lately, with reference to five cases in which he had operated, says, "In three, I believe, no other mode of amputation could have saved them, and two, I have not the least doubt, must have died had the thigh been amputated."

Mr Lister's experience, and a conversation which I subsequently had with Mr Carden on the subject, induced me to resolve that the first opportunity which presented itself should be taken to perform the operation. While thus inclined, I was requested, on the 19th of September last, by Mr Annandale, in absence of Dr Gillespie, to see a patient in the hospital who had been admitted with both of his legs completely shattered by a large mass of iron falling upon them. It was obvious that he must die if the limbs were retained, and no less so that amputation of both thighs would in all probability prove fatal. I therefore suggested that Mr Carden's operation might be performed, which was accordingly done by Mr Annandale with the most satisfactory result, as will appear from the accompanying photographic representation of the patient, who was 53 years of age.

Soon after this, on the 23d of October, I was requested by Dr Mackenzie of Kelso to see a young farmer, whose life was in great danger. It appeared that while on horseback, during the race-week, he had been struck by the shaft of a cart in the crowded street with such violence as to cause a fracture of his leg. There was no wound, but the limb suddenly swelled and became cold, with dark discoloration. Inflammatory symptoms succeeded, with corresponding constitutional disturbance, and on the fourth day it was generally supposed that the case must prove fatal from spreading gangrene. But Dr Mackenzie thought that amputation might still afford a chance of escape; and although the prostration was extreme, with a pulse hardly to be felt, so that cutting through the thigh must have been almost certainly and speedily fatal, I proposed to operate at the knee, and did so without delay, when it appeared that the posterior tibial artery had been ruptured at the

seat of injury. The patient was no sooner relieved from the mortified limb than he began to improve, and, through careful nursing, made a good recovery with an excellent stump.



On the 4th of November, Dr Hislop of North Berwick requested me to see a clergyman who had been confined to bed for more than twelve months, by disease of the knee-joint, with no prospect of improvement, and constantly-increasing weakness. It seemed that skin and bone naturally predominated in the constitution of his frame, and that from long-continued exhaustion little else of it remained. I should therefore have regarded amputation of the thigh as a most unpromising procedure, but with my recently-acquired faith in the knee-operation, felt no hesitation in performing it. Everything went on favourably afterwards, and the reverend gentleman speedily regained his health, with a comfortable stump.

On the 1st of January, W. M., aged 32, was admitted into the hospital on account of a compound fracture which his leg had sustained on the railway. An attempt was made to save the limb, apparently for a time with some prospect of success; but on the 9th, from the amount of discharge and the extent of shattering which was revealed by the ulceration and sloughing that had taken place, it became obvious that amputation afforded the only chance

of escape. I therefore operated at the knee; and although the circumstances were most unfavourable for recovery, I had the pleasure of seeing the patient gradually improve in health, with the prospect of a good stump.

A boy, *æt.* 6, was admitted into hospital on the 20th of January, with mortification of the foot, from a railway injury, and threatening of gangrene extending up the leg. I amputated at the ankle in the hope of arresting the mischief in progress, but with only partial success, since inflammation affected the periosteum, so as to cause necrosis of the whole tibia, and establish profusely discharging sinuses above as well as below the knee. The patient then became so extremely weak that his case appeared to be hopeless, and would, I believe, have proved to be so had we not possessed a milder alternative than amputation of the thigh. I removed the limb at the knee on the 14th of this month, and the operation was followed, instead of sinking, by such an improvement of condition as encourages us to look for the most satisfactory result.

Soon after his double amputation at the knee, Mr Annandale had a patient, in private, suffering from disease of the knee-joint, who was so exhausted by hectic, bed-sores, and profuse discharge, that amputation of the thigh seemed to afford no prospect of recovery. He therefore amputated at the knee on the 25th of October, with the almost-unlooked-for result of complete recovery.

Dr Joseph Bell, who takes charge of the surgical clinical wards in my absence, admitted a patient on the 26th of January, who had suffered a compound fracture of both legs on the railway then in progress of construction at Queensferry, whence he had been brought all the ten miles in a cart. One limb was shattered beyond the possibility of recovery, the other being less seriously injured. The former was amputated at the knee, and the latter so successfully treated that the man is now able to walk on it with the assistance of crutches.

From what has been said, I trust it will appear,—

1st, That Mr Carden's operation is less dangerous to life than amputation of the thigh.

2d, That the execution, ligature of vessels, and after-treatment, are simple and easy.

3d, That the resulting stump is comfortable and serviceable.

These considerations will, I trust, meet with due attention, and tend to promote the adoption of a procedure destined, I feel assured, to supersede amputation of the thigh, which, notwithstanding all the attempts to improve it, has so long remained an opprobrium of surgery.

ARTICLE II.—*The Action of Fungi in the Production of Disease.* By
TILBURY FOX, M.D. Lond.

SINCE the publication of my work on Vegetable Parasitic Diseases of the Skin, my attention has been assiduously devoted to the acquirement of new facts; and as the action of fungi in the production of disease calls loudly for examination, but is shunned by almost every observer, I trust I may be doing some service in stating the results of my own further observations and experiments. Vegetable parasitism is a subject whose study demands a great deal of patience, and the sacrifice of a good amount of time; it also necessitates a cautious regard to facts of a botanical and chemical nature, but holds out the promise of important results; it includes not merely the direct action of fungi upon man, but the as yet but little explored field of the intertransference of diseases of men, plants, and animals; that of ergotism or the influence of foods infested by minute vegetable organisms; the possible fact of the conveyance of poisons from person to person by the fungus elements, and the actual aiding of special poison production. The novelties in these and other similar matters are not of greater extent than their importance. Indeed, it is a conviction in the minds of many who have thought much upon the subject, that a not far distant time will see some of the greatest generalizations that medicine has as yet seen in connexion to this and kindred subjects.

Those who have carefully watched the matter during the past years are now confident that the time has arrived when a very fair and correct sketch may be given of this question, about which laudable but too ample differences have obtained; and, indeed, that many of the knotty points which have been made subjects of hot debate and contest, may receive a rational explanation acceptable to all. I would ask that the remarks about to be made may be regarded as constituting an appendix, as it were, to my original work.

There are three chief lines of investigation which especially deserve research, and these may be represented by the three following queries:—

1. Are the so-called parasites vegetable or not in nature?
2. What is the limit of variation in each kind of fungus? Are not most of the forms usually considered distinct, but varieties of a very few, perhaps one, species?
3. If vegetable, are fungi "accidentals" in disease, or do they necessarily directly or indirectly cause any special morbid changes; if so, of what nature are these?

To settle such questions as these requires considerable observation, and that not in a limited area: analogical evidence, derived from a search amongst facts, presented by the phenomena and behaviour of the healthy and diseased states and doings of plants and animals,

is most valuable and most *corrective* in its application. This I have endeavoured to procure.

With regard to the first point, the *vegetable* or *non-vegetable* nature of parasites. It is admitted on all sides, with scarcely one single exception, that the fungi met with browsing upon and within our surface are really of vegetable nature, that the germs are derived from the exterior, and that there is no such thing as some have indeed imagined, viz., a conversion of the elementary particles of the animal structures by a process of degeneration into those of vegetable nature, may I say a heresy that would uproot every sound principle of physiology, an ultra-polymorphism which denies *in toto* the existence of any law of limitation, the interdependence of life, and whose fair application legitimately carried to its fullest extent would arrive at a *reductio absurdam* the most amusing. There are most satisfactory reasons for rejecting the acceptance of such an hypothesis.

Firstly, The growth and independent life of the cell structures (fungi) when removed from the presence and influence of all living animal structures. I have over and over again made the elements of these cryptogams to vegetate freely in preparations put up for the purpose, and it has become a familiar experiment with me, although the failures—as one might expect—are in the great majority.

Secondly, The peculiar action of reagents, and especially liquor potassæ. As far as I know there is no animal structure that resists the action of this reagent in the same way that spores, sporules, and mycelia do; they remain practically unchanged, and do not swell up and become indistinct as is the case with the other structures. Iodine, again, detects the presence of the primordial utricle. Other behaviours might be mentioned, but I pass to—

Thirdly, The fact, of the presence of *identical* parasitic forms in the hard structures of animals, and indeed vegetables, where no (epithelial) cell structures of animal nature exist from whence the vegetable elements could spring. In bivalves, in corals, foraminifera, and a host of others, as shown by Müller, Claripede, Rose, Kölliker, and many more. The examples in the vegetable kingdom need not be detailed.

Fourthly, The want of transitional forms. Authorities rested the hypothesis of the “granular degeneration” mainly upon the supposed evidence of transitional forms. One writes, “We maintain that we have seen the cells of the rete mucosum passing through these stages of growth which have converted their nuclei into granules, the so-called sporules; we maintain that the granular condition is the normal fetal structure of the young epidermal cell, and that the morbid condition in question is an arrest of development of these cells at their fetal stage, and the cause of their consequent modification of destiny, no longer to rise through those higher stages of animalization which culminate in the production of

horn, but doomed in their crude condition to the lowest functions which belong to immature organic matter, viz., proliferation." I deny the existence of any transitional forms. It is easy to confound normal nuclei and sporules, but the arguments already made use of, and, especially the use of liquor potassæ, determine the difference. I believe it to be a very common mistake to confound nuclei and sporules. There is no further similarity, for directly the tubed phase (mycelial) commences all similarity goes at once. But in this particular point we will call to aid argument the—

Fifth, That the fungus elements are first visible at the upper part of the hair follicle, and migrate from above downwards towards the papilla and root; in other words, there is *a priori* evidence that the germs of the parasite are derived *ab externo*, and this is proved to be a fact by clinical observation. If the so-called fungus is a granular degeneration, such degeneration must commence where the nuclei are formed, and abundant in the early developmental stages, viz., at the formative point or papilla. Such is not the case. The earliest trace of spore, sporule, or mycelium is subepidermic and located just at the upper part of the follicle; from thence the parasite may be traced downwards to the bottom, whence it finds its way to the interior of the hair; and the effects of treatment tell the same tale. Remove or destroy every vestige of parasite, in the early stage particularly, and the disease is stopped. To affirm that this could alter such a thing as "granular degeneration" is not conceivable. Pluck out a diseased hair; if no spore is left behind, the hair is healthily formed at once. Besides there is no confirmation in the character of concomitants. The cells at the root of the hair are healthily formed; until the spores increase largely or specially invade the papilla there are no transitions. You have healthy structures in contrast with the fungus elements, until the formative apparatus is attacked. The mere plucking out of a hair could not alter the whole character of the nutrition as to bring back a disease (granular degeneration) abruptly into a state of health. The cause of the cell alteration is clearly not in the cell formation primarily, but due to some superadded influence which from without acts upon the cell nutrition.

The *variation* of fungi is a subject of intense interest at the present time. Observations crowd in upon us every day tending to overthrow the distinctions of species in most of the ordinary groups of cryptogams. The researches of Tulasne and De Bary especially have contributed to the establishment of the doctrine of polymorphism, which implies that one fungus may pass through a cycle of development, and in its different stages gave rise to many very different forms originally regarded as distinct species. Now, it is impossible to go into this matter very widely; and I therefore shall only adduce a few of the more striking examples of these forms of fungi which do not infest the human body, especially for the purpose of illustration. A striking example is afforded by the cases of uredinous plants. Mr

Berkeley experimented with "bunt" (*Tilletia*). He made the spores germinate; when they did so, they put out a species of stem or process, upon the which a second generation of thread-like spores was produced; these after a while joined together by transverse processes, in fact conjugated. The result was the production of a third generation, in turn giving rise to a fourth order. Bunt exhibits these four different kinds of fructification. Tulasne has also shown that the coniomycetous fungi have modes of reproduction by conidia, by stylospores, by spermatia (pycnidia), and asci. The pyrenomycetes have been shown to possess no less than six different kinds of form. De Bary's late researches into the history of *Puccinia graminis* (Neue Untersuchungen über Uredineen, insbesondere die Entwicklung der *Puccinia Graminis*, von A. De Bary, reprinted from Proceedings of the Berlin Academy, 1865; see also Ann. des Sc. Nat. xx. p. 1) have also shown that certain species of puccinia and uromyces exhibit five different sorts of reproductive organs: *spores*, teleutospores, as they are called, giving rise to a *promycelium*, upon which sporidia appear; these germinate and give rise to *æcidia* with spermagonia; the spores of the *æcidium* germinate, and, entering the stomata by small processes, give rise to a filamentous mycelium, upon which is produced the fifth form of the plant, viz., *uredo*; the *uredo* in turn produces teleutospores. This is the cycle observed, De Bary thinks, in all uredinous fungi. It occurs upon the same, or is spread over different plants. De Bary has shown also that the notion of the old relation of barberry rust and puccinia is quite a fact. It is quite certain that *uredo*, uromyces, puccinia, and *æcidium* are forms of the same fungus.

In reference to human parasites, I have already pointed out the various relations which exist between them. *Torula* is the fungus around which all the others group themselves; and I must refer to my work for details, the novel matters, concern puccinia, sarcina, and aspergillus more particularly. I am now fully convinced that no true puccinia occurs in the human subject. I have seen over and over again the appearance that I believe has been mistaken for true puccinia, but have never seen anything like the spores of the puccinia. I have found the same fungus which I figured in my work several times; recently in a case of the cast of the mucous membrane of the ear, sent me for examination by Mr Toynbee. The appearances had an exact resemblance to those figured by Dr Purser in the Dublin Quarterly Journal for November 1865, where the nail fungus is described as representing appearances identical with those of aspergillus, achorion, and puccinia. I conclude that the so-called puccinia is nothing more or less than a clavate termination of the mycelium of an oidium, or, what is the same thing, is penicillium. I am also quite sure that the leptomitous is nothing more or less than the basal condition of the oidial mycelium.

In the Botanische Zeitung, No. 2, for this year, will be found some very interesting observations upon the aspergillus. Sarcina

has always claimed my particular attention. It was in 1857 that I noticed a quaternate arrangement of spores, probably developed from the trichophyton, in a case of tinea tonsurans. In 1860 I produced the same by germinating the microsporon mentagrophytes; and in July 1860 began some experiments with penicillium, which gave me the same result, and indicated the relation of penicillium and sarcina. Sarcina, I stated, did not result from fissiparous division, but from a conjunction of spores, probably the nuclei of the torula cells, in their turn derived from the aerial spores of penicillium. Endogenous formation of nuclei occurring "as the result of the mixture or mutual influence of endochromes." From what I have seen since, I still believe such to be the true state of the case, but have never been able to get a satisfactory germination of sarcina, so as to produce any result that could be regarded as conclusive. The fructification of penicillium occurs in connexion with the development of sarcina, but it is difficult to connect the two things as stages. I am indebted to Dr Brinton, however, for some very important information, but it has only come to my knowledge within the last month. It appears that Dr Brinton and I were experimenting about the same time, and that he succeeded in getting a decided result, whereas I did not. Dr Brinton, in June 1860, watched the changes that took place in the sarcinae which were taken from a patient who came under his care. Dr Brinton assures me that he obtained the fructification of the penicillium by the development of the sarcina, and that he was enabled to watch the intermediate changes. The sarcinae became indistinct, endogenous formation of cells would appear to have taken place, and so far our experiments agree.

Further, the sarcinae sprout, develop a mycelium, and from this mycelium the penicillium fruit is produced. The figures given are those kindly placed at my disposal by Dr Brinton, and were taken from the microscopic objects at the time of observations in June 1860.

It is important to notice that the fact of the endogenous growth of cells inside the sarcinal masses militates strongly against the theory of fissiparous division. I think it will be found that there is some peculiarity of nitrogenized matter which conduces to the development of the sarcina. It would appear, however, that the latter is, under certain circumstances, able to produce penicillium; and one thing necessary to such a result is the production of endogenous cells. This mode of increase is seen in torula, conjoined to sprouting. In the stomach, fungi are subjected to the influence of both fluid and air. Growth in fluid conduces to endogenous growth, and the influence especially of the external air to the production of shoots or mycelial threads.

Suppose we take torula; what would seem to be its changes? If it grow in fluids without much air, the formation of brood cells and the discharge of nuclei, which may enlarge and produce cells like

the originals, or conjoin and give rise to *sarcina*, all depends upon the nature of the fluid medium. If the influence of the air be slight, torula tends to take on the mycelial form, and *leptomitus* is produced, when the filaments reach the air, oidium. If the influence of air be in excess



Penicillium from *Sarcina*. (About 350 diameters.)

and that of the fluid slight, then penicillium results from the fructification of the nuclei of the torula cells, and the free aerial spores of the penicillium reproduce torula again. Now, suppose the influence of air be brought to bear upon the early stages of these forms, to a certain extent a modification ensues, and thus *sarcina* may give rise to penicillium it is supposed. Such is an indication of the line of research opened out to inquirers. If I must give my own view, it would be one which regards torula, penicillium, and *sarcina* as forms of one and the same fungus,—oidium and *leptomitus* being included in the meaning of torula.

There is also a generally received impression that, in regard to the ordinary cryptogams of the surface, little importance is to be attached to the supposed difference of form, size, and shape, as indicative of distinct species. We have only to glance at the facts of botanical knowledge which have recently been made known, to show us how utterly futile is the attempt to make fructification even a differential test. I have always contended that all the forms of trichophyton and achorion are one and the same in nature, and consider this matter proved. I have now observed in the ordinary ringworm of the surface, the trichophyton assume the features of the *aspergillus* more than once, or at least answer to the description given

by authorities) of the latter fungus. The other experiments I have made entirely confirm Dr Lowe's observations as to the identity of achorion and aspergillus. The chief reason for refusing to admit the identity of parasitic fungi of man is based upon the difficulty in securing an interchange between the several varieties: this shows a want of knowledge in regard to the subject in hand; each cryptogam holds its own, so to speak, and the inability to produce the one from the other is no just ground, *per se*, for denying their identity. The variety A implies the existence of certain peculiar conditions which you cannot at will produce, and these conditions will not suit the growth of B (a variety of A), nor produce a modified result, such as would make one variety change into the other. This is seen in the uredines very plainly; therefore, the fact that achorion grows as achorion on an herpetic subject, and continues to preserve its own characters, without assuming the aspect of trichophyton, is really to be expected.

But there is something more herein contained that deserves attention. The most recent researches into fermentation clearly teach that *varieties of the same fungus* tend to give rise to different results, well shown by Pasteur in reference to acetic fermentation. When the mycoderm gets into an advanced stage of development, it gives rise by its growth not only to the production of acetic acid but various suffocating compounds which the chemist knows will result from the oxidation of ether and alcohol by platinum black. The application of this principle teaches us the necessity of finding out what is the peculiar state of soil adapted to the trichophyton, the favus, or other forms, just as we should discover the chemical properties of different kinds of fermenting fluids. Take the case of sarcina, there is some very special quality of fluid which is suited to the development of this plant from the penicillium spores; if we could discover this we should get most likely a clue to that diseased condition upon which all the symptoms in connexion with dilatation of the stomach, pyrosis, and sarcinal development depend. The fact of identity, in kind, implies to a certain extent similarity of principle, as regards therapeutical treatment; but it is more important to look upon *varieties of fungus* as indices to so many different conditions of the nutrition of the patient. This is what we entirely neglect. In the British and Foreign Medico-Chirurgical Review, April 1864, a reviewer writes that, "after all, the question is one which concerns the botanist more than the medical man, be the fungi one or many it is the same thing to the practitioner." To admit such a statement as true, is, I believe, to effectually bar out the most important consideration in the whole subject of parasitic diseases. It is contradicted by every fact which has come to light within the most recent period. It will be well, if in the future we attempt to work out the peculiarities of habit, of nutrition, and other concomitants allied particularly with each separate *phase* of fungus. That is to say, pay a little more attention

to the state of the patient, without, however, neglecting the action of the fungus itself, which we shall notice presently.

It may be as well to notice that the earlier the life of the fungus, the more simple is its structure, the more liable is it to change in form, and the more certain is its so-called catalytic action. Botanists express it by asserting that the fungus is most active in its spermatiferous state. This illustrates the importance of attending to the nuclear form of parasites, to which I have always called special attention, but which has been almost disregarded by observers. It is very important in a therapeutical and diagnostic point of view. I would sooner have a hair infiltrated with large spores, than minute sporules; because I recognise in the latter case a potential activity which is absent in the former, a power of growth and reproduction peculiarly active. Many a time have I seen in an apparently healthy hair a little collection of minute sporules, which, as the hair grows, are carried up into the shaft, presently developing and bursting the hair, splitting it up into its separate fibres, and giving rise to bending and fracture.

If I were asked to sum up briefly the result of the most recent minute observation, I would say that the parasitic fungi found upon the human surface are varieties which cluster around the penicillium or torula in their sporular state as the type; that in the air the tendency is to the production of the aspergillus form of fructification; that the parasites of the mucous membranes, or entophytes, are more or less forms of oidium, mycelial forms of the sporous state of penicillium; that sarcina is a conjugated form of the latter. *Chionypte Carteri*, the fungus of the madura foot, being nothing more or less than a higher stage of the oidium, approaching closely the genus mucor. I do not comprehend the distinction of aspergillus, penicillium, and mucor as seen on the human surface, having noted all the appearances supposed to be characteristic of each, in the development of the same fungus penicillium taken from the contents of the stomach of a pyrotic patient. Human parasitic cryptogams furnish capital exemplification of polymorphism; our duty is plain: to investigate and define the special diseased conditions which are related to and indicated by special phases of fungus.

The mode of entry of the Fungus into the System.—There is no difficulty in accounting for the access of germs to living bodies, for these germs are freely distributed and disseminated in the air. The best illustration of this fact may be noted in the experiments of M. Bazin (*Gazette Méd. de Paris*, July 30, 1864), which consisted in passing currents of air over the head of a favus patient, and thence over the open mouth of a jar containing ice. The ice cooled the air, causing the deposition of moisture, in the drops of which the achorion sporules were detected. The same thing may be shown by holding a moistened glass slip near the head of a patient, and just rubbing his scalp freely. Of course, actual contact is much more effectual in the implantation of germs. But, without delay, let us suppose that the sporular elements find their way to the

human surface; how get they deeply into the tissues? In various ways probably. Let us take a general sketch. The greater the degree of moisture and heat, the better is the chance of entrée.

First of all, the fungus elements may enter by fissures or natural orifices; for example, in ordinary ringworm the sporules lodge themselves at the opening of the hair follicles, and presently get beneath the epithelial scales. We shall see, directly, how. A great many experiments have been made at different times, upon this point, in the case of plants. De Bary (*Die gegenwartig herrschende Kartoffelkrankheit, ihre ursache und ihre Verhütung*. Leipsic, 1861), found that, in terminal filaments of potato mould, so-called zoospores were formed, which bud, protrude filaments forming a mycelium which has the power "of penetrating the cellular tissue in twelve hours, and when established there it bursts through the stomata of the leaves." This "boring" operation is quite likely to occur, especially where the structures are diseased; as, for example, the muscardine in silkworm, in diseased mucous surfaces or epithelial changes: here the entrance by continuity is easily accomplished by the growing filament. It has been supposed that mycelium may get within the shaft of the hair in some part of its course, in this way. I do not believe it. If a fungus finds an entrance, it is either through a cut end, a distinct fracture, or, what is usually the case, the soft growing root. A good deal of doubt has been expressed as to whether the spores could find their way into the interior of plants through the stomata. It seems pretty clear that the latter are not sufficiently large for the occurrence. There can be no question, however, that, in a large number of instances, the spores send out little processes, which get into the plant through the stomata. Here the recent experiments of De Bary help us again. They are noticed by Mr Cooke in his admirable popular work on microscopic fungi. This observer took a large number of common garden cress plants, placed their roots in water containing zoospores, and though the former became covered with these latter bodies, yet not a jot of evidence of penetration occurred. De Bary, however, found that if the cotyledons or seed-leaves are watered with fluid containing zoospores, that slender tubes put out by the zoospores enter the stomata, the terminal ends enlarge, branch out, and become the centres from which a ramifying mycelium is produced, which presently shows itself externally. De Bary tested 105 plants in like manner and under similar circumstances, with water free from zoospores, and without the production of any sign of rust in these. De Bary concludes that plants are not infected by spores entering through the roots or leaves, but through the medium of the seed leaves of cotyledons. But it is probable that the fluid contents of the spore cells may be absorbed and give rise to disease. The Rev. Mr Berkeley, several years ago, found that the germs of bunt placed in contact with seeds, infected them, without there being any evidence to show that any spore or mycelial thread had effected an entrance. It

seemed as if the granular fluid contents were taken up by the plant and caused mischief. It is possible that minute threads might have penetrated the seeds nevertheless. There is, however, no difficulty in supposing the granular contents of spores (sporules) capable of reproducing the typical spore. But, in the next place, there cannot be a doubt but that in the human subject the germs of the fungus find their way to the roots of the hairs, and are carried bodily upwards into the shaft in the process of growth, developing as they go, till at last they degenerate and break up the fibrous structure in which they are. By analogy we should quite expect that such a thing is possible, and, indeed, of frequent occurrence in the case of the tender roots of plants; and this is more likely to happen when the contents of the original spore (which is as large as the spongiole cell) happen to be discharged by bursting. Moreover, it is quite clear that the germs of parasites enter at a much earlier period than we are apt to imagine, and lie dormant, brooding mischief till the favourable opportunity arrives. De Bary proved this in the case of the white rust (cystopus) which hibernates as it were in the sub-epidermal structures during the winter, till the spring arrives. In addition, the fungi "make head," so to speak, into structures in virtue of the chemical action which they set up. This is best seen in the hard structures of animals. Carbonic acid is given off at the terminal cells. This dissolves the lime of the shell and allows the parasite to effect an entrance most easily. The experiments of Wittich, quoted by Robin, all tell in the same direction. Panceri has come to the conclusion, however, that in the case of the egg, the minute germs effect an introduction through minute microscopic holes which exist in the shell. *Lastly*, traumatic lesions afford an easy channel for the conveyance of fungi to deep structures. This is what happens in the mycetoma or fungus foot of India.

We have then, as modes of entrance — (1.) That through natural orifices; (2.) That in which the growing force forces the mycelial thread beneath the layers of the superficial tissues; (3.) That in which processes shoot out from the spore and enter by such openings as stomata; (4.) That where the cell contents are absorbed; (5.) That in which the spores are carried bodily inwards by growing parts; or (6.) dissolve away the opposing structures by chemical action; or (7) enter by traumatic lesions. In each and every instance the germs of parasites are derived *ab eterno* and not generated *spontaneously*.

There are some special circumstances that deserve comment. It has been asserted that microscopic entophytes have been discovered in close cavities utterly cut off from communication with the external air. But these instances are open to grave objection; fungi have been found in the fluid of the ventricles of the brain, which, however, was allowed to stand all night exposed before it was examined. Again, it is asserted that in the kidneys the like has been found. This is

open to exactly the same objection. The case of the egg parasite has been explained away by Panceri; and it has yet to be shown, supposing the urine has ever during life contained fungus elements, that air cannot enter the bladder. The case of germs of vegetable nature in the blood current presents some difficulty; but even here the most considerable caution is needed. We know that fungi spring up with enormous rapidity; and it must be proved that those spores and mycelia are present at the moment of death, nay during life, before we can give credence to any theory which asserts that they have been present and introduced during life, and not by a communication with the external air. It is still a question whether the endosmotic action of the villi may not be able to account for the presence of cryptogams in the blood current. As far as the facts of vegetable parasitism go, we are bound to deny any such occurrence. And, upon analogical grounds, I venture to assert that the entozoa found in muscle, which have lately caused no little sensation, are not vegetable in nature. Should they be proved so, it will entirely alter the whole subject of vegetable parasitism; for we are justified at present in asserting that there is probably no known instance of a growing plant in any situation not in direct or possible communication with the air. I am bound to say that Dr Thudicum believes in the vegetable nature (see Report of Medical Officer of Privy Council for 1865) of the rinderpest entozoa (?).

I pass to the consideration of the part played by fungi in diseased states. Two theories the most opposite in intention have been held by writers and others, so opposed that really the conclusion is forced upon one that both *must* be wrong and a middle belief correct. Whilst one batch of inquirers affirms that parasites are accidental, another contends that they are the essential cause of those diseased conditions found in "association" with their growth. Ehrenberg, in speaking of organized parasites at a time when the exact nature of many of them was indistinctly recognised, said, "that there is more cause for wonder at the limitation of their effects by the actions of living bodies they inhabit, than at any morbid effects they appear actually to produce." It must first of all be noted that there are certain conditions which are peculiarly favourable to the growth of vegetable parasites. The latter are ubiquitous, capable of resisting the action of heat, cold, and decomposition, have a tremendous and rapid power of increase, and will remain for a very long time in a state of inactivity; yet, notwithstanding all this facility, there are certain states of organisms against which they fail; which will somehow resist their inroad and attacks; and it is now clear that though parasites may for the moment get a temporary hold, yet they will not flourish upon a typically healthy surface. This is a fundamental truism that must be observed in reference to therapeutics. For rusts and mildews prevail in direct ratio to the wetness of the season, or after drought, as in the pea or hop; damp itself is very favourable, and where there is much

drought the vigour and the circulation of plants are diminished very considerably. When plants are very ripe also, there is a less degree of vitality present, in consequence of the cessation in great extent of the circulation and vital connexion between the fruit and the stem. The same thing holds good in every instance where animals, plants, or men are attacked. We may instance the case of muscardine. The experiments of Claude Bernard also showed that frogs kept in captivity got out of order, and aphthous conditions arose. A healthy frog brought near its diseased fellows "set contagion at defiance," but unhealthy frogs were at once attacked by the vegetation flourishing on the aphthous surfaces of others; and the case of favus in man, or scab in sheep, of which an account may be seen in the Gardener's Chronicle for April 24, 1864, as illustrative of the fact under notice.

There is always a certain resistant power about all healthy living beings; and a certain amount of fungus, however it acts, may be present without giving rise to what one can possibly call disease. In young life, of course, one would expect that fungi would obtain a hold more effectually than in old life; and it is very remarkable that the white rust before referred to, according to De Bary's experiments, should effect an entrance into the system of the garden cress, by attacking the young leaves or cotyledons. The young and tender stage becomes an easy prey; and this is exactly what we find in the human subject, the young being most liable to ringworm.

Taking all things into consideration, it is clear that parasitic disease, or—as I have named it generically *tinea*,—cannot be explained by either of the conflicting theories I have referred to, but consists of three distinct components, which must be recognised, if the physician would cure his patient well and quickly.

1. A certain state of soil: in speaking of the polymorphism of fungi, I noticed that each fungus appeared to require each its special kind of pabulum.

2. The access of air, and the presence of heat and moisture,—the conditions necessary to support the life of fungus. And,

3. The introduction from without to and action upon the body of the vegetable germs.

The first and second will be passed over without comment: my remarks are specially intended to define the action of the parasite in the production of diseased states. Now fungi are not "accidental" and unimportant, but act in several distinct ways when once they take hold and grow upon the surface. This is important; if we insist upon some *one modus operandi*, we shall assuredly find our position utterly untenable. They act then (often in more than one way in the same instance be it remembered),—

Firstly, mechanically.—If you simply rub into the surface some of the fungus elements, in many cases you get what we know as irritation. This is seen in the ordinary herpetic ringworm of the

surface, where the mycelial threads range over the skin beneath the epidermis, and lead to erythema, etc. A very remarkable case is recorded by Dr Kennedy of Dublin, in which a quantity of flax powder was inhaled by a lad, who became attacked with measles and peculiarly severe local dyspnoeal symptoms, evidently dependant upon the direct mechanical irritation exerted by the fungus elements. In the case of mildew of plants the same thing is seen, the threads of the mycelium grow and force asunder the tender structures near it. Now, it is this mechanical action exerted by the growing force which is at work, especially in ringworm. The fungus finds its way to the sub-epidermal space, from thence to the hair follicle, irritating and interfering mechanically with the growing parts, enters into the hair, and by its increase and development simply splits up the hair shaft, appropriating also its juices, and rendering it all the more brittle, and therefore the more easily destructible. To declare in such a case that the parasite is accidental in any sense of the word is to turn a deaf ear to the plainest voice of facts; but this very action can be isolated. I have performed a good many experiments at different times with diseased hairs out of the body, and occasionally it is possible to get a hair containing spores, which spores will germinate and actually produce the splitting up of the hair, and the other changes that are observed in ringworm. In fact, *to produce the lesion of ringworm out of the body*. In those instances in which the mycelium abounds, the epithelium seems to suffer particularly. On the mucous surfaces there are no such structures as hairs which form a lodgement, so to speak, for the fungi, and hence no marked results are visible. The cells of the tissues are invaded and destroyed, the mycelial phase abounds and ramifies in the secretion, and not in the tissues themselves; but there is the same *capability* of damaging when parasites attack only the internal surfaces. If we would wish for examples of the enormity of the force exerted by a growing fungus we have only to confine some of the more ordinary varieties and see the result. Now, it so happens that no other agent can produce in disease the same kind of action as that exerted by a growing fungus,—such as splits up the hairs in the way in which this is observed in tinea; and it is this state of things which I regard as the pathognomonic lesion of ringworm, viz., the mechanical action of the parasite upon the hair and epithelium, in connexion with other minor changes.

One word as to definition. I use the word tinea as the generic term, and particularize each variety by the terms favosa, tonsurans, sycosis, vesicolor, circinata, etc. The tinea signifying especially the diseased state of the hair and epithelium. Now, take the case of sycosis, which means inflammation of the follicles of the chin and lips. It may not be caused by a parasite; but undoubtedly cases are sometimes caused by a fungus, and these I called tinea sycosis. Again, tinea circinata means the parasitic herpes circinatus, and

tinea decalvans the baldness produced by the fungus (*microsporon Audouini*), as distinguished from alopecia, non-parasitic baldness, the result of many different causes. The term tinea is very distinctive.

Secondly, Fungi act by inducing local chemical change.—They absorb oxygen and give out carbonic acid, and, as has been before observed, they hereby secure to themselves the power of penetrating calcareous structures. In addition, a large amount of gas is evolved, as in cases of sarcinal disease. Moreover, they lead to fatty degeneration. If any one will take the trouble to examine carefully some of the old stubs in favus, he will notice a certain amount of fatty changes going on in the cell structures. Remove a hair of this kind loaded with sporules, and get the latter to germinate, and the fatty alteration goes on at a rapid rate, till after a time a large quantity of crystalline fat is produced. Now, this will not happen unless the fungus germinate; but happening, it is worked out in accordance with the views lately put forth by Dr Bence Jones, and was expressed in precise terms in my book on parasitic diseases. It has been remarked by many observers that fat is always present in considerable amount in connexion with the development of fungi. M. Signol believed that fat very much favoured the development of bacteridia. Perhaps the very best exemplification of the association of fatty change with parasitism is that afforded by the case of the madura foot, where the oily matter is so very abundant. The tissues degenerate, and the crystalline fat is so varied and peculiar as to have actually misled observers into the belief that it was a form of fungus. Now, it becomes a question whether fat assists the development of fungi, or whether the latter attract fatty matter, the fungus forming a centre of attraction for crystallization, or the fatty change be the result of cryptogamic growth. I adhere to my original belief, and Dr Carter is of the same opinion, that the fatty change is coincident with and a consequence of the growth of fungi. Nitrogenized and other matter becomes fatty in this way very readily. Of course, under such circumstances the fungi become a centre of attraction for the fat. It is a chemical action entirely, as far as the degeneration is concerned; a process of oxidation which the fungus induces under favourable circumstances in connexion with the performance of its own vital functions.

Thirdly, Fungi act as conveyers of poison.—This is a mode of influence which has been altogether disregarded by observers. If the endogenous pus cell can convey the noxious poison of an acute disease, why may not the elements of a fungus act in a similar capacity? Recent research has shown that all fungi exhibit great transportability. Now, what action have the cells afloat in the air of hospitals during the time of epidemics, such, for instance, as cholera (see Dr Thomson's *Observations at St Thomas's in 1854*); may they not take the virus of a hospital gangrene from one patient to another, acting the part of a fomes in the very same way, com-

paratively speaking, that man himself does? Suppose we inoculate with fungus elements, it is clear that in some instances symptoms ensue (as in Dr Kennedy's and Salisbury's cases) before the onset of local symptoms. Again, the fungous elements would appear to be most active in their early stage, that is to say, when the poison produced simultaneously with their development is in its freshest and most active condition. Again, respirators in epidemics have been found to be efficacious. And, lastly, direct experiments, upon plants especially, have shown that disease may be produced by the contact of fungus elements, when there is not a particle of evidence to prove that sporules, spores, or mycelial threads have entered the organism of such plants, but where there is the greatest probability that the granular and fluid contents may be the poisonous compound which, when absorbed, gives rise to the subsequent malady. It is not unlikely that in catarrh and influenza especially such a conveying property may be at work. We have the strongest possible amount of analogical evidence in regard to animal life, comprehended in all the details of the "animalcule theory of disease,"—a doctrine that may be pooh-poohed by some, but which must erelong be fairly discussed. One might give a great deal of very interesting matter under this head. Those who are interested in the subject should read Sir Henry Holland's article in his *Medical Notes and Reflections*, 4th edition, I think, on the *Animalcule Theory of Life*, and Dr Daubeny's essay in one of the volumes of the *Edinburgh Philosophical Journal*, some few years back. The occurrence of epidemics, be it noted, moreover, is often associated with the peculiar prevalence of various moulds and mildews,—a source of terror and superstitious horror in bygone time, which gave rise to the idea of a raining of blood. Plutarch refers to such an occurrence in the plague of Rome. Hecker, in his work on the *Epidemics of the Middle Ages*, also associates it with the disasters of 789 and 959. The spots were actually observed on garments, and called *lepra vestium*; *signacula* was another term. In 1502 and 1503, it again frightened everybody. Agricola was certainly one of the first to give a rational explanation, he attributing it to the appearance of a lichen. The fungi attacked walls, bread, cheese, meat even, and garments, in Venetia, in 1819, and also articles of food, and garments, and all fruits, during the years following to 1829. And is there not something similar observable now-a-days? Have not we had some very remarkable and severe epidemics, and have not fungi been remarkably abundant on vegetation? I will not theorize, but merely just draw attention to the coincidence. The particular action of fungi now under notice will perhaps be better appreciated in connexion with that now to be described.

Fourthly, which looks upon these organisms as developers of poison, and comprehends Dr Richardson's forsaken theory of zymosis,—a doctrine that appears to me most satisfactory. It has been sug-

gested at different times by one and another observer that the fungi themselves induce change *actually in the circulating current* sufficient to account for disease, either by setting up a kind of fermentative action in the blood, giving rise to the production of a specific compound,—a poison, in fact, just in like way to that which happens in ordinary fermentation, or setting up change by catalysis,—a wonderful enigma. Others affirm that no poison is produced in the body itself, but that the fungus helps out its increase when once introduced into the system. For my own part I cannot believe that any very important change could be induced by the growth of fungi in the blood current. The presence of air is so very necessary; and not only mere presence, but such as is implied by a direct communication between the growing vegetation and the external air. Outside the body, or in the cavities which communicate with the air, many very important and frequent changes are induced without a doubt.

Dr Salisbury is a careful observer. He declares, and as far as I know holds to his opinion, that a form of disease, if not identical, at any rate very like measles, results *under certain circumstances* from the inoculation of the fungus of wheat straw. Dr Kennedy has given confirmatory evidence. Does the fungus *per se* produce the result, or is it a conveyer, or is it the producer of the poison outside the body in the musty straw?

Dr Richardson, quoted by the late Dr Barker of Bedford, records the onset of erysipelatous mischief from a like cause. In France the most serious inflammatory mischiefs of veins and lymphatics have followed wounds inflicted with instruments used to cut off the diseased vine-shoots. Dr Collin, the medical inspector of the waters of the St Honoré, Nièvre, records even fatal results. MM. Demartes and Bouché of Vitranzy have also investigated this subject, and conclude that the oidium *can* produce such mischief, but they suggest some sort of coincidence between the special development of the oidium and the occurrence in greater frequency of inflammatory disease. It is to be hoped that the French Academy will, now it has taken note of the subject, enter into a full investigation of it. It is true that ill effect does not always follow experiments with the oidium. MM. Spenceux and Letellier failed to produce anything beyond a little redness and irritation by inoculating people with the rasping of leaves diseased by the oidium (Pract. Jour. of Med. and Surgery, Nov. 1864); and MM. Leplat and Taillard on the one, and M. Wertheim on the other hand, came to opposite results by injecting fungus elements into veins of dogs and other animals. There can be no questioning that some fungi are more hurtful than others, and much depends upon the concomitant conditions. The arundo donax, the large reed of the south of Europe, is attacked by a black rust, and those who cut the reeds suffer from very violent headaches; and it is affirmed by M. Michel that the spores produce a papular rash on the face, with much

swelling, and a good many serious general symptoms (Yearbook, 1861-2). It would seem that the fungus, *per se*, is not sufficient, but that there is something in addition which is intimately connected with the vitality of the fungus. This would seem to be taught by the case of bacteridia. Whatever they be, no injurious results happen unless the medium itself in which they exist contain some peculiar virulence of its own. Just as in the case of inflammatory attacks caused by oidial inoculation. The power of vegetable organisms to induce transformation, which must of course be accompanied by distinct chemical change, has been well exemplified by an experiment of M. Lemaire, who took some beans, placed them on a moist sponge, and found that bacteridia soon sprang up, before germination, being succeeded by monads and vibriones; and the like happened after the soil used had been heated to a red heat. Now, if a small quantity of phenic acid (which has the property of suspending infusorial development) was added, the germination came to a standstill until the phenic acid evaporated, when it recommenced. M. Pasteur's experiments on acetic fermentation tend to the like result; and M. Trécul's observations lead to the belief that the change induced in solutions by fungi, as in the case of alcoholic fermentation, depends upon the performance of the nutritive act of the vegetable cell. The fact is, the fungus, when growing, necessarily decomposes the medium, and induces chemical change, whilst the result depends upon the composition of the material acted upon. In like manner, it is conceivable that the fungus of wheat straw acts upon the juices of the stem, producing some subtle compound; bacteridia do the same in *sang de rate* and the oidium in the vine disease.

It has been supposed that the poisons of measles, influenza, cholera, nay, asthma, and some other acute diseases, may be produced in the way indicated; but it must be remembered that two or more of the modes of action already noticed may be conjoined; that is to say, a fungus may act mechanically as a conveyer and developer of poison at the same time, and in one case. But not only acute but chronic diseases are produced. I refer to the large class of instances in which vegetable parasites induce slow changes of deleterious nature in articles of diet, giving rise to "ergotism." Bad grain, bad potatoes, bad rice, bad maize, are illustrative. The late Russian epidemic, the Irish fevers, pellagra in Lombardy, gangrene in sheep and beasts, ergotism in horses, have all been regarded as taking origin especially from the play of ergotized foods. In the group of *chronic* maladies the material acted upon by the fungus is a solid. The access of air is not so perfect nor so free; the moisture is considerably less; all of which tends in great measure to account for the difference of the quality of the resultant morbid product. The productiveness of grain so infested is considerably lowered. Sir H. Davy proved this long ago. He found that diseased wheat yielded from 21 to 65 per cent. of nutritious

matter against the 95 per cent. of the healthy grain. It has been suggested at various times that the degeneration of rice by parasitic action gives rise to the formation of products which occasion very severe symptoms of intestinal irritation, resembling dysentery, and that œdema of the leg often follows. And it is not unlikely that the many peculiar ulcerative conditions of the lower extremities are favoured by the quality of food and induced in like manner. The diminution in the productiveness of the silkworm affected by muscardine affords a capital instance of analogical occurrence. The statistics issued by the Chamber of Commerce of Turin show that although formerly about some 650,000 myriagrammes of cocoons were produced in the country, in 1864, there were 525,000, and last year 283,000 only. I have been paying some little attention to the case of mildewed cotton,—hunting after *illustrative* facts,—and I find that the germs of mildew are really present in the cotton in its rough state, as sold in the market before it reaches the manufacturer. It of course is possible that the processes through which it passes in the hands of the latter may destroy all the vitality of the fungi, but this is not certain; but if it really does, still the fact of the presence of mildewed germs in vigour would imply the existence of a certain degree of deterioration in the actual fibre itself, perhaps induced by the bad cultivation or growing of the original plant,—a point of no mean interest to the merchant. It would make the fibre less able to resist the action of the size and other agents used in the manufacture into stuffs.

I have spoken of things going on outside the body, and then introduced to it; but within a recent time, certain facts have come to hand showing that under special conditions, though good food be taken into the stomach, yet, in the digestive tract, changes of objectionable character may be induced by the agency of fungi. I have to quote Dr Salisbury again as my authority. He believes that chronic diarrhœa in the army is caused in this way (see the Report of the Surgeon-general of Ohio, in the Amer. Jour. of Med. Sciences, 1865). Wherever there is a poor amylaceous diet, and there be retention of the food, the torula, almost always present, grows, and in so doing induces fermentative changes, with the evolution of gas,—the production of intestinal irritation and diarrhœa,—the torula vegetating into a myceliated “algoid” mass, which may be observed in the fœces; and it appears that its amount is in direct relation to the severity of the disease; the production of sugar being rapid and detectable in the mucous tissues. The green stools of children are so produced, and Dr Salisbury thinks also that semi-paralytic symptoms ensue. The case of sarcinal disease is on a par entirely; deranged digestion, detention of food, the presence of penicillium, and the evolution of gas with the formation of sarcinae; vomiting is the result of gastric, as diarrhœa that of intestinal irritation. The stomach in the former, and the intestines in the latter, getting semi-paralyzed, at least losing tone and getting relaxed. In both

cases there is the mechanical action of the fungus and the induction of chemical changes within the body. The case of diseased foods is one of surpassing consequence, and deserves all the attention we can afford to it.

The quality and character of the poisons or products of this fermentative act are matters of no little interest. Dr Richardson has lately deserted the zymotic, and given his adhesion to a new theory, which regards the poisons as of an alkaloid character,—basing this position upon the supposed isolation of the pyæmic poison; however, further experiment is needed to establish the truth of the new doctrine. If the poison of so-called zymotic diseases be chemically inorganic, how comes it that nothing of the kind can be obtained by chemical analysis? The diffusion and spread of disease is opposed also to such a view. There is a power of increment about these viruses which is very marvellous and peculiar. There is also a vital principle or act which is very distinct. Another feature worth notice is this, that the effect of the poison does not seem to be, as is the case with mere chemical agents, proportionate to the dose, so to speak, but to the peculiar virulency, which varies as much as the state of the nutrition of the organism acted upon. The viruses certainly, as to their characters, vary considerably, and are not *definite* in the way that we would expect if they were of an alkaloid nature. Independently even of these kinds of influence already noticed, fungi, in the fifth place, would seem to possess inherent noxious qualities in some cases. Just as insects have the power of producing special poisons, so may fungi in a much less degree. The anamita muscaria affords a resinous principle, which chemists isolate. In other cases—for example, the mushroom—there is evidently an alkaloid, as MM. Sicard and Schoras have shown (*Journal de Pharmacie*, 1865); but the action of it is different from that of viruses altogether.

Now, I have mentioned five different ways in which fungi may act; and these may be summed up as follow, being divided into those which are direct and indirect. Directly, they may act mechanically, or by inducing local chemical change; indirectly, by bringing about changes in substances out of the body, which are brought to influence the latter; by setting up a kind of fermentative action in part due to the oxidation consequent upon the nutritive changes in the plant, or by giving rise to products having an acute or a chronic action, and whose nature is at present a matter of doubt.

And now I am prepared to meet the hypothesis that parasitic disease has nothing essential to do with the development of parasites. Mr Hunt takes the boldest view in the ranks of the opposition, declaring that the causes of parasitic disease are four, and four only,—uncleanliness, atmospheric impurities, deficient exercise, and contagion. I take my stand upon the mechanical action of fungi, and the induction of fatty changes, and defy any one to shake me the

least from my footing. Mr Hunt states that the above four conditions "poison the blood, producing not only their immediate effects in the form of parasitic skin diseases, but laying the foundation probably of more serious disorders, manifested in after life by the presence of lumbrici, ascarides, tape-worm, pediculi, fungi, hydatids, tubercles, and perhaps cancerous germs, in the various organisms." What does this mean? That these varied mischiefs have not each their proper cause, but arise from one and the same influence. This is surely either subversion of the logical definition of cause—unconditional sequence—which is so tenaciously upheld and received as our only true belief. I grant that the four states lower the nutrition of the system, and make it more fit for parasitic growth; this is only one item of the total. The true state of the case I take to be this. That there is a necessary nidus, which is exalted by some into the position of the supreme disease, to the negation of any and every effect produced by the fungus itself, which finds the soil congenial,—a soil associated with bad living, and bad hygiene of all kinds; the fungus growing acts in the various ways already detailed; in ordinary cutaneous affections, the effect upon the hair and epithelium (mechanical and chemical) being *pathognomonic*. Parasitic disease, then, is a composite affair, consisting of mal-nutrition, a growing parasite, and certain effects of such growth.

There is yet one category of facts that needs a word or two of comment, viz., the comparative pathology or the intertransmission of parasitic (vegetable) maladies. In addition to what I have given in my work, a good deal of information has been accumulating. It is now admitted that the transmission of the common ringworm of the surface from animals to man is very common. I am informed upon good authority that this is of very frequent occurrence in Australia, the milkers of cows especially being largely affected. Professor Gerlach (abstract in Ed. Vet. Rev., vol. ii.) has noticed it in dogs, horses, and oxen, and in man, but the sheep and pig seem to offer exception. Dr Frazer (Dub. Quart. Journ. of Med. Science, May 1865) contributed a paper, "Remarks on a Common Herpetic Epizootic Affection, and on its alleged frequent Transmission to the Human Subject," containing cases. This gentleman quotes Mr Brady, and Mr Whitla, in reference to other instances. Dr Fehr has noticed in Switzerland the transmission from cattle to man. I can confirm by my own experience the truth of these statements. I do not mention any old cases, such as mice, affecting man; but my friend Dr Allechin informs me that he has seen the transmission of mange from a cat to a child.

Now, I might argue just in like manner in regard to the animal parasites. The two classes of cases are mutually illustrative of each other's *modus operandi*. I take the case of scabies. The acarus demands a suitable soil. It has been pretty well shown, in animals especially, that acari will not grow on all surfaces, but only on those whose hygienic condition we have reason to know from the

circumstances that have been at play is not that of health. The limit of variation is by no means made out in the case of the animal parasites. The relations of acari on bodies generally is being canvassed, especially by German writers. The mode of entry has an analogy also. There is the same difference of opinion as to whether the acari are accidentals or *veræ causæ*; but there is plenty of evidence to indicate the iraction as mechanical irritants, and as the possible developers of irritant products. But these points I cannot now enter upon.

The matter of the action of fungi is a large and wide one; already we see enough to show that the studious inquirer will be amply repaid if he tread carefully the somewhat now uncertain path before him, and the promising indications of success are many.

Lastly, one word for critics. I have attempted to put together novel points for those who feel inclined to know what is the exact position of our present knowledge on parasitism; to collect into a "digest" form what is scattered about in the pages of recent medical literature, as an appendix to my parasitic book. I crave indulgence at the hands of my readers, for I have found it very difficult to condense my remarks. An examination of the subject promises, if only in the field of analogical reasoning, the happiest and most usable results, which will certainly help in no little degree to remove from the escutcheon of Medicine the unjust opprobrium that it is an inexact science.

ARTICLE III.—*On the Conjoined Influence of the Nervous System and of Constitutional Tissue-Changes in the Production of Dropsies, and on the various Methods of Treatment applicable thereto.* By THOMAS LAYCOCK, M.D., Professor of the Practice of Medicine and of Clinical Medicine, and Lecturer on Medical Psychology and Mental Diseases in the University of Edinburgh. PART II., in continuation of a preceding Paper.

IN the first part of this communication (p. 775) I restricted myself to a demonstration of the fact that the nervous system exercises an important influence both directly and reflexly in producing and preventing dropsies and dropsical effusions. It is hardly necessary to remark that such influence can only take effect under appropriate conditions; for multitudes of nervous diseases occur without any dropsy supervening. The same point has been already considered in reference to all those causes of dropsies usually alleged, whether humoral, congestive, or hydrostatic, and which are admittedly predisposing causes. It is necessary, then, to the better understanding of dropsies, that an inquiry be made into another element, namely, the state of the affected tissues themselves, as shown by clinical observation, so as to determine what is the true

relation of those tissues to the nervous system, to morbid states of the blood to the head, and to the causes generally of dropsies.

THE TISSUE-CHANGES IN DROPSIES.

To comprehend what occurs in the tissues in cases of dropsy, it is necessary to bring the method of analogical inquiry into use, and determine thereby how far the general laws of degeneration of nutrition are applicable, and to what extent they are manifested in dropsical effusions. The following are some of the anatomical changes and clinical phenomena associated more or less with dropsies:—1. Purpura in all forms, and both acute and chronic; 2. Diathetic and metastatic irritations and inflammations, chiefly known as the rheumatic and gouty; 3. Certain fatty, fibrinous, and plastic exudations and degenerations, comprised under the terms fibroid, amyloid or waxy, atheromatous, syphilitic, and leprous, of which the capillaries and small arteries are chiefly the seat; 4. Variations in temperature, due to both centric and reflex influences of the nervous system; 5. Congestions and congestive inflammations and effusions of a symmetrical character, or directly associated with changes in the nervous system; and, 6. Serous fluxes from mucous surfaces dependant on nerve-lesions, of which a nervous albuminuria is the type (see p. 786). As to all these (and others), they are found in connexion with dropsies, and are so intimately associated in many cases with changes in the nervous system, that I have found it convenient in teaching to class them and others together as a distinct group of neuroses or nervous affections, in which disordered nutrition of the tissues is chiefly manifested, and have termed them *Trophesie*.¹ Some of them will be most conveniently considered in their relations to the nervous system exclusively, but others in relation to the tissue-changes in dropsies.

Purpura and Dropsies.—In a proportion of cases of general dropsy, purpura is manifested. It is usually attributed to a depraved condition of the blood like that of scorbutus, induced by albuminuria, unsuitable diet, and the like. There are cases, however, to which this theory does not apply, and in which the purpura probably arises from the same causes as the dropsy, with a difference in the condition of the red corpuscles. The following case serves to illustrate this fact:—

CASE XI.—*Paraplegic Edema and Purpura in a Girl aged 18; no Albuminuria, nor Cardiac or Pulmonary Disease; Nervousness and Nervous Palpitations; Pigmentation of the Face. Successful Treatment by Nerrine Tonics.*

Jane U., aged 18, a house and table maid, was recommended to my care by Dr Wilson of Alloa, on 28th July 1865. She was pale, with dark hair, and of small stature. Very collected and intelligent in her manner. She had no

¹ See my "Principles and Methods of Medical Observation and Research." 2d Edition, pp. 342, 344.

cough or palpitation, and no signs of disease of the heart or lungs. Tongue moderately clean; appetite good; legs oedematous, and covered fairly with spots as far as the knees, which are red, pale-red, and purplish, not raised, and varying in size from that of a pin's-head to a millet-seed,—one or two as large as a small pea. She said that previously to the New-year she had occasional fits of vomiting, and had to take medicine for them; was very nervous, and easily startled, and often experienced violent palpitation of the heart, beatings in the neck, and a strong pulsation at the pit of the stomach. She had had no swelling of the neck; but when the palpitation was very violent she became very dark in her complexion, just as if she had been exposed to the sun. Five weeks ago, she had very violent pain in the abdomen "all over it," and weakness and pain in the back. Shortly after, her legs began to swell, and purple spots broke out, especially after standing much. Similar spots appeared also on her arms whenever she had any extra weight to carry, or a strain was put upon them. The dropsical swelling extended as high as her hips, and sometimes the spots appeared on the thighs. The legs always felt cold and somewhat numb, but were sore to touch. Had lived in an English family, and had always plenty of excellent food. No constipation. Catamenia copious, recurring every two or three weeks. Had to strain to pass urine, and was sometimes unable to do so until she had sat over hot water. The urine was not copious; contained no albumen. The case was diagnosed as a uterine neurosis, and a pill prescribed to be taken three times a-day, consisting of valerianate of iron, gr. ij.; extract of nux vomica, gr. $\frac{1}{8}$; extract of chamomile, gr. ij.

Dr Wilson kindly reported to me the result of treatment as follows:—"Within a week after taking the medicine prescribed, a great improvement took place in her condition. The great uneasiness, or rather pain—oedema of the legs—that she complained of began to diminish, and at length disappeared, leaving her in her ordinary state of health. About a month since, her mother asked me if I could give her a prescription for the same pills, as she had lost the original, and the girl was afraid the swelling was returning. I called to-day and learned that she was in the enjoyment of excellent health."

A pigmentation of the face, but more especially of the eyelids, is not uncommon in women of nervous and rheumatic habits; and it is probable that the purple eruption in this case was the same as that termed many years ago by Schönlein *Peliosis rheumatica*. To this kind belongs also the purpura occasionally seen in pregnant women and in anomalous hysteria. There seems to be little doubt that a certain class of cases of acute *purpura hæmorrhagica* are of rheumatic origin. Dr Parkes has very clearly elucidated this point in its pathology.¹ The attacks of purpura and hæmorrhage to which the persons termed "bleeders" are constitutionally subject, are also intimately associated with rheumatic symptoms and tendencies; and in these and other cases the so-called "molimen hæmorrhagicum" is accompanied by nervous phenomena. But purpura may be directly induced by emotions. In June 1856, Dr T. Ogier Ward communicated cases in which it came on after a fright. One was that of a female child, aged 4 years, who had been extremely alarmed by being punished at school. The spots appeared on the fourth day after, and she died on the seventh, convulsed and comatose, from effusion of blood into the ventricles.

¹ Two Cases of Purpura, with Analysis of the Blood.—Lond. Med. Gazette, Nov. 1848.

The other was a boy, frightened by a horse that attacked him as he passed through a field. He bled immediately from the nose; and on undressing him at night he was found to be covered with purple spots and patches. It may be local from the same cause. Mr A. M. Edwards sent to me a case of chorea induced by fright, in which the right arm became affected with a sort of purpura lividity. Dr Seymour relates, in one of his published clinical lectures, the case of a woman far advanced in pregnancy, who experienced a dangerous attack of purpura immediately after a fright. Purpura of the legs has assumed the form of a quotidian intermittent. I have a note of a paroxysmal purpura in a girl of 17, recurring every seven days. Like rheumatism, purpura is also metastatic, both in ordinary forms, in persons of a rheumatic habit, and in the so-called bleeders.

The pathology of purpura is doubtless very complex, but these facts abundantly prove that the state of the nervous system is an important element in causation; and there seems little reason to doubt that both the capillaries and the blood-corpuscles are also involved. We are indebted, however, to Dr Wilson Fox for researches into the tissue-changes peculiar to purpura, in the case of a man suffering at the same time from syphilis and complicated with œdema, in whom a syphilitic eruption (ecthymatous and vesicular) was apparently the determining cause of the purpura. The œdema of the limbs was brawny, except above the ankles; the scrotum œdematous. After death, waxy degeneration of the smaller arteries and capillaries of the parts affected was found. The muscular fibres of some of the muscles had also undergone a similar degeneration, and those of the heart were in a few parts waxy, but in the affected spots the more general appearance was, that the fibres were finely granular, with indistinctness of the transverse striation.¹

The intimate relation between dropsies and rheumatic and syphilitic degenerations (very clearly shown in the cases I have related) has led me to suspect that in the former there is a degeneration of the capillaries as the proximate or essential cause, and I have had microscopic investigations made accordingly, but hitherto without result. Nevertheless, I still think careful microscopic research will prove that in dropsy, as in purpura and certain kinds of albuminuria, there is a structural degeneration of the vessels. It is no argument against these conclusions that purpura is, in the case related by Dr Fox and others, too acute a disease. As to this point, our opinions are wholly *à priori*, and founded solely upon experience as to chronic degenerations only. But there is no reason why these changes should not be as acute as those of ordinary inflammations; and it is certain as a fact, according to my experience, that fatty degeneration of the muscles, smaller vessels, and capillaries, is

¹ Case of Fatal Purpura associated with Waxy Degeneration of the Striated Muscles, and also of the Vessels of the affected Parts.—British and Foreign Med. Chir. Rev., Oct. 1865.

sometimes as idiopathic, acute, and self-limited as the changes in the joints in acute rheumatism. Fatty degeneration of the heart is sometimes very rapid.

The case of Dr Wilson Fox illustrates another point in the pathology of dropsical effusions and their affinities to purpura, namely, the influence of certain specific poisons in inducing them. As a fact of experience, the complication of purpura with rubeola, variola, and scarlatina is admitted; but the relation it bears to the dropsy which sometimes accompanies or follows those fevers is not so obvious. In the consequences of inoculation by septic poisons, like that of erysipelas, or by the venom of snakes, the conjoined results of œdema and purpura are well shown, together or separately, the blood and capillaries being rapidly disorganized.¹

Hæmaturia is a not unfrequent concomitant of dropsy. The smoky tint of the urine in scarlatinal dropsy is due to blood-corpuscles; in chronic cases the blood may be very abundant (Cases VII. and XII.). The usual explanation of this bleeding is "congestion;" but I think it may be more correctly classed with the hæmorrhagic effusions known as purpura, and is due to nerve-influence. In the case just related there was no hæmaturia, nor was blood found in the urine present in the bladder at death; so that it must have been poured directly into the pelves of the kidneys just before death.

The rapid production of œdema is, however, the more common result, and can only be explained (as is the purpura) by the direct local action of the poison on the tissues, the nerves, the capillaries, and their contained blood. I have seen the leg of a man swell so greatly from the sting of a wasp, that the leg of his trousers had to be ripped up to get it off the limb. The clinical counterpart of both the œdema and the hæmorrhages is to be seen, I think, not only in the ordinary forms of scarlatina with anasarca, but in those also in which hæmorrhagic effusion takes place at a very early stage into the ventricles of the brain, pericardium, pleuræ, pelves of the kidney. A case of this kind occurred to me lately in the Royal Infirmary, which threw considerable light on the nature of the tissue-changes in these cases, and in which they were very similar to those observed by Dr Wilson Fox. A man, aged 22, convalescent from exanthematic typhus (in which fatty degeneration of the heart is, I think, a common lesion, and often the cause

¹ The following illustrates the action of snake-venom:—"No sooner had these refractory animals (horses) abandoned the secure pastures for the high grounds than, attacked by snakes, one of them was brought staggering into camp, groaning piteously. A light was speedily procured, when a most revolting spectacle presented itself: the poor beast,—so covered with blood that he appeared literally to have been plunged into a bath of gore,—had evidently been bitten by a snake. . . . So that the venom had not power to produce immediate death, but effected a complete diapedesis or effusion of blood. The horse in a short time expired, evidently in great agony."—*Wild Scenes in South America* (Don Ramon Paez), p. 291.

of death), became affected with scarlatina. It was a smart attack, and the rash was very vivid; but no unusual symptoms were noted nor danger apprehended until the morning of the fourth day of the fever, when he rose to the night-chair and passed a copious bloody stool. He fainted (as it was thought) in consequence, but did not recover from the faint, and shortly died. After death extensive fatty degeneration of the capillaries of the small intestine was found; the pelves of both kidneys were filled with blood, and the serous and mucous tissues generally were reddened as if from sanguinolent infiltration. The condition of the heart seems, however, to have been the immediate cause of death. In the recent state, the ventricular muscular fibres broke down so readily into fragments, that it was difficult to obtain anything like a satisfactory view of their sarcous contents, and their softened readily disintegrating condition gave the impression of very extensive disorganization of the contractile substance. The liver was very fatty, and the hepatic juice contained free oil globules. These were also found abundantly in the blood.

Rheumatic and Gouty Degenerations and Dropsies.—The relations of dropsies to purpura, and of purpura to rheumatic affections, just shown, naturally suggest an explanation of the connexion observed between cardiac diseases of a rheumatic character and dropsies, founded on the doctrine of community of tissue-change. As this question is too extensive for discussion now, I would merely refer to the theory as offering a more satisfactory explanation than those of mechanical obstruction usually accepted and taught (see p. 782). For, by taking as our guide the established fact, that in all these chronic cases of general dropsy there is a degeneration of the smaller tubes, including the capillary bronchi and air-cells, as well as the bloodvessels, we are led directly to the conclusion that some general, constitutional, or diathetic cause, or class of causes, is inducing the tissue-changes. In rheumatic degeneration (speaking generally) the tissue-change is fibroid,—in gouty, atheromatous; but in both there appears to be a preliminary and predisposent stage of defective tone of the vessels, due, in some cases at least, to defective innervation. And not only is this practically the reason why certain localities are preferentially the seat of rheumatic or gouty change, whether acute or chronic in the first instance, but also why metastasis occurs. For if the explanation as to the prevention or removal of anasarca ("serous metastasis," see p. 785) be applied to the tissue-changes of rheumatism, we can clearly see that an inhibitory condition of the nervous system will cause the cessation of the local phenomena, while the productive condition arising at the same time will induce them elsewhere. Thus, the states of the nervous system in metastatic cases of rheumatism afford a simple and sufficient explanation of the phenomenon without dragging in a lumbering humoral doctrine. Further, particular con-

ditions of the nervous system and of particular organs will modify the tissues in dropsies and dropsical effusions, just as in rheumatism. The differences of age and sex, of employment, the portion of the nervous system affected, the state of the uterus and ovaria during menstruation and pregnancy (in which both the blood and nervous system are involved), and the consequences resulting from rheumatic diseases of the liver, lungs, kidneys, heart, must all exercise a direct modifying influence. From this more general point of view, the acute anasarca, which comes on after "taking cold" in rheumatic subjects, may be held to take the place of the articular affections in others, and that, consequently, in such as Cases I., II., V., IX., of cardiac dropsy, the pericarditis and anasarca may be looked upon as alike due to a common cause or class of causes. In like manner, the anasarca after scarlatina and the articular affections, which often both accompany and follow that fever, are of common origin. In all these forms there are the two chief elements of disease,—defective tissue-change, and defective innervation.

Syphilitic Degenerations and Dropsies.—The same doctrines illustrate the close relations between the syphilitic taint and dropsies. I am not aware how far these have been noted and recorded, but certainly Cases IV., VI., VII., illustrate a very common class of dropsies connected with syphilis. The waxy degeneration of the smaller vessels in syphilitic Bright's disease is only an illustration of what takes place elsewhere when the determining and localizing causes of syphilitic degeneration take effect. The difference between the syphilitic, gouty, and rheumatic degenerations respectively, is not, in fact, in the class of tissues affected, nor in the innervation, but in the particular kind of tissue-change. In the first there is "gummy deposit;" in the second, atheromatous change; in the third, fibroid or fibrinous degeneration; but in all a debility or defect of function, and of the same class of tissues. This fact, in the clinical history of dropsies, plainly points to the necessity of a diathetic method of treatment appropriate to each kind of dropsy, considered as a constitutional affection. The connexion of syphilis and purpura is little recognised. In the case of Dr Wilson Fox, quoted above, a good illustration of this is afforded. The condition of the syphilitic skin probably predisposes. A man, who had just entered the Infirmary to be under the care of the late Professor Miller for syphilis, was sent to the skin ward to be under my care, with an eruption which proved to be variolous. He died with purpura hæmorrhagica, and on inquiry it was found to be the second attack of smallpox he had experienced in six months. Unfortunately, it did not occur to me at the time to have the cutaneous tissues and capillaries examined microscopically; but more recent researches show that these undergo a peculiar change in constitutional syphilis. A woman under my care in the Royal Infirmary, suffering from phthisis and

purulent bronchitis, died suddenly from apparently the sudden failure of a weak fatty heart. She had syphilis, and her skin presented isolated slightly elevated circular patches of a tawny colour, rough and cracked on the surface, and without hairs or scales. Dr Smart (my then class-assistant) examined these patches microscopically, and found that they retained their circular form through the entire thickness of the derma. The outlines of the sweat and sebaceous glands and hair-follicles were distinctly visible, but their proper substance was entirely altered by the presence of a fibrous tissue dependant on hypertrophous degeneration of the proper white fibrous tissue of the derma. Bloodvessels, nerves, basement membranes, were replaced by fusiform nucleated fibrous tissue, which extended inward towards the adipose cells a line or two farther than the healthy tissue. The elevated appearance of the patches was due to the growth of the same kind of tissue. The change may be termed a cirrhosis or fibroid degeneration of the skin.

Congenital dropsy is sometimes observed in the newly born infant. Its causes are not known, but it may be referred to these diathetic states. Mr Burton has recorded two cases of infants born in succession of the same mother, affected at birth with ascites and universal anasarca. Dr Wilks made a post-mortem examination of the second infant, but found nothing amiss with any of the viscera, nor any obstruction in the arteries of the umbilical cord. The placenta, too, was healthy.¹

Anatomy of Œdema as a Tissue-Change.—The varieties in diathetic degeneration just described seem to be associated with corresponding differences in the constitution of the accompanying œdema. In cardiac dropsies of rheumatic origin, it seems to be of a more brawny character than in renal dropsies, and if (as M. Becquerel has shown) the effused fluid contains more solid matter in the former than in acute Bright's disease, and in the latter more than in the chronic kind, the difference is probably due, in part at least, to differences in the condition of the blood. But the tissue into which the effusion takes place must also make a difference in the solidity and resistance of the œdema. In renal dropsies it is chiefly, I think, into the subcutaneous cellular tissue; in the cardiac and rheumatic forms, more into the muscular tissue. This is plainly shown by the effects of incisions and punctures of dropsical limbs, which are always more useful, according to my experience, in the doughy renal anasarca. Probably, in many cases, both kinds of tissues are the seat. In a case of cardiac and renal dropsy I had examined after death, the œdema appeared to affect the derma but slightly, but the subjacent tissues were in a state of sponge-like infiltration. The connective and cellular tissues were somewhat on the stretch from simple distention, and the interspaces occupied by fluid. The vascular and muscular

¹ British Medical Journal, 2d May 1863.

tissues were pale and flabby, and the sarcolemma distended by the fluid into little pouches. No change of structure observed.

That the œdema will differ according to the state of the blood, and this according to the diathesis and the class of tissues affected, becomes more probable from a consideration of those kinds of diathetic degeneration which are, at least, closely analogous to brawny œdema,—namely, elephantiasis, or Barbadoes leg, and the tubercular forms of leprosy in which an organized fibrinous effusion seems to constitute the essence of the disease. In both the nervous system is involved,—demonstrably so in leprosy; in both there seems to be a specific cause, and in both the morbid change follows the same order as in ordinary œdemas. Thus, in elephantiasis, the legs and scrotum are most commonly the seat of the effusion. I think that a syphilitic taint, or at least some specific condition of the system as to tissue-change, is also associated with some kinds of brawny œdema. I lately saw a case, in consultation with a medical friend in Yorkshire, in which the legs had been for several years the seat of a brawny œdema to such an extent as to give the appearance of a Barbadoes leg. The father of the patient had syphilis and died of it, and mercurial fumigation benefited the case. The nervous system has also a direct influence on these brawny kinds. I was lately consulted as to the health of an English lady, who two years previously was suddenly awakened from sleep to see her infant streaming with blood. She immediately lost consciousness, and on being restored to it, her right side was found to be hemiplegic. When I saw her the face was slightly distorted, and on the palsied side was a foul ulcer. On close examination, the whole of that side was found to be infiltrated with brawny œdema. She was pregnant at the time, and possibly the state of her blood may have been the cause of the brawny condition. In the obstructive œdema termed *phlegmatia dolens*, this brawny character is very striking. The same influence of the nervous system was shown in the case of a Scottish gentleman who had been many years in Australia, and was recommended to my care by his eminent London physician. Two years previously he began to have twitchings in the calf of the right leg, with tottering; as it got worse the left leg was affected, and within the last few months the right hand and arm began to shake and experience cramps. He had pain in the nape of the neck, and tenderness on percussion; and on careful examination it was ascertained that the neck and shoulders were the seat of a brawny œdema, chiefly, however, on the right side. When he was first attacked, his face was swelled in like manner. This occurrence of purely local œdemas, in connexion with diathetic degeneration and defective innervation, is also witnessed in syphilitic neurosis of the face, as when the *portio dura* or trigeminal nerves are palsied. I have had several such cases, in which the œdema was restricted to the palsied region.

General Relations of the Nervous System to Dropsies.—From the foregoing considerations and facts we can draw some practical conclusions as to the relations of the nervous system to the tissues affected with dropsy or dropsical effusion. We can infer that it modifies the tissues:—1. By increasing or diminishing the activity of the capillary circulation, and therewith the supply of blood to them. 2. By functionally increasing or diminishing the retaining property of the capillaries, and thereby facilitating or preventing transudation. 3. By modifying the vital affinities of the capillaries and the blood within them—more especially the corpuscles,—and thus modifying the processes of blood-metamorphosis. 4. By so altering the nutrient processes in the capillaries themselves, that structural changes in the capillary textures result, and thus inducing a predisposition to the formation and transudation of albuminous fluid. Such predisposition being established in consequence of defective activity of the nutrient nervous system, dropsical effusions readily take place upon the occurrence of exciting causes, whether they arise in the nervous system itself or otherwise. In other words, there is a local or general affection of the vascular system and the tissues in certain dropsical effusions, together with a local or general affection of the nervous system.

On the side of the blood and tissues may be mentioned as predisposing causes:—1. A diathetic tendency to diseases of the vaso-motor system itself, and of the small vessels. 2. A diathetic tendency actually manifested, as in waxy degeneration and atheroma of the vessels. 3. A tendency to cardiac and vascular diseases, or the actual development of them, as in rheumatic affections. 4. Morbid conditions of the serum and blood-corpuscles, as from profuse hæmorrhages, albuminuria, and albuminous fluxes, or the fibrinous condition (hyperinosis) which coincides with rheumatic and puerperal states, and with which the state of the blood during menstruation and in scorbutus may be classed. 5. Certain general morbid conditions in which there is a general cachectic state of both the blood and tissues, as in the syphilitic, cancerous, leprous, cachexiæ.

On the side of the excretory organs, through the functions of which the due amount of water in the blood is maintained, the kidneys and sudoriparous glands are chiefly involved. All these general morbid conditions may be observed in practice without any anasarca, perhaps as frequently as with it. They are, therefore, insufficient of themselves to induce general dropsy, so long as the aqueous excretions equal the aqueous ingesta. If, however, the cutaneous secretion be suppressed, and then the kidneys fail to excrete urine in proportion to the amount of fluids taken, and no diarrhœa supervene, there is sure to be an accumulation of fluid in the cellular tissue, so that the patient gains weight. And as there is often great thirst in cases in which the renal function fails, the patient drinks more than usual, and becomes rapidly bloated

with water. Dropsy from a *plenum* thus caused, is, I think, amongst the established facts of pathology. Nevertheless, the very failure of function in the two organs of aqueous excretion—the skin and kidneys—is not unfrequently due to changes in the tone of the nervous system (which appear to be, in fact, of an inhibitory character), as well as to tissue-changes in the organs themselves.

HOW AND WHAT PART OF THE NERVOUS SYSTEM IS INVOLVED IN DROPSIES.

Having determined what part the tissues play in dropsies, we can now the more easily investigate the nature of the changes in the nervous system which influence them, and what part thereof is involved.

As to the nature of the changes in the nervous system, they are distinctly of two kinds, the one favouring dropsical effusion, and therefore essentially morbid, and the other resisting or preventing, and therefore essentially normal. This is true, paradoxical as the statement may appear, of such changes as are plainly morbid, as in Case V. of embolic hemiplegia. For in all the instances I have watched of differential nervous anasarca, the excepted limb became gradually infiltrated as the effusion increased elsewhere and the fatal termination approached, thus showing that the excepted tissues were enabled to hold out longer against the causes which had involved the more dropsical, and indicating the line of inquiry as to treatment.

And that this is the real state of things in a case of general anasarca is probable from other considerations. Certain general morbid states of the nervous system seem to inhibit anasarca generally. It was pertinently asked by my friend Dr Webster, why, if defect in the nervous system be cause of dropsy, are the insane so remarkably exempt from it? Out of 298 autopsies of insane persons recorded by him, only 4 were examples of simple anasarca; 12 were anasarcous, with effusion into the chest and pericardium; while in 23 there were effusions into the thoracic cavity, without any anasarca whatever.¹ Now, these facts—conclusively proving at least that it is not every kind of nervous disorder which induces anasarca—may be held to prove that the state of the nervous system in insanity is in some degree prophylactic against it—is of that morbid kind which is inhibitory. There are reasons for concluding that this condition arises in the course of other diseases of the nervous system. Under any deductions we can understand that the reason why one person gets dropsical with the blood-causes of anasarca in a trifling degree, while it is not manifested at all in others in whom they are strongly operative, rests probably on the different powers of that portion of the nervous system upon which what is termed tone depends, and that in pro-

¹ Report of Proceedings of Royal Med.-Chir. Society of London, in Med. Times and Gazette, and other journals, 13th May 1865.

portion as the tone is lowered locally or generally, the dropsy of the tissue or tissues will be caused or increased.

Further: in several of the cases detailed, two opposite and antagonistic conditions on opposite sides of the body seemed to be dependant upon the same unilateral and centric change—a fact of considerable practical importance. A similar result is shown by experimental researches on the spinal cord, for they prove that when it is divided half through laterally, changes take place in the two halves of the body below the section of an opposite character. 1. On that side of the body corresponding to the section there is increased heat and vascularity and hyperæsthesia. 2. On that side opposite to the section there is diminished heat and sensibility and anæsthesia. It is curious to observe how those who theorize from those facts confine their attention chiefly, if not entirely, to the positive phenomena, and give little if any consideration to the negative. But it is plain that since the two classes of phenomena are due to the same cause, they must be taken together in all theoretical considerations. If, then, in vivisection a lesion of one-half of the cord produces opposite conditions of the circulation in the two halves of the body, the same result should follow in morbid states of the cerebro-spinal axis; so that the same centric change which causes increased vascular activity on one side of the body ought to diminish it on the other. Now, this conclusion is established by a consideration of the cases of differential dropsy detailed.¹

A confirmation of these views is afforded by the fact that this inhibitory action of the nervous system is seen in other forms of disease besides dropsies. For example, section of the sympathetic or spinal cord on one side is followed by increase of sweat on the same side; if not by diminution on the other. Now, as suppressed perspiration is one of the chief exciting and aggravating causes of dropsies, while diaphoresis is one of the efficient means of relief, it is of practical importance to know the reason why. In certain cases, sweats are distinctly symmetrical. I have lately had under observation a case of phthisis, in which the profuse phthisical sweats did not occur on the left arm and left side of the thorax, while all the rest of the body was bathed in perspiration. They were evidently inhibited in the same way as the œdema in cases of general anasarca (Cases II., III., IX., X.). The following is a case of inhibited perspiration (paraplegic) in a case of diabetes. Edward N., æt. 23, a patient in Ward I., admitted 29th March

¹ These remarks apply exclusively to the results of hemicentric lesion upon the body, or those parts supplied with nerves from the seat of lesion. When the nerve-centres themselves are affected, then the lesion may have quite different results. Thus, injury to the external convolution of the left frontal lobe is so constantly followed by speech-palsy (aphasia), that it is concluded the organ of language is unilateral; whereas the fact only proves to my mind that the lesion on the left side, while it effectually stops the function there mechanically, also stops it on the opposite side dynamically by decussating inhibitory action.

1865, to be under my care, received a severe crushing blow on the lumbar region two years previously, whereupon his health failed, and he suffered from diabetes mellitus. On admission the patient was found to pass 200 oz. of urine daily, of sp. gr. 1040, and containing sugar. He sweated at night very much, but the perspiration was limited to the upper half of the body above the umbilicus, —all below that point was quite dry, so that diaphoresis there was inhibited. That there was some paraplegic condition was manifest, although the only signs of lesion were pains in the joints and a feeling of fatigue in the calves. The hands and forearms were also quite dry. He had *muscæ volitantes*, and objects appeared to be twice their natural size. There are also both hemiplegic and paraplegic sweats in fevers. Often, as in phthisis, the sweats in intermittents are limited to the head. "A young man who had been hemiplegic on the left side from infancy came into hospital suffering from a tertian. He sweated on that side only which was not palsied. He assured us that he never sweated except on that one side—the arm, leg, neck, and side of face. In another case of tertian, the sweating was always limited to the two legs, where it was very profuse. Immediately above the knees the skin resumed at once (*brusquement*) the dry condition."¹ This latter limitation is of very common occurrence in œdema, wasting palsy, spinal neuralgiæ, etc.

There are cases on record in which the entire series of phenomena of an ague were limited to one-half of the body, or to a limb, and evidently by inhibitory influence of the nervous system in those parts which were free.² Still more instructive are those instances of hemiplegic eruption in cases of measles and scarlatina in which the cutaneous inflammation is limited to one-half of the body, and for which the most satisfactory explanation is to be found in the law of antagonistic nervous production and inhibition. "Retrosession" of the eruption can also be explained in the same way. And it would appear, too, that in those cases in which there is no eruption, and yet a proclivity to anasarca, the two conditions are really in close relation to each other, through a common cause having its origin in the state of the nervous system of the individual.

It is, doubtless, a very natural inference from all these considerations that it is the sympathetic or vaso-motor system which is involved in dropsies. Thus, Professor Claude Bernard found galvanism to inhibit the functions of the sympathetic branches of the salivary glands in dogs, and arrest the secretion as well as the circulation. There are, however, considerations which would make us hesitate to adopt so restricted a theory; but chiefly I would

¹ Andral, *Clinique Médicale*, vol. i. p. 477 (Fièvres).

² The case of Mary W., in *Boston Med. and Surg. Journal*, 15th Nov. 1855, is a recent example of this. The ague fit was restricted to the right half of the body. In a case related in the *Ephemer. Med. de Montpellier*, the left side was attacked in a female otherwise healthy, aged 22. Others are recorded.

mention this, that these dropsical effusions result from changes in the nerves and nerve-centres, which are not manifested by the ordinary signs of vaso-motor, motor, or sensory neuroses, but by changes in the blood and the capillaries, which latter are really modifications of nutrition, and belong, therefore, to the processes of vegetative life. Now, these go on in all organisms, independently of a nervous system; so that the function of that system is but to direct and regulate them. Such an element of the nervous system must necessarily underlie all the others, and from the nature of its functions may be rightly termed trophical. To these belong the regulation of the temperature of the body, attributed by many to the vaso-motor system, the proper function of which is to regulate the distribution of the nutrient fluid. It by no means follows that because there are coincidentally heat and vascular activity developed in tissues after section of the portion of the sympathetic in relation with them, that that is the cause of the heat. On the contrary, as I have already shown in this Journal (July 1863), the heat is just as probably the cause of the vascular activity. It is, in fact, when both innervation and circulation are most defective, that, under certain conditions of the tissues, heat is most abundantly produced, as in the corpses of persons dead of yellow fever and the cholera. In dropsies the temperature is usually low, but not always; but, whether or no, it is probably regulated by the condition of both the tissues and the trophical system.

Whether this trophical system has an influence on the production of water in the body, or even on the combustion of it, is a question not to be altogether neglected in investigating the pathology of dropsies. Water is produced apparently in vital processes, and not a few observations have been made and recorded as to the production of water in the body, in cases of diabetes, but their accuracy has been questioned. It is, doubtless, difficult to watch a diabetic patient sufficiently closely, and the fact may be at least held in doubt. But then, on the other hand, if, as Dr Parkes has shown, more water is taken in in fevers than can be accounted for by the excretions, what becomes of it, unless it is burnt up in the body?

I would not, however, be understood as excluding the other elements of the nervous system from all share in causation. The sensory portion has manifestly important relations to all these tissue-changes, and the cerebro-spinal, vaso-motor, and sympathetic do in some way exercise a direct action on the nutrition of the tissues in dropsies, and on the changes in the blood and the capillaries. Such a system as the trophical must, from its very functions, be co-extensive with the whole, and include the whole, nervous system; so that, although the volitional, motor, and sensory nerves be not primarily involved, disorders of motion and sensation may help us to conclusions as to the seat of centric derangements. Now, in hemisections of the spinal cord, it is observed that there is anaesthesia of the opposite side, and hyperaesthesia of the same side.

This is explained by the fact that the sensory nerves decussate all the way up, so that lateral section at one side cuts the sensory fibrils of the opposite side, whereas it only excites that centric exaltation of sensibility in the afferent or sensory nerves of the same side, which any irritants are apt to do. In these cases it seems probable, from Dr B.-Séguard's experiments, that the oxygen of the air is the irritant. But Dr Séguard has also shown that hemisection higher up, as, for example, at the level of the *corpora restiformia*, excites hyperæsthesia on the same side as the injury. These facts help us, then, to a conclusion as to the seat of the lesion in cases of anasarca. In Case I., the hyperæsthesia being on the side opposite to the anasarca, the seat of the lesion was encephalic, and in the medulla oblongata. The morbid mental state seems to confirm the conclusion. But in the cases in which the œdema was on the same side as the hyperæsthesia, the lesion was spinal; and the absence of all mental symptoms confirm the conclusion. In Case III., the encephalic lesions (apoplexy) were consecutive to the hyperæsthesia and œdema. Case IV. was manifestly of spinal origin, and we may fairly infer from the symptoms that the right half of the "oculo-spinal" region of the cord upwards was involved.

Much might be learned by an inquiry into the causes of dropsies which affect innervation. I shall only say here, however, that anything which depresses the powers of the nervous system, and in particular which involves its nutrition, is a certain cause of dropsy when the other conditions coincide. Thus, grief and anxiety, bodily fatigue, exposure to cold, all induce dropsy by thus acting.

THE TREATMENT OF DROPSIES.

It necessarily follows from the preceding considerations, that in the treatment of a case of general dropsy, regard must be had to the condition of at least the nervous system, the blood, the tissues affected, and the organs which eliminate water—chiefly the skin and the kidneys. And, in accordance with current ideas of therapeutics, we should have to select corresponding remedies, namely, nervines, analeptics, alteratives, and diaphoretics and diuretics, including hydragogue cathartics. But it would help us little if we were to take results only for comparison, and not the process by which the results follow, because a nervine may be both diuretic and alterative,—an alterative both nervine and diuretic. To attain to anything like a clear view of the best remedies for any particular case, we must, therefore, abandon the old empirical method.

Alteratives as Diuretics.—There is a class of remedies which, because of their uses in diathetic diseases, like rheumatism, gout, syphilis, may be termed diathetic alteratives. Such have been in constant use in dropsies under the name of diuretics. An important group includes liquor potassæ, and the acetate, citrate, bitartrate, carbonate, and nitrate of potass; and in a less degree salts of soda, specially the carbonates and phosphates. It is an obvious

conclusion that, as these alkalines have been found useful in gouty and rheumatic affections, they may influence the renal excretion, the blood, and the dropsical tissues, in virtue of a similar *modus operandi*, and thus *drugs which, as to their effect are diuretics, are as to their mode of action alteratives*. This generalization is rendered more probable by extending its application. Thus, lemon juice is a valuable remedy in certain kinds of acute rheumatism and rheumatic gout; it has been found equally effectual in dropsy and dropsical affections, proving a powerful diuretic in doses of a table-spoonful or more three times a-day.¹ I do not enter at all into the therapeutical theories according to which these remedies have been prescribed, preferring to compare the results of experience in their use, and to note the practical conclusion that in dropsies of rheumatic origin, or occurring in rheumatic and gouty subjects, the remedies for gout and rheumatism are found useful. The iodide of potassium has also been found a valuable diuretic, but more particularly in dropsies of syphilitic subjects, and probably because it is really the most reliable alterative in syphilitic degenerations generally. *Mercurials*, in combination with other remedies, have been used as diuretics, as in Dr Baillie's formula of blue pill, with squills and digitalis. Dr Pereira prescribed from ʒss. to ʒj. of the solution of corrosive sublimate to be taken every six hours, with great benefit in cardiac and other so-called obstructive dropsies. Probably mercurials would be most useful in syphilitic dropsies. *Arsenic* has been administered in dropsy with advantage, and as it is an established remedy for certain forms of rheumatic gout, leprosy, and fevers (malarious), ending in anasarca and ascites, it is probably a diathetic alterative in dropsies. It has, however, a direct action on the nervous system, as in chorea, neuralgia, epilepsy; so that it is to be classed with the vascular nervines.

Antimony may be classed with arsenic and mercurial drugs as to its chemical nature, but it seems to act more directly on the heart and vascular system, rapidly lowering the pulse. It is undoubtedly of great value in the acute anasarcas, especially when conjoined with the hot-air bath; the effusion disappearing rapidly under the combination. In articular dropsies it is also useful. In combination with opium, or even alone, it is, like digitalis, a very

¹ Apple juice has been administered with the same good results. There is a history on record of a man who was so anasarcaous that the cuticle of his thighs and legs cracked from distention. He had also an insatiable thirst, and as the season for cider-making arrived when he was reduced to the greatest state of distress, the idea of drinking new cider became so exquisitely pleasing to him, that, considering his case to be quite desperate, the people about him gave it to him *ad libitum*. He accordingly drank it mixed with water, seldom to the extent of less than five or six quarts a-day. In consequence he discharged eight or nine quarts of urine in twelve hours, and continued to do so until he was radically cured.—“Several Extraordinary Instances of the Cure of the Dropsy collected by Geo. Baker, M.D.” (two other like cases are noted), in Medical Trans., vol. ii. pp. 242, 245.

valuable remedy in delirium not of inflammatory origin. We may, therefore, conclude that it acts on the vascular system.

Nervines as Diuretics.—There are two groups of vegetable remedies in dropsies which act on the nervous system; the one being used as diuretics, the other as hydragogue cathartics. To the former belong tobacco, digitalis, squill, senega, and colchicum.

Digitalis is perhaps the most certain and powerful diuretic in dropsies. It was first used to this end by Withering, nearly a century ago, who found it useful chiefly in those kinds in which the œdema was doughy; in cases in which it was solid and resisting digitalis was of little use. Consequently it is not indicated in the rheumatic dropsies. Withering also observed that it was most useful in cases in which the pulse was feeble and intermitting, but seldom succeeded in those in which it was “tight and cordy.” In modern practice it has been found useful as a cardiac tonic, and to this result its diuretic action has been referred, in accordance with the obstructive theory of cardiac dropsies. Sir H. Holland, however, in his interesting and valuable notes on the use of digitalis,¹ suggests that the seeming contradiction in the depressive effects of digitalis on the heart being less manifest in debilitated persons, may be explained by its manner of action on the vascular system, still so imperfectly known, and more recently it is explained that it acts directly on the capillaries of the heart, causing them to contract. Consequently, when they are relaxed, it gives tone and vigour to the heart’s action; but, on the contrary, if their contractility be normal, renders it abnormal, and so weakens the heart’s action by diminishing the supply of blood to its muscular tissue. The late Dr R. B. Todd laid down a rule for its administration, which has been very generally adopted, viz.:—That it is not to be prescribed in cases in which there is regurgitation, especially aortic. I hesitated long to administer it in a case which I subjoin, in accordance with this rule, but finding all other remedies of no avail, I at last prescribed it with good results.

CASE XII.—*Cardiac Dropsy; Pericarditis (?) ; Anasarca, differential and diagonal; Hæmaturia; Granular Tube-casts. Treatment by Infusion of Digitalis.*

Andrew S., aged 21, a shoemaker, was treated in the Clinical Ward, Royal Infirmary, during the summer of 1865. His mother died of dropsy. Two years previously he had an inflammation of the left lung, when he spat blood profusely. This hæmoptysis recurred from time to time, with great difficulty of breathing, violent cough, and copious expectoration (fibroid lung?). He had palpitation, and a systolic thrill and murmur over the apex. No dropsy or albuminuria. He was readmitted under my care on 20th October, with ascites and general anasarca, loud cardiac murmurs, signs of pulmonary œdema, and emphysema and copious albuminuria with tube casts. The œdema was firm. At first it was observed that the prepuce was œdematous when the scrotum was unaffected; and, although the anasarca was general, there was a diagonal differentiation in the extent to which the limbs were affected. The symptoms became more and more intense, so that by the beginning of January last he

¹ Medical Notes and Reflections, 3d edition, p. 550.

had complete orthopnoea; cough very troublesome; expectoration copious and streaked with blood; thirst great; urine 20 oz. per diem, bloody, with much epithelium and fatty and granular casts. A loud rough cardiac systolic murmur heard at both apex and base; transverse dullness greatly increased; apex beat $2\frac{1}{2}$ inches below the nipple. Elaterium, scammony powder, sesquichloride of iron, lytta, secale cornutum, horse-radish spirit with infusion of broom tops and spirits of nitre, acupuncture of legs, dry cupping of loins, hot-air bath, and antimony, were all tried with no lasting benefit. At the date of January 5, he had gained 49 lb. in weight from dropsical accumulation, and his measurements (taken by Mr Moon, clinical clerk), were as stated below. At that date he was ordered to take $\mathfrak{z}\text{ij}$. of infusion of digitalis every four hours. The urine was more than doubled in quantity, in thirty-six hours' perspiration commenced, and on 10th January the measurements were diminished as stated below:—

Measurements of Anasarcaous Limbs in Inches in Case of Andrew S.

	Over the Knuckles.		Wrist.		Middle of Forearm.		Ankle.		Calf.		Lower Margin of Patella.		Upper Margin of Patella.		10 inches above the Patella.	
	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.	R.	L.
1866.																
July 5.	$8\frac{1}{2}$	$8\frac{1}{4}$	$7\frac{1}{2}$	$7\frac{1}{4}$	$9\frac{1}{2}$	9	10	11	$16\frac{1}{2}$	$15\frac{1}{2}$	15	$15\frac{1}{2}$	$16\frac{1}{2}$	$17\frac{1}{2}$	$21\frac{1}{2}$	22
" 15.	8	8	$6\frac{1}{2}$	$6\frac{3}{4}$	...	...	9	9	14	$13\frac{1}{2}$	$13\frac{1}{2}$	14	16	$16\frac{1}{2}$	$20\frac{1}{2}$	20
Diminished in 9 days,	$0\frac{1}{2}$	$0\frac{1}{4}$	1	$0\frac{1}{2}$	...	...	1	2	$2\frac{1}{2}$	2	$1\frac{1}{2}$	$1\frac{1}{2}$	$0\frac{1}{2}$	1	1	2

On 5th January the ascites was on a level with the umbilicus. On 15th January the ascites was $3\frac{1}{2}$ inches below the umbilicus.

It will be observed that the right leg and left thigh were the most anasarcaous, and *vice versa*, and the right hand and arm more anasarcaous than the left. There was a like difference in temperature. On 15th January the right leg at ankle was 87° ; left $88\frac{1}{4}^{\circ}$; the right leg at thigh was 95° ; left 94° . So that the legs and thighs were differentially affected, both as to effusion and temperature. In about four weeks more (on 10th February) the clerk reports, "The limbs are now getting to their natural size. He does not perspire so much; urine pale, sp. gr. 1017, alkaline, no albumen; breathing easier; thirst less."

There could be no doubt that the digitalis alone modified the symptoms so favourably in this case; and although the hypertrophy and regurgitant murmurs seemed to contra-indicate its administration, the patient had rarely to intermit taking it on account of cardiac symptoms. On looking on the comparative measurements it will be seen that generally the limbs most swollen differentially were most rapidly influenced by the drug,—a fact which might lead us to the conclusion that the drug acted on the tissues themselves, and not merely because of the diuresis which followed.

It is obvious that the true *modus operandi* of digitalis has yet to be established; but I think there is no reason for theoretically limiting its action in dropsies, or in hæmoptysis, mania, epilepsy, delirium tremens, and other diseases of the vascular system to the capillaries of the heart: the more obvious conclusion is that it acts upon the capillaries generally and on those of the nerve-centres through the vaso-motor system, and is thus both a nervine and tissue alterant.

Tobacco is closely allied to digitalis in its action, and was first tried extensively in dropsies by Dr Fowler.¹ He seems to have tried it in every form of dropsy,—peritoneal, cardiac, and renal,—with good results. The most marked advantage was shown in uncomplicated cases of ascites. I tried it applied externally to the abdomen in the form of infusion, in conjunction with the internal use of digitalis, in the case of a young girl aged twenty, in whom a distressing ascites came on without any discoverable cause in the liver or peritoneum, so that paracentesis seemed to be indicated. In a few days, however, the fluid diminished rapidly under the treatment mentioned, a copious diuresis being the result.

Colchicum rightly administered is a powerful diuretic. Like digitalis and tobacco it acts upon the heart and the part affected, and is probably, therefore, a vascular nervine. Its well known value in gouty diseases indicates its use in dropsies of gouty subjects. It may be remarked, however, that as to these and other means of cure of dropsy, what is wanted is—not new remedies—but a more thorough knowledge of those we have. Senega, scilla, actæa, guaiacum, ammoniacum, are all of this class. Until we are better acquainted with the best modes of administering our old and well-tried drugs, it is a vain labour to introduce into practice the new and untried.

Hydragogue Cathartics are supposed to operate by eliminating watery fluid from the blood through the intestinal canal, and are prescribed when skin and kidneys fail in their functions. Some of these are powerful nervines, as hellebore, elder, elaterium; others are powerful irritants, as croton oil, camboge, colocynth, scammony. They may, therefore, exercise an influence altogether apart from the mere elimination of water. That irritation of the intestinal canal has a reflex influence on the spinal cord, and through that on the kidneys and affected tissues is, I think, a fact too much overlooked in dropsies. I had a case of chronic dysentery under my care in a man aged 35, who had no dropsical symptoms, and no albuminuria, yet after death his kidneys were seen to be exquisite examples of the contracted kidney. The whole of the colon was found to be the seat of extensive ulceration and waxy degeneration, and to this condition the exemption from dropsy and albuminuria was probably due. This is by no means a solitary instance in my experience. It has long been suspected that the value of calomel as a purgative in acute hydropcephalus is due to its reflex action on the brain through the intestinal mucous membrane. Occasionally remarkable benefit results from this method of treating dropsies; but, except in ascites, it is, upon the whole, of doubtful value, and its uses might be attained at less waste of strength by sinapisms to the back and legs carefully applied, so as to act by reflex action through the skin instead of the intestines.

Nervine Tonics and Stimulants.—There is a large and diversified

¹ Medical Reports on the Effects of Tobacco in the case of Dropsies and Dysurics, 1785, 2d edition, 1788.

group of means and drugs which act beneficially in dropsies directly through the nervous system. In Case XI., the *valerianate of iron*, combined with extracts of nux vomica and chamomile, was of immediate benefit. The tincture of the *sesquichloride of iron* seems to be rather a stimulant to the spinal cord than a blood-tonic. *Cantharides* used as a diuretic is, in fact, a powerful nervine tonic for the genito-urinary system, and is indicated and beneficial in renal dropsies, associated with paraplegic paresis, spermatorrhoea, and sexual debility. I have used the *Secale cornutum* also with advantage in some cases. It is a nervine tonic of the vascular kind. *Strychnine* is a remedy of very varying value. In cases of dropsy with nervous thirst (polydipsia), albuminuria, and copious micturition (diabetes insipidus), it is of essential service. In a case of this kind under my care, in a gardener aged 39, of long standing, in which 178 oz. of highly albuminous urine, sp. gr. 1010, were passed daily, the quantity was reduced by strychnine in twelve days to 70 oz., and all the symptoms were ameliorated. The morbid appetite for water in dropsy is, I think, most commonly renal in its origin, and is due to a hyperæsthesia of the pelves of the kidneys. This view of the thirst of the dropsical is not, I think, commonly entertained, but it has arisen in my mind from observing the excessive thirst in cases of gouty pyelitis and gravel. Doubtless in some it is a genuine nervous thirst,—that is to say, is due to changes (hyperæsthetic) in the encephalic centre of the appetite. It is a very important symptom in dropsies, because the skin is generally dry; so that, should the kidneys fail in their functions, dropsy is rapidly developed. Hence the anasarca which arises in cases of diabetes mellitus when the urine becomes scanty. Strychnine seems to aggravate some kinds of dropsy, more especially those in which the urine is scanty and the lungs œdematous; so that it demands judgment and experience in its use. *Opium*, too, is of doubtful value in this class of cases, although in some kinds morphia seems to increase the quantity of urine.¹

There are persons who are constitutionally very intolerant of opium; these are usually of the neuro-vascular diathesis, and predisposed to atheromatous degeneration. In such opium and strychnine are likely to be injurious, but digitalis beneficial.

Faradization of the lumbo-sacral region is indicated in renal dropsies. I have observed that occasionally it immediately but

¹ A case is mentioned by Sir George Baker (*opere citato*) of a woman, aged 39, who had suffered from a formidable ascites since the age of 20, and who, in consequence of violent rheumatic pains, was ordered to take four doses of Dover's powder, ℥ij. to each dose, on four successive nights. An hour or two after the second dose "she began to void her urine in large quantities, which she continued to do through the whole night, and as fast as she voided the water the belly sunk. She took another dose of the powder the next night, which, having had the effect of the former, the belly entirely sunk, and in all appearance the evacuation of water had been total and complete." The cure was radical.

only temporarily increases the amount of urine; in other cases, it seems to be of permanent advantage, and might be made a simple and valuable auxiliary to other means.

The Hot-Air Bath has been commonly prescribed with the view of stimulating the sudoriparous glands, but heat has a powerful action on the nerve-centres when applied to the skin; so that the process may be classed with faradization. It is most useful, according to my experience, in acute cases, and in the desquamative forms of renal dropsy; it is also available in the syphilitic, but not generally borne well in the rheumatic and cardiac kinds, nor when the skin remains dry. I have treated cases successfully by the use of the hot-air bath alone, and a suitable diet and regimen. *Irritants* and *counter irritants* have been already noticed as really belonging to the nervine class, and acting upon the excretory organs and affected tissues by reflex action; dry cupping over the loins is often useful, and belongs to this class of remedies.

Diet and Regimen in Dropsies.—In the acute cases the diet and regimen are such as would be adopted in an ordinary case of acute rheumatism, the anasarca being the analogue of the articular affection. Warmth in bed, diluents, and quiet with friction over the lumbar region. In the chronic cases of general dropsy more care is required in determining the diathetic character in the blood and nerve causes. As a general rule the tone of the nervous system should be maintained by free exposure to the atmosphere, and confinement to bed especially avoided as long as possible. Carriage exercise is not always available, but sitting out in the open air may be attained, or at least the room be well ventilated. In all cases the limbs should be well bandaged with flannel, or the legs encased in elastic stockings. When the albumen is abundant in the urine, albuminous food must be taken freely. In cases following upon flooding I have seen it useful to combine glycyrrhine and bitter infusions and extracts with perchloride of iron. Mineral waters adapted to the diathetic condition should be selected; and, in choosing a “watering place,” a dry, bracing mountain-air should be preferred. When the thirst is urgent, and the urine scanty, the important question arises whether drink should be freely allowed or not. If the urine be acid, and there be lumbar pains, the diluents suitable for rheumatic affections are indicated,—such as weak lime-juice, lemonade, weak solutions of cream of tartar, aerated alkaline waters, alkaline Vichy and other mineral waters. If it appear, however, that the thirst is not renal but nervous (a polydipsia), opium or its salts are more likely to relieve the symptom, and to enable the kidneys to carry off the excess of water taken by the patient. Diaphoresis induced by the hot-air bath is also indicated.

ARTICLE IV.—*On Encysted Serous Pelvic Effusions following Puerperal Inflammation.* By J. MATTHEWS DUNCAN, A.M., M.D., F.R.S.E., etc., etc.

(Read before the Medico-Chirurgical Society, 7th March 1866.)

THE first case of the kind now to be described, which came under my notice, occurred in 1855. It at once excited in me great interest on account of its rarity; and although I have, since that time, had extensive experience in pelvic tumours of all descriptions, I have only met with other two similar cases. In obstetric literature, also, I have met with no satisfactory description of cases like those I am now to relate. For the size of the cysts in my cases seems alone sufficient to separate them, to some extent at least, from the small serous cysts of the uterus, described by M. Huguier, in his paper "*On the Cysts of the Uterus and the Follicular Cysts of the Vagina*;"¹ with which nevertheless I am disposed to place them in juxtaposition. It is impossible to decide absolutely how my cases should be named and classified, because none of them ended fatally; and, without autopsies, one cannot place beyond doubt the seat and relations of the cysts. When M. Huguier's paper was published in 1847, he had only recognised during life a serous cyst of the uterus in two cases, which he describes. Both were, he says, the result of metro-peritonitis, a condition to which he ascribes all the cases known to him; a condition, also, which was certainly present in the three cases which I have seen. By the word "cyst," as used in connexion with the cases now under consideration, I do not wish to imply the presence of a cyst-wall proper to the serous cystic contents. Such a cyst-wall or bag, not, however, necessarily a new structure, is implied in the word as used by M. Huguier when he speaks of serous cysts of the uterus. Of Huguier's cysts I lately saw an example in the autopsy of a case of ordinary cancer of the neck of the uterus; where two little serous bags of the size of hazel-nuts were, without adhesions, lying in Douglas's space attached to the lower part of the posterior wall of the uterus by a base narrower than the breadth of the cysts at their middle parts. In the cases to be now recorded, it is probable that the serous fluid was enclosed in the peritoneal cavity, by the parietal peritoneum on the one side and various parts of the bowels and uterus on the other side; these various parts being united by adhesions; as is frequently observed in cases of pelvic abscess and hamatocoele.

This anatomical site is well illustrated by a dissection of a case of cancer of the body of the uterus recorded by M. Forget of Strasbourg. I shall here give some particulars of it on this account, and because it is, in other respects, the case most nearly allied to those under consideration in this paper,² of which I know the details.

¹ Mémoires de la Société de Chirurgie de Paris. Tome premier. 1847.

² Gazette Médicale de Paris, 1851, p. 641.

The woman died of cancer of the body of the uterus, at the age of 62. She had seven years previously been believed to have ovarian dropsy. She was four times tapped. The fluid drawn off on the first occasion was brown and muddy: on the two last occasions it was a yellowish limpid serum. On opening the abdomen there was found, in the situation of the supposed ovarian cyst, an ovoid cavity containing a large quantity of a yellow limpid serosity. This cavity was formed, anteriorly by the great omentum thickened and adhering to the anterior wall of the abdomen, posteriorly by the mass of small intestines adhering together and covered by a false membrane. Inferiorly, this cavity seems to have been formed by the uterus and ovaries reduced to a putrilage.

At this point I may mention the few details given of a case which was probably identical in nature with ours, to be found in a lecture by Sir James Y. Simpson.¹ The patient was a young girl, of twelve or thirteen years of age, in whom he had made sure that inflammation had been set up in the broad ligament of the uterus, and caused great swelling and induration of it. He introduced the exploring-needle with the view of bringing away pus and reducing the swelling. But there flowed a transparent fluid that made an onlooker suppose that the bladder had been punctured. A considerable quantity escaped, and it rapidly coagulated in the cup containing it.

CASE I.—Mrs L., æt. 20, was delivered of a male child in the Royal Maternity Hospital on the 10th September 1855. The labour was natural. She says that for weeks after delivery she was troubled with pelvic pains and rigors, which were supposed to be after-pains. These she describes very vaguely. They continued with variations of intensity till near the end of November, when she first felt a lump on the left side of the lower belly.

On the 27th November, she came to consult me at the Royal Dispensary. Her state then was as follows:—She had feverish symptoms,—quick pulse, hot skin. A large and very tender tumour, easily circumscribed, was felt through the abdominal wall, bulging up through the left half of the brim of the pelvis. It was immovable and hard. The finger examining per vaginam found the cervix uteri high up in the pelvis, displaced backwards and fixed. In front of the cervix, the whole left side of the upper part of the cavity of the pelvis was occupied by the hard tumour, presenting no soft or fluctuating portion. Between the 8th of December, when an examination was made, and the 11th of the same month, when it was repeated, a soft tumour was formed in the recto-vaginal space. I punctured it with a Pouteau's trocar, and a transparent straw-coloured fluid slowly distilled through the canula, to the amount of about eight ounces. On cooling, it became grass-green in colour, and coagulated into a jelly. The slow distillation of the fluid was explained by the coagulation of a leech-like mass in the

¹ Medical Times, 1859; vol. xix. p. 27.

canula, which partially plugged it. The discharge made little difference, except in the recto-vaginal portion of the tumour, which was diminished. On December 14, I again tapped the same part, and drew off about five ounces of similar fluid, and with like results. On December 28, a vaginal discharge began, the nature of which was not decidedly made out. It was not purulent and did not last long. About this time the whole tumour had commenced to diminish in size.

Nothing further was done in the way of interfering with the tumour. The patient escaped my observation during 1856 till November. She was then without much complaint, but sought advice for the restoration of her menses, which had not appeared since her confinement. They were re-established in January 1857, and were regular till her death.

In November 1856, the tumour was found to be reduced to a mass of fixed hardness in its old site. The uterus also was fixed. In the early months of 1857, this fixed hardness rapidly diminished and almost entirely disappeared. She now enjoyed good health.

In the middle of March, she was taken with symptoms of ileus, and after a few days' illness, died. No connexion could be made out during life between her former disease and her fatal illness; and, unfortunately, an autopsy could not be procured.

CASE II.—Mrs N., æt. 38, admitted into the Royal Infirmary November 17, 1864. Has had four children, and has always enjoyed good health. Had an easy labour three weeks ago, and was delivered of a male child.

Four days after delivery, she was seized with pain about the right inguinal region, shooting through to her back. She was then feverish. Soon she felt a lump growing in that part of her belly. She was about this time seen by Dr Andrew Inglis, who regarded her case as one of pelvic abscess.

In the right hypogastric region there is felt a large tumour, extending as high as midway between the umbilicus and pubes, and from the right side of the belly to a little beyond the mesial line. It is scarcely tender now when pressed. Some resonance on percussion can be heard over it. The cervix uteri is high in the pelvis, and pressed forwards against the symphysis pubis. Behind the cervix uteri the upper part of the pelvic cavity is felt to be occupied by a hard bulging mass. The uterus lies in front of the tumour; its cavity is nearly three inches long; the examining probe easily slips through the left Fallopian tube into the peritoneal cavity.

November 19.—The tumour was punctured per vaginam by a Pouteau's trocar, and about nine ounces of fluid, scarcely turbid, and of a light-green colour, were drawn off. The fluid, on microscopical examination, was found to contain a few pus cells. The cervix uteri now resumed its natural situation. The abdominal tumour was reduced to a hardness felt rising an inch and a half above Poupart's ligament.

December 3.—The patient, feeling well, would not remain longer in the hospital. On leaving, no tumour could be felt. But the uterus was fixed, and a hardness occupied the pelvic cavity above and behind the cervix.

She promised to return, if she found her malady not completely removed. But she has not again been seen at the hospital, nor by the physicians who took care of her at her own home.¹

I know of no records of cases resembling those now narrated. The large bulk of the cysts in these cases distinguishes them, as already said, from those described by Huguier. But regarding such cases as I have here given, as of possible occurrence, this distinguished author makes the following remarks:—"Leur volume varie à l'infini, depuis celui d'un grain de millet jusqu'à celui d'un œuf de poule ou même d'une orange. Bien que je n'en aie pas rencontré de plus volumineux, on conçoit qu'il puisse en exister; peut-être même les exemples de prétendus kystes de l'ovaire, qui, au rapport de MM. Sedillot et Camus, ont guéri spontanément après s'être ouverts dans l'abdomen, les deux cas qu'Auguste Bérard me disait avoir guéris à l'aide de l'acupuncture, étaient ils des kystes séreux et volumineux de l'utérus; car on sait combien la simple ouverture ou ponction des kystes ovariens est rarement suivie de guérison." This suggested explanation of supposed cures of ovarian dropsies by simple tapping, or by spontaneous bursting into the peritoneal cavity, is very satisfactory. With M. Huguier, I regard such cures as in a high degree doubtful;—considering, at all events, that, while post-mortem verification of such cures is absent, they are pretty well explained by supposing that, instead of ovarian dropsies, inflammatory serous cysts were the subjects of the treatment.

In my cases it was never a question whether the disease was ovarian dropsy or not. They had characters sufficient at once to remove them from that category. They were rapidly produced under inflammatory symptoms in their site, they were immovable, they were tender to the touch, they displaced and fixed the uterus. In short, they presented all the appearances of ordinary pelvic abscess, and as such they were treated.²

But, though at first supposed to be abscesses, their subsequent history, at least after tapping, showed the error of this notion. For in no stage of ordinary pelvic inflammation or abscess does the plunge of a trocar lead into a cavity, or result in the copious flow of serous fluid. Such a paracentesis, however, does lead into a cavity, and does result in abundant discharge of serous fluid in certain cases of inflammation of the tunica vaginalis testis, of the pleura, the pericardium, the arachnoid cavity, and the cavities

¹ Chiefly from the hospital report, by Dr J. C. Russel.

² See a remark by Dr McClintock on cysts behind the uterus.—*Clinical Memoirs on Diseases of Women*, p. 260.

of the ventricles of the brain. In this place, I shall not enter upon the question whether the not unfrequent inflammatory serous accumulations, which I have enumerated, are identical in nature with one another, or whether any of them is analogous to that described in this paper. I merely wish to point out a source of analogies to be studied in connexion with this topic. Authors may be found (Bernutz, etc.) who speak of serous, fibrinous, and sero-fibrinous inflammations of the pelvic peritoneum. But none describe cases, like the two here related, where the accumulated fluid might justify a comparison of them with inflammatory effusions in the pleura or pericardium.

ARTICLE V.—*On the Galvano-puncture of Aneurisms.* By JOHN DUNCAN, M.A., M.D., F.R.C.S. Ed.

(Read before the Medico-Chirurgical Society, 7th March 1866.)

THE treatment of aneurism has always occupied a good deal of attention, and many methods have been proposed. Though we have arrived at none universally applicable, we have to a certain extent settled provisionally the value of our various means of cure. Compression, perhaps the safest of all, can be used for a considerable number. The injection of perchloride of iron is admirable for aneurisms not on a main trunk, if the circulation in the sac can be temporarily arrested. Where these have failed or are inapplicable, the Hunterian and old operations may be adopted for external aneurism almost according to convenience. Amputation, distal ligature, manipulation, galvano-puncture, have each a field of action; and to the last of these I wish to draw attention in connexion with a case of thoracic aneurism, the only one so far as I know thus treated in Great Britain.

The patient was forty-five years of age, of middle height, rather spare and sallow, square-shouldered, of an energetic disposition. No family history bore upon the disease from which he suffered.

During the last six months of 1863, Mr A. experienced shooting pains in the shoulders and upper part of the chest, but believing them to be rheumatic, called to his aid merely patience and flannel. One morning, however, in December of that year, he discovered a small pulsating swelling in the middle of his chest. An aneurism had perforated the sternum. The perforation was situated in the middle line, at the junction of the manubrium with the gladiolus of the sternum. The tumour was no larger than a hazel-nut, and the normal percussion dulness of that region was not much increased in extent. At this time, and indeed throughout the case, there was no aneurismal bruit, neither was there any alteration of the pupils. The second sound of the heart was slightly accentuated. Through the spring and summer of 1864, the tumour grew slowly to the size of an orange, while the treatment was directed to maintain perfect

health. The patient's only suffering was from occasional wandering pains. But in the autumn, the neuralgia became very severe, and the swelling increased more rapidly. The iodide of potassium, seems to have given relief whenever, in like circumstances, it has been used, and in this case it did not fail. It was administered in full doses for a considerable time, and under it not only did the pain completely disappear, but the tumour actually diminished in size.

The aneurism now, in October 1864, occupied nearly the upper half of the sternum, stretching beyond it above and to the sides; in shape, a flattened hemisphere, quite without nodules, and with a moderate pulsation. From that time it progressed slowly for a year, extending more upwards than downwards, sometimes stationary, never retrograding. Notwithstanding its increase, Mr A.'s general health remained so good that he was able to follow uninterruptedly his usual vocation, concealing the unsightly deformity by comforters and a loose great coat. He was, however, greatly annoyed by flatulent dyspepsia, and grew thinner and paler and weaker. He had little of his former pain, but his nights became restless, his cough troublesome, his breathing difficult when he lay upon his back, and the necessary use of opiates increased the stomacheic disorder. In October 1865, the acetate of lead was tried and pushed to plumbism; for about that time an apparently successful case had been recorded by Dr Owen Rees. It had absolutely no effect. The tumour grew faster than ever, and by the 10th of November had acquired a bulk truly enormous. So high did it reach in the neck, that as he sat, stooping from weakness, his chin rested upon it; and he asked me one day if it might not push back his head by its increase. It covered half of the sternum and of each clavicle, and from side to side over its highest part it measured thirteen inches. It was no longer smoothly hemispherical. Nodulated eminences had arisen here and there. Now one and now another had rapidly increased, reddening the skin, and threatening to burst, but hitherto they had failed to do so. At length, however, there came at the upper part a small vesicle, the size of a pea, perhaps from friction of the coverings. It was smeared with lard, and the softest lamb's-wool was placed between the clothing and the skin. Thus treated, it shrivelled and disappeared in a day or two; but, on the 15th of November, another of the same size filled with bloody serum arose lower down, where the skin was extremely thin. As this tightened and puckered with the pulsation, it seemed as if every moment it would give vent to a gush of blood. It burst in a day or two, but exposed simply the raw surface of the skin as after a blister. This surface then took on a slow ulcerative action, deepening little but gaining in superficialities, while one or two new vesicles formed around it, which either dried up again or added to its extent. Matters progressed thus, while the patient's strength rapidly failed, till, on the 2d of December, a very slight oozing of blood took place, which was stopped by applying the perchloride

of iron solution. It was evident, however, that, if nothing were done, the patient could live but a day or two at furthest, for bleeding from some point of the extensive ulceration must speedily recur. What was to be done had of course been anxiously and often thought over and discussed.

My father had already made an essay with galvanism for the cure of an abdominal aneurism which was threatening to point in the posterior lumbar region. Fifteen minutes after the introduction of the needles, pulsation had entirely ceased, and the operation had been performed without the slightest pain or inconvenience to the patient. Unfortunately this person afterwards resumed his occupation as a post-boy. In a fortnight he rode to Roslin, and indulged in a quite unwarrantable amount of exercise. That night he died suddenly, it was supposed by the medical man who saw him from rupture of the aneurism internally. To the day of his death he felt perfectly well.

The case was encouraging, and it was determined as a last resource to apply galvano-puncture in the present instance. The injection of perchloride of iron was evidently too dangerous, for as each drop formed its coagulum it might easily be carried into the circulation; and the fatal result attending the only attempt to introduce wire into the sac of such an aneurism gave little encouragement for a repetition. On the other hand, galvano-puncture had been attended with a large amount of success, and the needles gave a holding point for the clot during its formation.

It was thought good, therefore, to request the assistance of Dr Wright,¹ whose knowledge of matters relating to natural science is well known, and who had been conjoined with my father in his former experiment. Every thing had been prepared for the evening of 3d December, when unfortunately, at four o'clock that day, I was hurriedly summoned to the patient on account of copious hæmorrhage. It had already greatly ceased under the perchloride of iron, and a little further pressure arrested it entirely, but not before the patient had been rendered very faint and the pulse very feeble. I at once sent for my father and Dr Wright, and also for Professor Simpson, who was desirous of witnessing the operation.

By six o'clock every thing was ready, and the patient was placed on his back upon the sofa. Two needles were introduced, one on each side of the sac not far from its base, and were so placed that they must have overlapped each other by an inch at the point, while lying about that distance apart. The conducting wires were attached after the needles had been introduced, and a very feeble sensation accompanied the closure of the circle. The patient thought that he experienced a slight shock also on its being opened, but nothing was felt during the whole period of galvanic action. Three quarters of an hour after the current began to pass, the intensity of the pulsations had decidedly diminished, and the wires

¹ I may state that Dr Wright undertook the preparation of the apparatus to be used.

were transposed at the battery, so that the positive needle now became negative. In an hour and three quarters the pulsation had still farther diminished, but without any measurable change of bulk in the tumour. By percussion, air could be mapped out on the surface in little wells, principally to the right front. As it increased in quantity pretty rapidly, and its entrance into the circulation was feared, the needles were removed at five minutes past eight o'clock, or two hours and ten minutes after their insertion. Towards the end the pulse had become a little more feeble from the long-continued restraint in one position, which Mr A. bore with difficulty in his exhausted condition, though fortunately he had not experienced that impediment to respiration which was usual when he lay upon his back. Some brandy was administered; he was undressed and put to bed. He was directed not to turn too much upon his face, that all danger from the gas might be avoided.

4th December.—He had passed on the whole a comfortable night, sleeping well towards morning. The pulse was 80, rather feeble. The force of pulsation in the tumour, though greater than last night, was decidedly less than before the operation. At eleven o'clock, a very faint tympanitic note was yet elicited by percussion on certain parts of the tumour, but it disappeared in the afternoon.

At half-past seven in the evening, two needles were again introduced a little higher than before. They were more than twice as thick as the first pair, and somewhat longer; so that they must have overlapped nearly two inches. Their action was much more rapid than on the former occasion. In fifteen minutes there was clear percussion at parts, and the poles were changed. In five-and-twenty minutes the tympanitis was very extensive, and it was thought right to remove the needles. The air, as before, was principally on the right side, but much more was formed in this than in the first operation, and some even escaped along the needles. A very notable diminution in the force of the beats was observable.

5th.—He was restless during the night. He would often start from a doze, in the belief that blood was trickling down his side, and that he was dying of hæmorrhage. In consequence, he was feverish a little, and exhausted in the morning. The tumour in parts gave the sensation of dough, in others, of a half-empty bladder. A few drops of bloody serum escaped by the puncture of the second pair of needles.

6th.—He was extremely well, having passed the night in quiet sleep. His pulse was about 80, stronger than it had been, and he enjoyed taking small quantities of arrow-root, calt's-foot jelly, and beef-tea. In the tumour very little air could be detected, and the pulsation remained moderate.

On the 7th, he suffered a good deal from flatulence, and could take almost no food. A vesicle, dark and containing some air, had arisen during the night. It was higher up than the ulcerated surface, which was still covered by the lint steeped in perchloride

of iron. In the evening, his morphia draught was increased from 60 to 80 minims.

8th.—He had slept well, and felt much refreshed. An ulcer now occupied the site of the vesicle, and, as the discharge was a little foetid, Condyl's fluid was applied. He continued lively throughout the day; but next morning was very much worse, and he rapidly grew weaker, suffering greatly from flatulence and colic, till he died on the morning of the 11th December. The ulceration spread a little during these last days; but there was no hæmorrhage except a very slight staining of the lint from the granulations, and a few drops of bloody fluid from the punctures.

A post-mortem examination was made thirty-six hours after death. The skin was reflected from the sides of the tumour outwards, the clavicles, several of the ribs, and the lower portion of the sternum were divided, and removed along with the contents of the thorax and all the parts in relation to the aneurism as high as the cricoid cartilage. In doing so the left lung was found to be slightly adherent, and the surface of the third dorsal vertebra to be carious, with a little foetid pus and bone debris lying between it, the aorta, and œsophagus. The lungs and heart were healthy.

The tumour was now examined. Over the anterior surface the skin was much discoloured,—here raised in vesicles, there in patches of ulceration. The wall under these ulcerations was of the thinnest, and small perforations admitted the probe; whether or not produced by the manipulations on removal it was not easy to say. They lay principally round a hard black mass two inches in circumference, where the perchloride of iron had been applied. On the sides the skin was normal in appearance. A little extravasated blood lay beneath it, where the second needles had been introduced; where the left one had been, a few drops of pus.

An incision was made into the presternal tumour from side to side, and the clots were examined. These were of three kinds. Radiating outwards from the orifice of the sternum, closely adherent to the wall of the sac, was a firm, smooth, and organized clot, about one inch in thickness, but gradually thinning towards the anterior part of the sac, where it was absent. It could be separated into numerous layers with great facility. In the centre was a granular, somewhat decolorized clot,¹ about the size of the closed fist. It was in parts adherent to the laminated layer, and numerous processes stretched from it to the circumference in all directions. It could also be separated into layers, but with difficulty. A dark line ran through it from the left, and was supposed to mark the course of the second needle. A soft black clot occupied the space (one inch) between this mass and the anterior wall, but was continuous in gradation of colour and firmness with many of the processes among which it lay. The sternal orifice and the arch of the aorta were partially filled by a loose and evidently post-mortem clot.

¹ This was probably produced by the electric current.

The parts were washed, stuffed, and carefully dissected. Over the sides and lower portion of the aneurism the pectoralis major stretched, its fibres lost in front. A few of the most external fasciculi were still attached to the left clavicle, giving thus a measure of the lateral extent on that side. Higher, the sternal muscles were stretched over the anterior, the scaleni and omo-hyoids over the posterior surface of the tumour.

The aorta began to be dilated immediately after its origin, and regained its normal size at the third dorsal vertebra. Its greatest diameter was opposite the sternum, where it was firmly united to that bone, with the triangularis sterni disappearing upon it. Throughout it was perfectly smooth, with one small projection inferiorly at the junction of the second and third portions. Its branches were normal, but somewhat elevated. None of the surrounding parts were compressed by it, but the right recurrent laryngeal nerve seemed a little lengthened. The mucous membrane of the trachea was reddened, and the muciparous glands on the membranous portion were large and injected. The œsophagus was thickened and inflamed opposite the carious vertebra, elsewhere normal.

The interior of the large sac was rough where the clots had been attached, but the sternal orifice and the arch of the aorta were smooth. A few calcareous plates were scattered over the arch, which generally was a little thicker than natural.

The ends of the sternum, the two clavicles, and a rib on each side, were jagged, and projected into the cavity. Of the sternum the whole manubrium had disappeared; of the left clavicle, about an inch and a-half; of the right, considerably more, and about one inch of each first rib. A part of the second ribs also was gone, but their extremities did not project into the sac. The orifice, however, was not so large as this amount of absorption would lead one to expect, for the shoulders, deprived of their support, had fallen forwards. It would admit four fingers, placed conewise up to the second joint.

Although galvano-puncture has frequently been performed, it is so little known in this country, and the accounts of it in our systematic works are so inaccurate, that I may be allowed a few words on the subject. There is a doubt as to the first proposer; but the question is of little importance, and I shall merely detain you to remark that it was proposed in 1832,¹ and shortly afterwards put in practice. Liston gets the credit of having early performed the operation on a subclavian aneurism. He did not do so; but when Phillips, in 1847, wrote to the *Archives Générales*, claiming priority, and stating that he operated in the case, not Liston, who merely referred to it,² the editorial reply was curt,—“We never heard of your papers; Mr Liston makes no mention of you; how could we divine the name of Phillips?”³ A great name often attracts farther credit.

¹ *Vide Archives Gén. de Méd.*, 1847, p. 526. ² See *Lancet*, 1838, p. 669.

³ *Archives Gén. de Méd.*, 1847, p. 250.

These early and unsuccessful attempts were lost sight of for ten years ; but electro-puncture was revived by Petrequin in 1845, and a considerable amount of experience has since been accumulated. Many experiments have been made on the occlusion of arteries in animals, chiefly by M. Abeille,¹ and by commissions of the Milan Scientific Society. Their results were thoroughly satisfactory.

Thus, in eleven experiments on dogs and sheep, M. Abeille invariably found, on killing the animal a few days afterwards, a firm and even laminated clot closely adherent to the vessels. M. Asson reports, as the result of the labours of an Italian committee, that galvanism is a mode of producing a firm obstructing clot, which is composed of fibrinous granulations, connected together and adherent to the walls of the vessel. It has been asserted that this is due to inflammation excited by the needle or galvanic current in the walls of the artery ; but I have found that, if the animal be killed immediately after making these experiments, a clot already exists at the point operated on ; and Phillips observed that it required forty-eight hours to obstruct a vessel by needles through it.

About sixty cases of aneurism or other tumours containing blood have been treated by this method in the human subject, and the following table will give an idea of their nature and the results. It is copied from Ciniselli's Memoir on the subject,² with certain additions.

Site of Aneurism.	No.	Cured.	Not Cured.	Deaths.
Thoracic aorta, including Mr A.'s case,	4	...	4	...
Carotid, common, ...	4	...	1	3
Subclavian, ...	3	1	1	1
Lingual, in neck, ...	1	1	...	...
Temporal, ...	1	1	...	...
Ophthalmic, ...	1	...	1	...
Brachial, bend of elbow, ...	15	10	5	...
Radial, ...	2	1	1	...
Palmar arch, ...	1	1	...	...
Abdominal aorta, ...	1	...	1	...
Sciatic, ...	1	...	1	...
External iliac, ...	1	1	...	...
Femoral, ...	3	1	1	1
Popliteal, ...	11	5	4	2
Site unknown, ...	2	1	1	...
Cricoid, ...	3	1	2	...
Pulsating tumour, head of fibula, ...	1	...	1	...
Varix, ...	3	3	...	...
Totals, ...	58	27	24	7
Leaving out veins and tumour, ...	54	24	23	7
Totals of Ciniselli's original table, ...	50	23	20	7

¹ Archives Gén., 1849, p. 491.

² On the Electro-Puncture of Aneurism. Cremona, 1856.

The table at first sight looks unfavourable, but various circumstances must be taken into account. It must be remembered, on the one hand, that the third column includes the ameliorated, and, on the other, that many of the cases were beyond the reach of ordinary treatment, nay, beyond a hope of cure. Thus, in the death line, is a carotid aneurism which was complicated by fistulæ from suppurating glands, by hydrothorax, and by "grave nervous phenomena" from pressure on the pneumogastric. On the whole, if it be considered that the operation was often imperfectly performed, it may be taken as proved, at least, that aneurisms can be cured by this means.

Nor on comparison with the mortality list, in ligature of arteries, is the disadvantage with galvano-puncture, as the following will show :—

Ligature from Norris's Tables.

	No. of Cases.	Deaths.	Per-centage.
Subclavian, . . .	69	33	47·8
Carotid, . . .	149	54	36·2
Femoral, . . .	204	50	24·5
External iliac, . . .	118	33	27·9
Internal iliac, . . .	7	3	42·8
Common iliac, . . .	32	25	78·1
Total, . . .	579	198	33·1
Galvanism, . . .	54	7	12·9

Compare, again, the second and third columns of our galvanism table with the statistics of compression, as arranged by Mr Hutchison in 1856, which include the four previous years—a period when that method was not in its infancy.

Cases.	No.	Success.	Failure.
Femoral, . . .	5	0	5
Popliteal, . . .	46	24	22
Ant. tibial, . . .	1	1	0
Post tibial, . . .	1	0	1
Radial, . . .	1	0	1
Totals, . . .	54	25	29
Galvanism, . . .	54	24	30

The mortality columns cannot be compared, for, although 7 died out of the 29 failures by compression, the deaths of several are attributable to subsequent amputation or ligature. Compression has undoubtedly been greatly improved, and in the statistics published by Mr Hutchison in 1860, out of 39 popliteal aneurisms 25 are given as cured, and only 14 failed; but I have chosen the other for comparison, because I believe electro-puncture is also capable of being perfected.

It is of importance, in estimating the value of an operation, that

the frequency and the sources of danger be thoroughly understood. In Holmes' Surgery it is stated, that of 43 galvano-puncture cases, 13 had been the subjects of grave, the remaining 30 of slight, accidents. The table is taken from Ciniselli's pamphlet, but with an important omission as an exact copy proves.

23 Cured.	{ Without or with slight accidents, . . .	17
	{ With grave accidents, . . .	6
20 Not cured.	{ Without or with slight accidents, . . .	13
	{ With grave accidents, . . .	7

Add to this, that there were about 70 applications of electricity to these 43 aneurisms, and it will be seen that the above statement is by no means a just representation of the dangers. The most common and important accident which follows electro-puncture is inflammation of the sac or of the surrounding tissues. More than half of the total number of patients thus treated have suffered from it in a greater or less degree. In the sac it may be produced by the simple rapid deposition of a clot, whatever be the means employed; but much more often in the cases before us it was excited by the chemical action of the electric current. This, however, is a form of the accident, which, although it may be followed by unpleasant or even fatal results, yet not unfrequently, when slight, tends to a prosperous issue by its own proper action. In fact, a very considerable number of the cures have been consequent to it. A much more serious effect of the current is cauterization of the skin or of the aneurismal wall in the course of the needles. This has occurred only too often, and has led, as might be expected, to troublesome complications in the shape of abscess, sloughing, and hæmorrhage. In some a slough has been at once produced, and bleeding has accompanied its separation, or has been restrained only by the coagulum deposited at the same time, or by the use of styptics over the ligature. In others an abscess has formed in the wall after a varying interval, and has opened externally, in many, fortunately, with no bad result.

The most intense pain is often experienced during the operation, so great, indeed, as to cause convulsions and alarming syncope. A patient of M. Petrequin¹ utterly refused to allow a second introduction of the needles, though success seemed certain. M. Abeille,² in describing his very successful subclavian case, states that the patient, having emerged from the influence of sulphuric ether during the operation, uttered the most piercing shrieks, and was fearfully convulsed. "She poured," he says, "with sweat. No surgical operation can cause such tortures."

This pain, however, is found to be present only when the other accidents I have mentioned are so; and many have experienced nothing further than the shock upon opening and closing the current which was felt by Mr A. This shock, of course, must generally exist; for, although the blood itself is not sensitive, it

¹ Archives Gén. de Méd., 1847, p. 527.

² Ibid., 1849, p. 491.

conveys the electricity to the wall, just as, if the hand be immersed in water when an electric current is acting, the influence of the wires will be felt even though they be not touched. Such a sensation, however, is hardly perceptible except with a powerful battery. Pain would, indeed, be felt were the interrupted current used, as it not unfrequently was; but for other reasons that form of galvanism ought not to be employed.

Leaving out of account the accidental puncture of a nerve,¹ pain then depends on cauterization in the aneurismal wall. This, as also the main source of danger, we must aim at avoiding, and I think we are in a position to do so. In most of the operations the needles were coated with varnish, having a certain length at the point uncovered. But this was found to be unsatisfactory. The varnish was imperfect or stripped in introduction, and cauterization still often took place. Restelli² complains that in conducting his experiments he could get no varnish to effect perfect insulation. Ciniselli, impressed with the fact that this was almost the sole source of danger, and believing that no insulation could be made perfect, supposed that he could prevent the action on the tissues where it is most serious, viz., at the negative pole, by passing through it a current of positive electricity in the first place. The object may be attained, as was proved in Mr A.'s case, by coating the needles with glass; although it also has the disadvantage of being liable to strip. One of them did so, and it was withdrawn, covered with sealing-wax, and re-introduced by its original puncture. Dr Wright had suggested the use of vulcanite, which could be polished to perfect smoothness with the rest of the needle, but, through a mistake of the maker, glass was applied to the second as well as the first set. There is, moreover, a little difficulty in applying vulcanite. It is evident, however, that glass, sealing-wax, caoutchouc, or gutta-percha, will act as perfect insulators, and can be readily enough applied to the metal. I have found them to be so in various experiments, and prefer, on the whole, a mixture of shellac and gutta-percha. If care be also taken to avoid touching with the unprotected point any portion of the inner surface of the sac, there is no danger from chemical decomposition; while thermic action is easily prevented by merely keeping the electrodes sufficiently apart.³

Another source of danger may be avoided by a very simple precaution. The needles should be of the smallest size consistent with sufficient strength, for the tense wall of an aneurism is liable to inflammation from slight causes. In Mr A.'s case the first pair were fine, and their course was invisible at the post-mortem examination. With a view to more rapid action the second pair were made as thick as stocking wires, and the consequence was the escape of air

¹ Dr Bellingham punctured the sciatic nerve.

² Gazette Médicale, 1847.

³ Dr Bellingham, and, if I remember rightly, Sir Everard Home, cured aneurisms by the introduction of heated wires.

along their sides, and slightly into the cellular tissue, extravasation of blood in their course, and on the left side the formation of a drop or two of pus. The electric action may be increased, if required, by increasing the number of electrodes, which might be united together after insertion. Several minute punctures are evidently better than two large ones. Such a course would, moreover, afford a large number of points for subsequent fibrinous deposit, if the sac were not filled with coagulum at one sitting. If, again, it be true that slowness of formation adds to the firmness of the clot, we may thus have it so without retarding the operation as a whole. Not that we should follow the example of the operator, who inserted no less than fifty needles into a popliteal aneurism, and who had, on withdrawing them, to combat hæmorrhage, not a little alarming.

The dangers of introducing air into the circulation, and of producing emboli, have been much discussed. No inconvenience has been experienced from either in the past, and but little apprehension need be entertained for the future. The tough gas-containing froth tends to rise to the surface, is very little likely to be carried into the circulation, and is so rapidly absorbed that it can never pass the capillaries. Moreover, to produce untoward effects, the introduction into the heart of a quantity of gas much larger than can thus be produced would be required.¹ The clot, again, adheres so closely to the needles, and is in itself so consistent, that with difficulty could any portion be carried away. Many experiments have been made on animals with arteries unligatured and uncompressed, and in none, so far as was observed, did gas or emboli pass down the vessels. In the human subject the artery has often been obliterated along with the aneurism, and even then no embolism has ever taken place, though the arteries were leading trunks of the body.

We have thus glanced at the dangers. Let us examine now the modes in which cures occur. They are three in number.

1. In a few cases the surgeon, on concluding the operation, fears that it has failed; but, after some hours or days, observes the tumour gradually become harder, till at length its pulsation disappears, and in time it is slowly absorbed.

2. Sometimes, on the other hand, the pulsations cease, and the aneurism becomes firm during the action of the current. One of two results may then follow. With the majority condensation and absorption proceed; but pulsation may, I believe, return, through partial melting of the clot, and the aneurism remain half cured, or be filled up by subsequent deposit of fibrin.

3. A third mode is by inflammatory action, which begins to show itself in some almost immediately, in others after the lapse of a day or two, by the usual local and general symptoms. These subside

¹ Nysten found that it required from 40 to 120 cubic centimetres of air suddenly introduced into the jugular vein.—*Sur l'Introduction de l'Air dans les Veines.* Bouillaud.

(but rarely without giving ground for alarm), and the tumour is left fully solidified.¹

In order of frequency, cures have taken place most often with inflammatory accompaniment; next, by immediate and complete coagulation; lastly, by partial clot, with subsequent fibrinous deposit. But the most frequent is precisely that which we wish to avoid; and our efforts must be directed to attain such thorough mastery of our instrument as may enable us to produce invariably one of the two last mentioned. We approach this certainty by ascertaining the causes of accident; but a knowledge of the blood-changes induced by electric currents would greatly assist our endeavours. Various theories have been framed to account for the coagulation, but none of them are entirely satisfactory. According to some it is a special action of electricity unknown and *sui generis*; while others hold that an elevated temperature in the traject of the current is the efficient cause. But the most popular view held by Bellingham, Restelli, Apjohn, and others is, that the acid produced at the positive pole by decomposition of salts in the blood is the agent in coagulating the albumen. Althaus² and others even go so far as to recommend that the negative pole be not inserted at all, but applied to the aneurismal wall as a flat plate. This is not only unsatisfactory, because, with a sufficient battery the skin might be cauterized, and the decomposing power lessened, but is founded on a theory, to a certain extent, erroneous.

To determine the point we are discussing many experiments have been made on all kinds of animal fluids, and with the most contradictory results. Passing these over let me refer to some observations made by Smee and Van Deen, which are especially valuable, because they were made without the slightest reference to the medical application of electricity. Dr A. Smee³ has shown that by passing oxygen through an albuminous fluid, a substance resembling fibrin may be obtained, and that this transformation is assisted by a temperature of 98° to 110°, by making the fluid slightly acid, and by the presence of a serous membrane in it. He has also shown that by the passage of hydrogen through albumen, jelly-like and viscid substances may be produced, which he calls artificial chondrin and mucin, and that alkalinity assists their formation. These actions are extremely slow, are rapid only when the oxygen and hydrogen are produced by the electric current from the fluid itself, and then probably there are other agents at work. It is interesting to note that Van Deen,⁴ in a series of experiments on the chemical action of electricity obtained from albumen, among

¹ For instances of these various modes. See cases by Eyre. *Lancet*, 1853; Nélaton, *Gazette des Hôpitaux*, 1862; Petrequin, *Arch. Gén.*, 1847 and 1849; Comtes Rendu, etc.; and the Memoirs on the subject, by Ciniselli and Schuh.

² *Med. Times and Gazette*, 1862, August 30, September 13.

³ *Roy. Soc. Trans.*, 1863.

⁴ *Archiv zur Natur- und Heilkunde*. Donders, 1862.

other things, a substance which he characterized by putting the word fibrin within brackets, and with a mark of interrogation after it.

With the help of my friend Dr Fraser, to whose skill in these matters I am largely indebted, I commenced some experiments on albuminous fluids. They are by no means sufficiently numerous or varied, and we intend to continue them; but some of the results we have already obtained may not be uninteresting. We used a Bunsen's battery of six cells, with generally platinum wire at the extremity of the conductors, and contrary to the assertions of Baumgarten, and more especially of Steinlein, the platinum proved a most admirable pole.

From experiments on white of egg we obtained the following facts:—

1. With the continuous current the characteristic action was produced rapidly, even with the poles four inches apart; but it increased as the electrodes were approximated, till, with one inch between them, they so coagulated in half an hour nearly one cubic inch of the fluid that nothing was spilled on the inversion of the glass. Parallelism or obliquity of the poles made no perceptible difference.

2. The interrupted current was almost inert till the electrodes were brought within a quarter of an inch of each other, and even then the coagulation was very slow, though of the same character as the other. As to the nature of the coagulum, our results correspond very much to those obtained by Smeec, varying somewhat according to the specific gravity and nature of the fluid. We have acted on pure albumen of high and low density, on impure albuminous fluids, such as serum, defibrinated blood, the contents of a hydrocele, as well as upon blood itself in and out of the body. In every instance have we obtained a coagulum, and everything tends to the conclusion that in an aneurism the froth and jelly which arises at the negative electrode, would last, at all events, sufficiently long to form a most efficient nucleus, while the opaque mass at the positive pole would permanently remain unaffected, either by the water or any amount of alkalinity in the blood. It would make this paper too long to detail our numerous experiments, in the course of which we met with several interesting facts, which we propose to arrange in another essay. Let me only say, that in the blood itself we have observed that three substances are produced, viz., a viscid froth by the negative electrode, a black coagulum very distinct in the midst of florid arterial blood at the positive, and in addition to these a quantity of dark defibrinated fluid. In the living arteries, also, we have succeeded in producing coagula with each pole separately, as well as with the two in the same vessel. It is difficult to estimate the value of the treatment by galvanism. Judging simply by its past performances, many have thought that it should be restricted to aneurisms beyond the reach of other means, or where such means are so dangerous as to be practically

unavailing. I have shown, however, that most of the accidents have arisen from faulty apparatus and manipulation; and I believe that if the following precautions were taken, our statistics would be extremely favourable:—

1. The needles should be carefully insulated.

2. They should be prevented from touching each other or the sac, and to attain this are best introduced from the same side, parallel to each other, and from one to two inches apart. Thus, also, their action, while sufficiently rapid, is not too localized.

3. They should be extremely fine, but may be multiplied according to the size of the aneurism. They should be made of steel, but had better be coated with gold, as the steel is eaten away with great rapidity, and the surface acting is thus diminished.

4. A battery of medium strength, and with a continuous current, should be employed. Four to eight of Grove's or Bunsen's cells are sufficient for a large aneurism.¹

5. The action may be continued till pulsation stops, or gas can be clearly detected by percussion.

Our knowledge of the immediate action of electricity on the blood is not yet so thorough that we can confidently predict success, but we can, I think, so eliminate sources of danger, that only want of success will be the result in any case. Electro-puncture might, therefore, be employed, and for myself I would not hesitate to employ it wherever the arterial orifice is of moderate size, and I believe it might even be farther extended. Cirroid aneurisms, and those not seated on the main artery of a limb, are, of course, especially well adapted for its use; and in cases similar to Mr A.'s it affords the only, but that a good hope of relief.

It is deserving of much more attention than has been accorded it in this country, were it only on account of the admirable cures effected by Peterquin, Abeille, Ciniselli, and Nélaton.

ARTICLE VI.—*Traumatic Idiocy*. By ARTHUR MITCHELL, A.M., M.D., F.R.S.E., Deputy-Commissioner in Lunacy for Scotland.

THE idiocy of Scotland is the result of a great variety of causes. Some of these produce a large proportion of the whole, and others again a proportion which is comparatively small. For instance, the diseases of infancy and childhood,—and, foremost among these, hooping-cough, scarlatina, and continued and remitting fevers,—are rich producers of the idiocy which is among us; while, in what we understand by the term accidents, the *origo mali* of a small number of cases is to be found. Though small, however, the number is not so small as to be insignificant; and since the cause is an ex-

¹ In the case I have related four cells of a Bunsen's battery were used in the first, eight in the second operation.

istant and a constant one, it cannot be regarded as unworthy of notice.

Many married women are obliged, or conceive themselves to be obliged, to contribute to the support of the household by work done away from their own homes. During their absence the younger children are often committed to the care of the elder—themselves but young, and quite unfit for the task imposed on them. That this neglect of young children should lead to the occasional occurrence of accidents by blows, falls, fire, water, or exposure, is not surprising.¹ Even under the most efficient supervision of children such accidents occur, and assuredly they will more probably do so when the arrangements made for their care involve something perhaps worse than simple defect.

Accidents of the character described, and arising under the circumstances alluded to, are of frequent occurrence. The great majority of them of course are slight, and inflict no permanent injury on the children. Not a few of them, however, are of great gravity, and prove fatal. Others, again, though not so serious as to cause death, still lastingly injure the bodily, or both the bodily and mental health.

To these last I have had my attention often drawn, and from investi-

¹ Accidents alone are here alluded to as the result of this neglect of children, but they constitute only one of many unfortunate results, and perhaps the least important of all.

I cannot tell how many lives of children would be annually saved to the country, if all mothers of families could remain constantly at home; but I have seen enough to convince me that the number would be very considerable.

All through infancy and childhood the absence of the mother is productive of great moral and physical injury to her children, and society eventually pays for this. The effect on the death-rate is most felt while the children are very young. M. Dollfus, the great mill-owner at Mulhouse, struck with the high mortality of the young infants of women engaged in the cotton factories, conceived the idea of paying lying-in women their weekly wages for six weeks, in order to allow of their remaining at home during that period, and bestowing the necessary care on their new-born infants. The result was that the mortality fell from a mean of 36 or 38 per cent. to below 25 per cent. (*Vide Med. Times and Gazette*, 3d December 1864). What it would have fallen to if they had remained twelve instead of six weeks at home, I cannot say, nor can I tell what further fall would have followed their remaining always at home. But this I believe to be certain, that both falls would have been well marked.

I have more than once had the opportunity of visiting, at Brussels and Vienna, Les Crèches—institutions for taking care during the day of the young children of women who work away from their own homes,—and I have frequently been told by the superintendents that the milk of women, whose occupations were laborious, did not appear to nourish or do well with their infants, being apt to produce diarrhoea. The case of women employed in the public washing establishments was often adduced as an example. I was not prepared to find the evil manifesting itself in this way, having been in the habit of thinking that the injury to the child was the result mainly of general neglect and discomfort, long-fasting, and exposure to cold.

It must not be supposed that the only idiosyncrasy traceable to this neglect of young children is that originating in accidents, for this would be very far indeed from the fact.

gations, which I have had the opportunity of making, I am led to the conclusion, that *at least* 2 per cent. of all the idiocy in our land is strictly traumatic in its origin. I do not include in this estimate defects of mind, arising from injuries sustained during the act of parturition—chiefly from prolonged pressure of the head in the pelvis. We have the 2 per cent. exclusive of these. At first sight this seems to express a small result; but it loses that look as soon as we begin to deal with thousands of cases, and when we begin to perceive that it is the steady product of a cause still, and in practice continuously in operation.

Judging from what has come under my own observation, idiocy from *accidents* appears to be more common in rural than in city districts. I see no clear reason why this should be so, unless perhaps it may arise from a greater use by city mothers, in the management of their children, of *soothing syrups* and such like mischievous abominations,—in which case toxic idiocy ought to be more frequent in towns than in country districts, and this I believe to be the fact. It has been suggested to me that the residence of the mill-worker is nearer to the place of her work, that she can see her children more frequently, and that she can more easily find a neighbour to exercise a supervision during her absence, than is the case with the field-worker, and that in this way we may explain the difference, which I think I have observed, between the frequency of true traumatic idiocy among urban and rural populations.

The foregoing remarks point almost entirely to idiocy occurring among the lower classes, and direct attention to one source only of accidental injury,—viz., neglect and careless management in the rearing of children. But traumatic idiocy occurs among all classes, and the circumstances which lead to the accidents causing it are very various. These points I shall endeavour to bring out in the illustrations of my subject, which follow.

It is not easy to classify all those accidents, of which I have encountered idiocy as a sequence, but I think it will be convenient to make an effort to do so, and I shall accordingly divide them into four groups.

Before proceeding to discuss these, however, it is necessary to make this general remark. When an idiot is presented for examination, and inquiries into his history are made, it happens from time to time that an *accident* is spoken of in such a way as to show that his defect is regarded as due to it. This leads to further investigation, first, as to the exact nature of the accident, so as to establish its probable sufficiency or insufficiency to produce what has been attributed to it, and then as to the existence of other points in the patient's history, which might more satisfactorily, or as satisfactorily, account for his mental defect. It happens, as the result of this, that the inquirer is left in doubt about some cases, while in others he convinces himself that the idiocy is not the product of the accident in question, and in others again that it is

so. Cases of the last kind alone are used in this paper, and, this being understood, they will be referred to as briefly as possible.

I. *Blows, Falls, or Mechanical Injuries to the Head.*

Obs. 1. A stage-coach was driven over a little boy, who unexpectedly attempted to cross the road in front of it. His age was three years at the time, and he was healthy and vigorous in mind and body. He was severely injured, but the exact nature of the injuries could not be ascertained. This is known, however, that his life was considered in danger, and that, when he recovered, he was hemiplegic and had lost the power of speaking. As he grew older, it became quite evident that he was an idiot.

Obs. 2. A cart-wheel passed over the head and body of a child two years old. His arm was broken, there was bleeding from the ears, and one eyeball was much injured. There is every reason to believe that this boy was perfectly sound before the accident. For many days after the occurrence he was in a precarious state. He is now a speechless one-eyed idiot, but not paralytic.

Obs. 3. When nine months old, a child was knocked out of its nurse's arms by a horse. Several of its ribs were broken, its lower jaw was broken, and other injuries about the head were sustained. A suspicion of mental defect soon followed this accident, though previous to it the child was regarded as bright, merry, and full of health.

Obs. 4. The parents of a young woman, who had an illegitimate child, turned her out of her home soon after the birth of the child. She took low lodgings in one of our large cities. Her landlady was a drunken Irish woman and kept a disorderly house. In the course of a quarrel and fight, which took place there, the child, then seven months old, was struck with a ladle on the head. The right eyeball was in some way pushed out and completely lost, and severe injuries were inflicted, which were thought likely to prove fatal. At the time of the accident, the child was vigorous and healthy and sucking well, but "she never got right over it," and grew up a weakly puny idiot.

Obs. 5. When labouring under an attack of puerperal mania, the mother of a young child struck and ill used it in such a way as almost to kill it. Among other injuries inflicted was the gouging out of one of the eyeballs. The child was seized with convulsions on the day following, and has grown up an epileptic imbecile.

Obs. 6. A little boy, three or four years old, managed to climb over the balustrade of a staircase, and fell from a considerable height on his head. He remained insensible for a considerable time, and on recovery was found to be paralytic. His imbecility is now quite marked.

II. *Accidents by Fire.*

Among the children of the lower orders, fire is a fruitful source of accident—often leading to death, but oftener still to bodily

deformities. Only in three or four cases, however, have I encountered cases in which idiocy could be clearly traced as the product of such accidents.

Obs. 7. In one of these a very young child was left by its mother in the cradle by the fireside. During her absence, it is believed that a dog, which was also left in the house, attracted by the crying of the child, had upset the cradle in a friendly effort to look at its occupant. The child rolled to the fire, which was an open one, and its clothing was ignited. There is still good evidence that the injury was of a very frightful nature, and there is every reason to believe that the child with difficulty survived it. During the night following the accident, the child was convulsed, and next day it was noticed that he squinted, which he continues to do. He is now a restless, troublesome, and mischievous idiot.

Obs. 8. In another case, a little boy had been left in bed by his mother, under the care of his sister who was some seven or eight years old, and who naturally joined some of her little friends in their plays outside, forgetting her charge in her own enjoyment. While she was out, the child managed to creep out of bed and to set fire to his scanty night-dress, which was the only clothing on at the time. His screams attracted the attention of a passer by, who gave immediate aid. The burning in this case was not very severe or extensive, but the fright or terror of the child seems to have been something quite extraordinary. Nothing would soothe or quiet him, and in an hour or two he was convulsed. He has had fits at longish intervals ever since, and is now decidedly imbecile.

III. *Accidents from Exposure.*

Obs. 9. When about three or four years old, a child fell into a rivulet on a very cold and frosty day. He crept out and lay down on the bank, and when found his dress was frozen to it. Next day he was discovered to be paralytic and a severe illness followed. He is now idiotic, but no sign of defect either of mind or body had been suspected before the exposure.

Obs. 10. At the age of four, a little girl, previously regarded as quite healthy, lost herself among the hills, and was not discovered for three days, during which time she was without food. She was found in a state of great exhaustion, and for some time it was doubtful if she would recover. A short time after the exposure, it was discovered that she had partially lost the power of one arm and one leg, and this she has never regained. She is now a well-marked imbecile.

IV. *Accidents connected with Parturition.*

It is not my intention here to do more than allude to that frequent idiocy following the injuries which children sometimes sustain in the act of parturition, and which are produced in the majority of cases by lengthened pressure of the head in the pelvis.

These accidents have been discussed in two papers on the connexion of difficult parturition with idiocy, which appeared in the Medical Times and Gazette some years ago. Idiocy of this kind is of course traumatic—as much so as if the head had been crushed by the passing of a wheel over it. But I do not specially notice it here, for the reason that I have done so elsewhere.

A few cases of idiocy, however, have presented themselves to me which have appeared to be traceable to accidents following parturition. For instance:—

Obs. 11. A woman was delivered of a child when alone in a cottage on the hillside. Before help came, the child had been for three hours on a clay floor, the mother being beside it on her knees with the placenta undelivered. This occurred in the depth of winter. The child never sucked well, and was “paralytic from birth.” It is now a well-marked imbecile.

Obs. 12. In another case the idiocy appeared to be due to an excessive loss of blood from the cord very shortly after birth. The child is described as having “fainted,” and it is said there was great difficulty “in bringing him round.” Some days after his birth, he had a convulsive fit. He never throve, and as he grew up became an acknowledged imbecile.

These twelve observations are selected in order to illustrate the nature of those accidents to infancy and childhood, which may be followed by idiocy. It would have been easy greatly to have increased their number, but it is thought that they are sufficient for the purpose—the chief object of my paper being to show that there is among us an appreciable amount of idiocy resulting from accidents, and that many of these are of a preventible character.

General Remarks.—It must not be supposed that the term Traumatic Idiocy has any important psychological significance. The patients who would be set down as labouring under it, are associated by little else than a certain similarity in the cause of their condition, and the term Traumatic Idiocy is simply convenient when it is wished to indicate this. It points to no particular character of mental defect, and includes many of the different forms and degrees of idiocy. Paralysis and epilepsy would probably be found in frequent association with it as complications, but they are at least as often absent as present.

There are, however, certain forms of idiocy, which patients properly classed as labouring under traumatic idiocy never would exhibit. One of these, for instance, which is detached from the other forms of idiocy by lines quite as clear as those which separate mania from melancholia, has invariably an intra-uterine origin, and there is good reason to believe that the abnormalities which it presents date from the early months of pregnancy. A case of this kind could not be classed as one of traumatic idiocy, in the meaning at least which we have here given to that term.

Part Second.

REVIEWS.

A Practical Treatise on Urinary and Renal Diseases, including Urinary Deposits. By WILLIAM ROBERTS, M.D., F.R.C.P., Physician to the Manchester Royal Infirmary, etc. London: Walton and Maberly: 1865.

DR ROBERTS is already well known to the medical profession as an able writer on several important subjects. In particular, his essay on "Wasting Palsy" indicated powers of a high order, and was a very valuable contribution to medical literature. Urinary diseases have long been one of his favourite studies; and in the present volume he gives us the results of his matured experience.

Dr Roberts' work is divided into three parts: the first treats of the chemical and physical properties of the urine in health and disease, and includes urinary deposits; the second, of urinary diseases, of which the chief characteristic is an alteration of the urine; the third, of organic diseases of the kidney. The plan, it will thus be seen, is very complete, and the manner in which it has been carried out is in the highest degree satisfactory. We agree so entirely in most points with Dr Roberts that our notice of his work must consist mainly of laying before our readers his opinions on some special points.

It is highly important that every physician should be familiar with the changes which the urine undergoes in keeping, but we believe that the statements contained in the following passage will be new to many:—

"The changes which take place in urine after emission are a frequent source of misapprehension. Taking the reaction of the urine as a guide, these changes may be said to take place in two opposite directions, namely, towards excessive acidity on the one hand, and toward alkalescence on the other.

"1. It has been found that healthy urine, when exposed to the air, undergoes a regular series of spontaneous changes, to which Scherer gave the name of *acid urinary fermentation*. The main feature of this process is a progressive increase of the acid reaction. As a consequence of this, there usually occurs, first, a precipitation of the amorphous urates, then of uric acid, and often of oxalate of lime. Frequently, likewise, confervoid vegetations, either the mould or sugar fungus, make their appearance. The acidity goes on steadily increasing for some four or five days, sometimes for a week or ten days, and then begins to decline, as the urine passes into a state of putrefaction. It now becomes opaque from the development of myriads of minute linear particles (*vibrios*); the odour and reaction of ammonia, together with an offensive effluvia of putrescence, become perceptible. The amorphous urate deposit will now be found changed into dark round masses of urate of ammonia; uric acid crystals give place to bright prisms of triple phosphate, and an abundant sedi-

ment of amorphous phosphate of lime sinks to the bottom of the vessel. The confervoid vegetations cease to grow with the change of reaction; and finally perish as the secretion becomes fairly putrid.

"But matters do not always pass thus. Urines of low specific gravity, or of low acidity, either do not pass through this cycle of changes at all, or do so in a very imperfect manner. Their acidity undergoes no appreciable increase; and in a day or two, or even in a few hours, they become ammoniacal.

"The increased acidity which occurs in the progress of the acid urinary fermentation depends chiefly on the generation of lactic acid, partly also on the production of acetic acid. Scherer believes that the mucus of the bladder acts as a ferment on the urinary pigment, and transforms it into lactic acid. Like other fermentive processes, this one is impeded or arrested by alcohol and by boiling; also by removing the ferment (the vesical mucus) by filtration.

"2. The changes which take place in an opposite direction, that is, towards alkalescence, are much more prone to mislead than those just described. The transformation of urea into carbonate of ammonia is a frequent source of confusion in the examination of the urine. This transformation is brought about with great rapidity by contact with any decomposing organic matter, especially by the contact of decomposed urine. The physical and chemical characters of the secretion are then so altered, that it is unfit for clinical examination, and should invariably be rejected, except in cases where the transformation takes place within the urinary passages, and a more natural specimen is therefore not procurable."

The characters of the different deposits are very well described, and the microscopic appearances they present are illustrated by numerous well-executed engravings. The subject of oxaluria, and the clinical importance of the persistent presence of crystals of oxalate of lime in the urine, has excited much attention, and given rise to much controversy. While some have elevated the group of symptoms said to accompany the deposit into a special pathological state under the name of oxalic-acid diathesis, others regard the existence of oxaluria as merely one of a group of symptoms, the result, as Dr Roberts expresses it, of "a nervous system upset, because overtasked, and a digestion deranged, because mismanaged." This is entirely our own opinion; and though we consider oxaluria important as a symptom, especially in pointing to the treatment to be followed, we believe that the following propositions quoted by Dr Roberts from Beneke are well founded:—

"1. Oxaluria, a condition which accompanies the lighter or severer forms of illness, has its proximate cause in an impeded metamorphosis—that is, in an insufficient activity of that stage of oxidation which changes oxalic acid into carbonic acid.

"2. Oxalic acid has, if not its sole, its chief source in the azotised constituents of the blood and food: everything, therefore, which retards the metamorphosis of these constituents occasions oxaluria.

"3. Such a retardation of the metamorphosis of the azotised constituents of the blood may be determined by the following causes:—

"a. Abuse of azotised articles of food (direct retardation).

"b. Abuse of saccharine and starchy articles of food (indirect retardation).

"c. Insufficiency of the red blood-corpuscles and (eventually) diminished oxidation.

"d. Insufficient enjoyment of pure, fresh, ventilated air.

"e. Organic lesions which in any way impede respiration and the circulation of the blood.

"*f.* Conditions of the nervous system which bear a character of depression, whether these arise primarily from mental derangement or from pathological states of the blood.

"4. Excess of alkaline bases in the blood: which, as numerous observations tend to show, plays an important part among the ætiological conditions of oxaluria; and it is not improbable that an increased production of lactic and butyric acids in the digestive canal, consequent thereupon, impedes the development of the red blood-corpuscles, and thereby generates that chlorotic state which so often occasions and accompanies oxaluria.

"5. Catarrhal conditions of the intestinal mucous membrane, in case they are accompanied by oxaluria, have at most only a common source. They may determine oxaluria by causing deranged digestion, but cannot be considered as its proximate cause."

The remarks regarding the treatment of the condition associated with oxaluria are equally judicious:—

"After the foregoing reasoning and conclusions, it is scarcely necessary to say that oxaluria does not, in the opinion of the present writer, furnish special indications for treatment: nevertheless it will be found that, apart from the existence of organic disease, the conditions most frequently found associated with oxaluria, varied as they are, call for a tolerably uniform therapeutical action. They demand a quickening of the oxidation processes, and a careful regulation of the diet. The skin should be encouraged to activity by systematic use of cold sponging, friction of the skin with flesh-brushes, wearing of flannel vests and drawers, regulated exercise in the open air—if available, horse exercise. Many of the cases yield only to repeated change of air: the bracing atmosphere of upland and seaside localities generally suits the best. It will often be found advantageous to withdraw for a time the use of tea and coffee, and to substitute milk: or if this prove heavy, milk mixed with one-fourth of lime water. The diet should be judiciously compounded of due proportions of animal and vegetable substances—diminishing the one or the other group of aliments according to the ascertained idiosyncrasy of the patient. He must be cautioned against heavy meals, and trained to partake more moderately of four meals a-day. Digestion may be promoted by the administration of the mineral acids in light bitter infusions, or by small doses of the bicarbonate of potash in the same combination. It is not easy to determine beforehand which of these opposite medicaments will prove most grateful to the stomach. The rule of choice is, to administer the acid when the dyspeptic symptoms point to an atonic state of the organ and of the body generally; and the alkali when the signs point to gastric and general irritation."

Testing for albumen is no doubt in general a very simple matter, still we think the following passage is worthy of being laid before our readers,—“Nitric acid is an extremely delicate test for albumen. The best manner of applying it is to fill a test-tube to the depth of about an inch; then, inclining the tube, to pour in strong nitric acid in such a manner that it may trickle down along the sides of the tube to the bottom, and form a stratum some quarter of an inch thick below the urine. Added in this manner there is scarcely any mingling of the two fluids, and if albumen be present, three strata or layers will be observed; one, perfectly colourless, of nitric acid at the bottom; immediately above this, an opalescent zone of coagulated albumen; and atop, the unaltered urine. If there be only a trace of albumen, two or three minutes elapse before the opalescent zone becomes visible.

"There is no method equal to this for detecting minute quantities of albumen. The reaction of the urine does not interfere with its operation. Only one caution is necessary. In concentrated urines, and especially febrile urines, the addition of the acid is apt to precipitate the amorphous urates, and thus to occasion a turbidity which might be mistaken for albumen. The two conditions are, however, easily distinguished by observing the level at which the cloudiness begins, and the direction in which it spreads. Albumen begins to coagulate immediately above the stratum of acid, and the turbidity spreads upwards; but the urates first appear at or near the surface of the urine, and the opacity spreads downwards. Heat also readily resolves the doubt, for the urates speedily disappear when the urine is warmed, but turbidity from albumen is not affected by heat."

The second part of Dr Roberts' work, on Urinary Diseases, treats of diabetes mellitus and insipidus, gravel and calculus, and chylous urine. Perhaps the most interesting portion is that devoted to the medical treatment of urinary concretions,—a subject to which the author has devoted much attention, and a paper on which he lately communicated to the Medical and Chirurgical Society.

The subject of Organic Diseases of the Kidneys forms the largest portion of the work; and we naturally turn with most interest to the section on chronic Bright's disease.

Dr Roberts' arrangement of the anatomical changes which take place is the following:—

Type I.—*Kidney smooth, white, and enlarged*; in extreme cases, rarely met with, kidney atrophic.

Type II.—*Kidney granular, brownish or red, and contracted*.

Type III.—*Kidney lardaceous or waxy* (so-called *amyloid* degeneration).

The first form is described as usually the result of an acute renal affection; the second as existing in most of the cases of Bright's disease which are chronic from the beginning; and the third as usually the consequence of a wasting disease, such as phthisis, caries, long-continued suppuration, and constitutional syphilis.

Practically, we agree with this division of Dr Roberts, but we do not think that it is happily expressed; and we think he would have done far better in describing with Virchow the three forms as the parenchymatous, the interstitial, and the waxy. To the name "smooth white kidney," as characterizing the first group, we entertain a decided objection. In many instances it would apply far better to the waxy form, than to the cases it is intended to characterize. The most typically "smooth white" kidneys we have met with have been examples of waxy disease; in the great majority of cases so styled by Dr Roberts, the surface has a somewhat mottled appearance owing to the presence on the surface of numerous small, opaque, non-prominent granulations, the result of fatty degeneration of the epithelium of the convoluted tubules. Dr Roberts' second group, including the "granular" kidney, no doubt

corresponds with Virchow's interstitial form and to Todd's "gouty kidney," but he does not seem to have made up his mind as to whether the changes commence in the epithelial cells or in the intertubular structure. Regarding this point we think no doubt can be entertained; the change we believe to be exactly analogous to what occurs in cirrhosis of the liver, the intertubular structure is first affected, and any changes in the epithelium are secondary to it.

Dr Roberts does not clearly express what we consider a very important point with reference to chronic Bright's disease: namely, that we comparatively seldom meet with an absolutely pure example of one or other form of the disease. In most cases, two or even three of the forms are combined; the waxy, in particular, frequently supervening upon the parenchymatous ("smooth white") form.

On the much-contested subject of the cause of uræmic symptoms Dr Roberts makes some judicious observations. After stating some of the different theories which have been propounded, he thus expresses his own opinion:—

"None of these theories, considered exclusively, explains satisfactorily the protean phenomena of uræmic intoxication, as witnessed at the bedside. The subjects of Bright's disease suffer under a deep abnormality in the composition of the blood and tissues. The blood is unnaturally watery and poor in albumen; the blood and tissues are unnaturally charged with the primary histolytic products (creatine, extractives, etc.), and with excrementitious urinary compounds (urea and uric acid), perhaps also with the products of the decomposition of some of these. This state appears to induce in the nervous centres a proneness to sudden disorder and loss of equilibrium. A crisis may at any moment be brought about by an exaltation of one or several of the disturbing elements, or by a supervention of some new and different cause of irritation (hysteria menstruation). A similar hyper-sensitive state of the nervous system prevails naturally in early life; and an irritation which would be of no moment in an adult (teething, worms, embarrassed digestion, cutaneous irritation, etc.) suffices, in an infant, to awaken convulsive and comatose phenomena closely resembling those of uræmia."

On the subjects of hydro-nephrosis, cystic degeneration, cancer, tubercle, parasites, malposition, and malformations, Dr Roberts' work is unusually full. We quote the following passage from his description of the causes and diagnosis of movable kidneys:—

"But although, in a majority of the cases, mobility of the kidney appears to have been acquired from some accident or circumstance arising in the course of life, there are instances in which a congenital anomaly in the anatomical connexions of the gland has evidently operated to favour its production. In two cases, the peritoneum was found reflected over the posterior, as well as the anterior surface of the kidney, so as to enclose it within a fold of peritoneum, or mesonephron, which permitted the organ very considerable motion in the abdomen. Mr Durham examined the body of a woman, aged 34, in which the left kidney was very movable. He found the descending colon much nearer the middle

line than usual, and, instead of forming the sigmoid flexure in the left iliac fossa, it turned across the lumbar vertebræ, and passed down into the pelvis on the right side of the sacrum. It was manifest that the mobility of the kidney in this instance depended, in great measure, on the abnormal arrangement of the peritoneum, necessarily associated with the malposition of the colon. So far, therefore, it must be regarded as congenital. When traced from the side of the vertebral column, the peritoneum, instead of passing over the anterior surface of the kidney, only just touched the lower part of its inner border, and then, having formed the descending meso-colon, again touched its outer border. The lesser sac of the peritoneum also, instead of being confined to its ordinary limits, passed so far to the left as to cover the posterior surface of the spleen, and so far downwards as to touch, and be reflected from, the upper border of the kidney. Thus there was no distinct meso-nephron; but the kidney, instead of being supported and kept down by a single layer of peritoneum, was left free to move between and beneath three diverging layers. Upon dissection it was further found, that there was scarcely any fat in the lumbar region, but a quantity of very loose areolar tissue. (Guy's Hosp. Rep. 1860.)

"The *diagnosis* of movable kidneys is chiefly important, from the risk of confounding an ailment which is comparatively trifling, with some graver disease. In a considerable number of instances, the affection was long mistaken by the patient and his medical attendants for a tumour within the abdomen; and the patient was subjected, in addition to the alarm which such a notion necessarily engendered, to heroic and exhausting or troublesome plans of treatment. The diagnosis is indeed generally easy; and the errors committed have arisen, from the possibility of this condition not having been present to the mind of the practitioner, rather than from the inherent obscurity of the case.

"A movable tumour having the size and shape of the kidney, or approaching thereto, is found on either side of the abdomen, generally in the hypochondriac region; it can be pushed into the lumbar space, and again out of it, at will, by the thumb and fingers. When the corresponding loin is examined, the absence of the kidney from its usual place is rendered evident, by the flattening or hollowing of the part, and by the tympanitic note yielded on percussion. It is only in very obese individuals, and in cases where the displacement and mobility are slight, that any difficulty can arise. It must be remembered, that the displaced organ sometimes contracts adhesions in its new position, and thereby loses, partly or wholly, its freedom of movement."

It only remains to us to strongly recommend to our readers Dr Roberts' work, as containing an admirable resumé of the present state of knowledge of urinary diseases, and as a safe and reliable guide to the clinical observer.

Some Effects of the Climate of Italy. By THOMAS KING CHAMBERS, M.D., F.R.C.P., Consulting Physician and Lecturer on the Practice of Medicine at Saint Mary's Hospital. London: Churchills: 1865.

THIS is an admirable little volume. The only circumstance we have to regret in connexion with it is the illness of the author, which rendered a temporary absence from England advisable. Dr Chambers, about two years ago, was obliged to submit to amputation of the thigh, on account of degeneration and rupture of the

popliteal artery. Eight months afterwards he endeavoured to resume practice, but found himself quite unable for it on account of weakness of the circulation, and generally deficient nutrition. Acting in conformity with the advice of his medical friends he went to Italy, and after a residence there of nearly a year returned to England. The result is thus expressed in his own words: "And now I receive the reward of my obedience in meeting again, sooner than I had even hoped, my old pupils and friends in St Mary's lecture-room, with restored health and strength, and by God's blessing enabled to engage actively in the duties of my profession." On his restoration to health we most heartily congratulate Dr Chambers; and we trust that he may long continue to add to his Clinical Lectures, from which he has deservedly gained the character of being one of the soundest, and, at the same time, most popular medical writers of our day.

The substance of the matter contained in the present volume was delivered in the form of lectures at St Mary's School, and the lectures were at the time published in "The Lancet." The author has re-arranged them, added some new matter, and divided them into four chapters, headed respectively, Personal Experience; Statistical Evidence; Powers of the Air; Practical Applications. Dr Chambers begins the first chapter by describing the difficulties he met with in endeavouring to procure a good guide book; he did not appear to be suffering from any of the complaints for which spas and baths are usually prescribed, and so started for the Mediterranean with some misgivings, feeling that he was, to a great degree, an experimental traveller. Dr Chambers' account of the state of his health at this time forms the text for some excellent remarks on a peculiar constitutional condition of common occurrence, in which at first there is rather a tendency to disease than any local morbid condition. There is a general tendency to degeneration, and a period of "broken health" precedes any special local manifestation.

The principal features of this period of broken health are the following:—The patient feels himself less capable of and less inclined than formerly for either exertion or amusement; his rest is insufficient, and he rises with a sense of weariness; his appetite is poor, food oppresses him, at the same time there are not the usual symptoms of indigestion; the knees and ankles often become stiff and swelled towards evening, and vague pains are felt in different parts of the body; the complexion loses its transparency, and though the patient fails in strength he sometimes increases in bulk, owing to the growth of soft fat in the cellular tissue. "To the mind of the physiologist perhaps the most striking evidence of the defective vital power in both sexes is in the urine. I have said it is pale and scanty; it is also of lower specific gravity than one would expect from its scantiness. Yet there is not that deposit of lithates which shows imperfect assimilation in a healthy person.

Examined under the microscope, it exhibits not infrequently floating crystals of oxalate of lime, and I have heard the irritability of bladder attributed to the pricking of these sharp crystals. But they are really a great deal too small for that, and, besides, I have found great irritability when they were absent. No—the symptom seems rather due to the deficiency of the normal cause of acidity. For usually the pale urine of these patients is alkaline or neutral, either of which states is sufficient to cause the desire to evacuate the bladder without any diseased state of the urinary organs.

“The deficiency of urine is not supplemented by any increase in the secretion from the skin, as is the case in health. The perspirations are scanty and chilly, and appear to be mere transudations of water, instead of being impregnated with the sebaceous secretion as in robust persons.”

This is the first stage of the disease; the second stage is established when disorganization of some special organ, capable of being diagnosed, has been established, the failing part being usually one which has been overworked during the early period. Among the special diseases most likely to occur under such circumstances Dr Chambers refers to the following:—dilatation of the heart; atrophic paralysis of certain muscles; Bright's disease; chronic bronchitis; and softening of the brain. The condition in question is not, however, in Dr Chambers' opinion, often followed by tuberculosis. It is of the highest importance that the first stage should be recognised, as if judiciously treated, recovery may be looked for; whereas, when the second stage has been established, we can only at best hope to prevent the patient from becoming worse. The first cause which, in the opinion of Dr Chambers, underlies this whole series of pathological changes is a weakness of the blood-circulation.

In an Italian climate Dr Chambers soon began to feel that health was returning; he had taken no medicine, yet he found himself a different man. “Was the change due to time only, or had the climate anything to do with it?” This question Dr Chambers was resolved to answer. “I wanted to put to the test of facts our vague notions; I wanted to know from collateral and indirect evidence what was the actual effect of the Italian climate in the production of disease, and what in the progress of disease when once produced. I was determined to bring back somewhat more than a mere collection of opinions.” The first result of Dr Chambers' inquiries was that throughout the whole of Italy, acute diseases were vastly more common and more fatal than chronic diseases. Dr Pantaleoni of Nice informed him, that of Italian patients he had ninety-five acute to five chronic, and these latter chiefly hysterical and neuralgic; whereas, as he remarked, in the case books of London physicians, the proportions would be found to be inverted. Such were the opinions of other native physicians, some “fixing at a guess as four-fifths, others as two-thirds, as the proportion of acute cases in their native *clientela*.” Now, supposing

it to be established that chronic diseases seldom originate in Italy, and that they are less troublesome and less fatal than in other countries, it seems a natural conclusion that the Italian climate should be suitable for their cure.

Dr Chambers in his second chapter, entitled "Statistical Evidence," endeavours to show by figures, that the impression entertained regarding the infrequency in Italy of chronic disease is well founded. For this purpose he first compares the mortality from particular diseases of Genoa with that of London; and then contrasts the number of patients admitted with particular diseases into the Ospitale Maggiore of Milan, with those admitted with the same diseases into St Mary's Hospital, London. We can do no more than quote the deductions which Dr Chambers has drawn from his inquiries, but these deductions are certainly remarkable.

"*First*, There is a remarkable excess in the mortality from chronic disease in London over that of an Italian town similarly circumstanced.

"*Secondly*, This excess in mortality is united to and probably arises from an excess in prevalence.

"*Thirdly*, The excess is most striking in diseases where a tendency to degeneration of tissue is the most marked, and the most directly the cause of disease.

"I refer, especially to morbus Brightii and aneurism; but it is scarcely less observable in pathological changes of mucous membranes, which in England have a great tendency to pass into a chronic degenerative stage, and thus to destroy life. This is strikingly shown by the pulmonary mucous membrane. At St Mary's Hospital, though the cases returned as febrile bronchitis are scarce above one-third of the proportion at Milan Hospital (1 in 29 against 1 in 10), yet the numbers of chronic bronchitis are extraordinarily disproportionate (1 in 32 against 1 in 8823).

"*Fourthly*, The proportion of degenerative to other diseases is here exhibited in such as are wont to seek public aid in sickness, and is not confined to those in easy circumstances. It may therefore with reason be attributed to the climate or endemic habits of life, and not to any peculiarities of class."

No doubt the very different habits of Italians and English of the lower classes must widely influence the amount and the form of disease; but even all due allowance being made in this respect, a large amount of the difference, we agree with Dr Chambers in thinking, must be ascribed to climate.

In the third chapter, under the title of "Powers of the Air," Dr Chambers considers the obvious and external peculiarities of Italian life and climate which are likely to have an influence on health and disease. The principal features of an Italian climate depend on the great extent of coast-line; the greater saltness of the Mediterranean than of other seas; and the sheltered position of

many localities. The result is equalization of temperature, a dry clear air, and a higher amount and degree of sunlight.

"Now reflect," says Dr Chambers, "how these various agencies may act on the human body. The dryness of air, without excessive heat or cold, renders it unnecessary for the mucous membranes to put on their slimy coats of mucus. They are in a more active condition for the work of absorbing oxygen, digesting, extracting nutriment or water, or whatever else they are required to do. They are filled with blood, and pass it on rapidly with its fresh burden of renewed life to the tissues.

"The evenness of temperature renders it possible for all, invalids included, to be a great deal in the open air.

"The action of sunlight in reddening the blood is familiar to even the poets. The etiolated plant of northern climes is quickly by it rendered crisp and hardy, and the same effect may be inferred to take place in the etiolated animal."

At the same time, less solid food and less alcohol is required, and less can be taken than in colder climates; there is thus less temptation to slight daily excesses, and rancidity, so common in slow digestion, is avoided.

The last chapter consists of "Practical Applications." We can only quote a single passage, describing the classes of cases likely to be benefited, or the reverse, by a residence in Italy.

"It will be seen on reflection, that the common link uniting all the cases which I have enumerated as likely to profit by seeking an Italian climate, is a weakness of circulation, and a consequent tendency to chronic and degenerative changes. I believe it is this which is curable and cured in them, and not anything special in the several pathological changes by which their ailments are named. Thus is explained what has hitherto appeared mysterious and inconsistent in medical experience; one case has received benefit, and another called by the same name has received only injury from a residence in the same spot, or from travelling over the same ground. It is easy, for example, by inquiry to find instances of pulmonary tubercle in the first, second, or even third stage, which have decidedly improved during a residence in Italy, and equally easy to find the same number of corresponding cases which have died, or got worse quicker than they probably would have done at home. The difference between them will be found to lie in the diathesis which I have mentioned.

"It will be well understood from what has gone before, that I consider some morbid states of body as likely to be aggravated by that climate which is so beneficial to others. All those to whom excitement of the circulation is injurious should flee the country. Persons of *apoplectic habit* run great risks there, as may be seen by comparing the comparative number of deaths from that cause at Genoa with those in London in the table at page 37; where it will seem that the proportion is nearly double. Those also who have a tendency to *congestive inflammations*; such, for example, as cases of sharply-beating hearts with imperfect valves, who are apt to have pneumonia quickly supervene. Harm also is done to early cases of *tubercular consumption* with a quick, excitable circulation, and a liability to hæmorrhage; whether that hæmorrhage appear externally as hæmoptysis, or is extravasated in the pulmonary tissue. *Piles* also and *menorrhagia* are made worse, and sometimes brought on, by

travelling in Italy, in spite of precautions. All such cases, therefore, should be warned off, and indeed often are warned off by the honest medical advisers resident in the country.

"It would appear, from the extraordinary number recorded in the Rendiconto of the Milan Hospital of inflammations of the brain and spinal cord, that the nerve-tissue is very liable to suffer acute organic change there. I have not been in the way of confirming this by personal observation, but I have noticed that, certainly, functional diseases of the nervous system are often intensified, and sometimes brought on in previously healthy parties. And notably *hysteria*. Hysterical women should never be sent on tour to these climes without great caution, or a travelling medical man with them."

In conclusion, we have only to recommend Dr Chambers' book to the best attention of our readers. It is written in a thoroughly philosophical and scientific spirit, and contains facts from which the physician can judge for himself as to the propriety of recommending to a particular patient a residence in Italy.

A Dictionary of Practical Medicine. By JAMES COPLAND, M.D., F.R.S., abridged by the Author, assisted by JAMES C. COPLAND, M.R.C.S., and M.S.A. And throughout brought down to the present state of Medical Science. London : Longmans : 1866.

DR COPLAND'S Dictionary of Practical Medicine is perhaps the most extraordinary literary monument ever erected by a single individual. The amount of literary research displayed is absolutely marvellous, while evidence of the practical experience of the author is met with on every page. The size and the price of the work, however, put it out of the reach of many, and we trust that the present abridged edition, with its diminished bulk and cost, may attain a wide circulation. Though an "abridgment," this volume is anything but small. It consists of upwards of 1500 pages, printed in small type in double columns, and contains information on almost every subject connected with medicine. The principle on which this abridgment has been performed, has been to leave out or shorten various articles connected with general pathology, while the practical portions have generally been retained. In many cases, on the other hand, additions have been made, embracing the newest doctrines in regard to pathology and treatment. To the practitioner this volume will prove an invaluable mine of reference in everything connected with the practice of medicine.

Part Third.

PERISCOPE.

MEDICAL JURISPRUDENCE.

ON SIMULATED AMAUROSIS. BY DR C. SCHWEIGGER, BERLIN.

(Read before the New York Ophthalmological Society.)

THE detection of simulated amaurosis, by involving the individual in contradictions with the simplest laws of physiology, and thereby compelling him to speak the truth, while he imagines himself to be giving support to his false assertion, affords an interesting illustration of the exactness of ophthalmoscopic science.

The means we have on hand for this purpose are very simple, but of course differ somewhat according to the alleged morbid symptoms. Mostly it is monocular amblyopia or amaurosis which is simulated, very seldom binocular, and only the more experienced and skilful simulators choose to pretend a moderate degree of binocular amblyopia, or a narrowing of the field of vision. With men we are generally able to find a more or less obvious reason for the simulation; not so with women, however, who often, to gratify some inexplicable whim, resign for years and years every sort of social enjoyment only for the sake of pretending a non-existing disease.

The suspicion of a simulated amaurosis is raised whenever we find a high degree of amblyopia, together with an iris actively contractile in dim illumination, and a perfectly normal state of the optic disc and the retina, shown by the ophthalmoscope. It is important to know that a perfectly normal state of the ophthalmoscopic image may be combined with amaurosis or amblyopia of both eyes, not only, for example, in cases of alcoholism or uræmia, but also without any other perceptible disease; but in the latter cases there is either a speedy recovery of vision, or the fundamental disease of the optic nerve becomes visible by the ophthalmoscope.

Simulated amblyopia of a medium degree is best detected by the use of Snellen's test types, by an accurate determination of the degree of amblyopia, and by repeating the experiment with letters of different size, and at different distances. We have, for instance, a diminution of vision of $\frac{1}{40}$, then No. 200 ought to be seen at 10', No. 100 at 5', No. 75 at 3 $\frac{1}{2}$ ', No. 50 at 2 $\frac{1}{2}$ ', and so on as far as No. 10 at 6 inches. It will be a pretty difficult task for a simulator not to entrap himself in contradictions, if care be taken to measure accurately the distances at which the different types are made out, and to note down every single statement. If we have to deal with a narrowing of the field of vision, the best plan will be to sketch the letter accurately on a large sheet of paper. In order to keep the same distance for all designs, I use a rod of about 12 inches in length, which the patient himself fixes with his head against the paper, so as to make it impossible to alter the position of the head in such a manner as to render the projection of the field of vision larger or smaller. On repeating this experiment carefully several times in the course of some days, and on comparing the different projections, we shall soon discover a marked discrepancy between the patient's separate statements in cases of simulation. If the field of vision is alleged to be contracted in one direction near the point of fixation, we have a very elegant method of testing at once the truth of the assertion. After having drawn the projection of the visual field, we leave the patient in the same position, but with both eyes open. Now, if we hold a prism before one of his eyes, double vision is the natural consequence; and, holding

the base of the prism in the right direction, we may approximate one of the doubly-seen points of fixation to the limit of the alleged field of vision. Adding, then, a stronger prism in the same direction, we can bring one of these double images beyond the alleged limits, when double vision must cease, and only one point can be seen if the contraction of the field of vision really exists,—double sight being of course impossible if one of the double images is formed on an insensitive part of the retina. The degree of the prism necessary to make the experiment conclusive may be found by ascertaining the linear distance of double images produced by our prisms in the same distance we use for projecting the field of vision. After having ascertained this, we may easily arrange matters so as to make it entirely impossible for a simulator to know when he must see single and when double. One thing more ought to be taken into account, namely, the fact that double images produced by prisms with their base outwards may be united, and single vision procured by a corresponding convergency of the visual axes; by divergency, prisms of medium strength with their base inwards can be neutralized, while only the weakest with their bases above or below can be overpowered by a contraction of the inferior and superior recti muscles.

The detection of simulated amaurosis of one eye by prisms was first taught by von Graefe. After having examined the alleged amblyopia, we carefully try the sight of the other eye, and holding a prism in front of it with the base upwards or downwards, inquire whether a small point or a fine line appears simple or double. Most of the simulators, imagining that we are dealing merely with the sound eye, at once acknowledge double vision, and in this manner I have several times made them read the smallest print with the alleged amaurotic eye alone, covering the sound eye by-and-by without their knowing it. In some cases, however, I tried the prisms without effect; either the simulators were already aware of their use and would not acknowledge the double images, or there was indeed no simultaneous vision with both eyes. Thus we see, for instance, exceptionally in cases of squinting, that each eye separately sees well enough, while both eyes never work simultaneously, so as to make it entirely impossible to bring on double vision by prisms. It is also possible, though not very probable, that similar cases may occur without squinting. In cases, therefore, where there is nothing to be done by prisms, I tried to procure other means to detect simulation, and found them in the use of the stereoscope. In this instrument each eye has a separate field of vision quite inaccessible to the other eye. This separation is the essential point of the instrument; convex lenses are added only for the purpose of approximating the stereoscopic designs, allowing at the same time a relaxation of accommodation and nearly parallel lines of vision; in myopics, therefore, the convex lenses might be dispensed with. Prisms with their base outwards, combined with the convex lenses, have only the effect of enlarging the outer side of both fields of vision. For all the purposes we now speak of, this combination of prisms with convex lenses is quite superfluous, even in some respects troublesome.

Knowing, now, that in simultaneous vision both separate fields of the stereoscope are united in one, simulation becomes evident if we find this union in a case of alleged monocular amaurosis. We have, for instance, in the one field horizontal parallel lines about one quarter of an inch apart; in the other, vertical parallel lines separated by the same distance; then, in the united field of vision, we see both systems of lines united in regular squares. As soon as these squares are seen, therefore, it is evident that there can be no monocular amaurosis. We may even go a step further, and use the stereoscope quite independently of the existence or non-existence of simultaneous vision, and in this respect it is superior to the prisms. If we draw in each separate field of vision that vertical line whose image goes through the centre of the retina, then, in the united stereoscopic field, not only both lines are seen as one, but every object situated to the right of one of those lines is projected to the right side of the field of vision, and appears as if it were seen with the right

eye. The same, of course, is the case with the left side. This gives the means of determining, in cases of simulated monocular amblyopia, the acuteness of vision, and, if we choose, even the range of accommodation. For this purpose we arrange matters as follows:—We have at the bottom of the stereoscope a sheet of paper, marked only with the two lines above mentioned. Now, if we have to deal, for instance, with an alleged amblyopia of the left eye, we place in the left field of the stereoscope, but to the right side of the vertical line, any object, say a piece of a printed paper,—with this exception, the whole of the bottom of the stereoscope is left blank. In the united stereoscopic field the paper will then appear as the right side, and will make so strong an impression that it is seen with the right eye, that I doubt whether anybody can resist it; with a stereoscope which allows the convex lenses to be approached to or withdrawn from the bottom of the stereoscope, we can, if we choose, at the same time, ascertain the range of accommodation.—*New York Medical Journal*.

THE MICRO-SPECTROSCOPE IN THE DETECTION OF BLOOD-STAINS.

AT the trial of Coe for the Mountain Ash murder, corroborative evidence of the presence of blood on the hatchet was furnished by the micro-spectroscope. Dr W. B. Herapath conducted the scientific inquiry, and we believe has the merit of first using the micro-spectroscope in medico-legal inquiry. Of course, the evidence furnished by it cannot prove the presence of human blood, but it may prove a valuable corroborator of other testimony. The following is Dr Herapath's evidence:—

Dr Bird Herapath sworn: "I am a Fellow of the Royal Societies of London and Edinburgh. I practise as analytical chemist and also physician. The hatchet produced was given me by Mr Wrenn, and I carefully examined it. On the metallic portion I did not find any marks upon which I could rely. I removed the handle and experimented on thin slices of wood which I took from underneath the metallic ring. I examined those sections with a microscope, and found the majority of the stains were due to oxide of iron; some of them showed clotted blood—in some cases the woody portions had been infiltrated with the colouring matter of blood changed by the action of water. On some of the sections of the handle I found globules of blood, and by the micrometer I measured the size of those globules. I placed a section of the handle in a glass cell in which there was a fluid medium, and the blood globules floated off into the cell, and by the measurement of these I could determine the size of globules therein contained. These globules were exactly the same size as some globules from dried human blood which I purposely procured, and tested with the same apparatus in the same way. Finding this evidence of blood to be small, I obtained more numerous sections of the coloured surface off the handle of the hatchet—immersed them in distilled water, and obtained thereby a slightly coloured solution, which after filtering was ready for chemical tests, and for optical examination by the micro-spectroscope. I subjected this fluid to the action of light, and it had undoubtedly the properties peculiar to a solution of blood. When a solution of blood was examined in this instrument [instrument here produced] the fluid absorbed some of the rays of light, and thus altered the spectrum or rainbow. Within the green and on the border of the yellow rays two dark absorption bands were produced by the blood fluid. Only one other substance would produce two dark bands—that is, cochineal dissolved in ammonia, but the position of the two bands was different. The spectroscope alone would not enable me to *readily* distinguish between the two, but combined with chemical examination it would satisfactorily do so. From this optical test I was satisfied that the sections of the hatchet had been stained with blood—and by chemical analysis I also demonstrated it was blood. The combination of the three tests showed that the substance on the hatchet must have been blood.

"Cross-examined: I should not like to say that the stains were those of human blood, but my opinion is that they were."—*Medical Times and Gazette*.

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLV.—MEETING VI.

7th March 1866.—Dr MOIR, *President of the Society*, in the Chair.

I. CASE OF ADDISON'S DISEASE.

Dr Argyll Robertson, in the absence of *Dr Sanders*, brought before the Society a patient presenting the coloration met with in Addison's disease. There was a general bronzing of the whole skin, while certain parts presented a deeper colour than others. The deposit of pigment was slightly greater below the eyelids, and was very marked about the scrotum and penis. The mucous membranes were also affected. At the tip and edges of the tongue were a few well marked patches, also on the lips and buccal mucous membrane. The conjunctiva was not affected. Three years ago the patient had begun to feel pain in the right lumbar region; before that time he had been in perfect health. The pain was so severe as almost to incapacitate him from work; it had been persistent more or less. The skin had gradually assumed its present dark colour. General debility with loss of weight was a marked symptom of the case.

II. ANEURISM OF AORTA.

Dr Haldane showed a specimen of aneurism of the aorta where tracheotomy had been performed and had been temporarily successful, the patient having survived more than a fortnight, but then died from pyæmia. The case will appear in full in an early number of this Journal.

III. SPECIMENS OF TUMOURS.

Mr Annandale showed the following specimens of tumours:—

1. A medullary tumour from the breast of a lady 54 years of age, removed after death. The disease seemed to have been quite local, there being no malignant deposit in any of the other viscera.
2. A simple glandular tumour of the mamma from a woman 64 years of age. Such tumours, it was well known, were most frequent in young females.
3. A cartilaginous tumour of the head of the tibia, removed by Professor Syme; the interior of the bone was found infiltrated with a similar deposit; the case illustrated the necessity of removing the bone along with the tumour.
4. A cystic tumour of the lower jaw, consisting not of one but of several cysts. Half of the jaw was removed by Professor Syme. The tumour had previously been tapped, but without relief.

IV. OBSTRUCTION OF THE BOWELS.

Mr Annandale showed a preparation from a case of intestinal obstruction. The patient, a young gentleman in East Lothian, had been seized ten days before he was seen by *Mr Annandale* with pain in the bowels, followed by total obstruction. When seen he was suffering from all the symptoms of strangulated hernia; all the situations in which a hernia could occur were examined, but nothing was found. Purgatives had already been tried, but without effect. Opiates were accordingly administered, and a tube was passed two feet up the bowel, but no obstruction was found. Three days afterwards he was in much the same state; again a tube was introduced and warm water injected. Soon

afterwards the patient died. On examination, a knuckle of ileum, where it joins the cecum, was found twisted; this portion of bowel was quite dead and had given way at the point, and there was a little peritonitis. The nature of the case had been diagnosed during life as one of twisting of the gut, for there had been no discharge of bloody mucus such as occurs in intussusception. If Mr Annandale met with a similar case, he would make a free incision, try to find out the seat of strangulation, and relieve it.

V. FRACTURE OF UPPER PART OF RADIUS.

Dr P. II. Watson showed a specimen of a very rare injury,—fracture of the head of the radius on both sides. The patient had fallen from a height, and lighted upon the palms of his hands.

VI. CASE OF AMPUTATION OF THE THIGH FOR ELEPHANTIASIS.

Dr Handyside read a paper, by *Dr Varten*, describing a successful case of amputation of the thigh for elephantiasis, in which acupressure had been employed.

VII. ON THE GALVANO-PUNCTURE OF ANEURISM.

Dr John Duncan read a communication on this subject, which will be found at page 920 of this number of the Journal.

VIII. ON PELVIC SEROUS CYSTS.

Dr Matthews Duncan read a paper on this subject, which will be found at page 916 of this number of the Journal.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXV.—MEETING VI.

14th February 1866.—*Dr BURN*, Vice-President, in the Chair.

I. ON UMBILICAL CORDS. BY *DR INGLIS*.

Dr Inglis showed two umbilical cords, each over four feet long. They were both taken from the same patient: one four years ago, and the other that morning. Both times the child died from strangulation of the cord about three days before delivery, which took place about the full time. This recurrence, he said, was not uncommon, and had been mentioned by him at a meeting of the Royal Medical Society in 1858, when he showed a cord five feet four inches, taken from a patient who had lost a child from the same cause previously in the country. On that occasion, also, she said that the doctor had taken home the cord, as it was the longest he had ever met with. Since then she has lost another child in the same manner.

II. ON FETATION IN A RUDIMENTARY UTERINE CORNU (WITH PREPARATIONS). BY *WILLIAM TURNER*, ESQ., M.B.

Mr Turner's paper will appear in this Journal for May. An interesting conversation followed.

Dr Keiller remarked, that the Obstetrical Society had frequently been indebted to Mr Turner for elucidating interesting pathological points; and on the present occasion there could be no difference of opinion as to the interest and value to be attached to the important communication just listened to.

It was too much the case to have pathological specimens, and what are often described as "valuable preparations," put up in jars, so that their general appearance or external features alone could be guessed at; and our museums were too often rendered of much less use than they ought to be because of the wholesale bottling fashion usually followed. In every museum, for example, may be seen specimens of malformations and "monstrosities" of the most

interesting physiological and anatomical character; yet how few of such "preparations" were to be seen beyond their surface, simply because they have not been subjected to a minute examination and scientific interrogation, like that which has been done in the present instance. He (Dr K.) had often insisted on the importance of subjecting abnormal specimens, especially those exhibiting unusual structural development, to proper examination. A case of united twin, or so-called "double monster," fell into his (Dr K.'s) hands years ago, which, instead of being put up to be stared at "on Tuesdays and Fridays," was dissected most carefully by Mr Hallett, an able predecessor of Mr Turner, and whose scientific paper on the subject achieved more than could be gathered from a whole collection of "untouched" malformations.

With regard to the novel views arrived at by Mr Turner, the proof of their correctness seemed to be almost conclusive. It was the common idea of obstetric and other observers, that the cases such as those exhibited and commented on by Mr Turner were in every respect *extra-uterine*: their uterine character as laid down so clearly in the paper now read being never referred to.

He (Dr K.) had occasion to discuss, in his lectures, the subject of extra-uterine pregnancy, and to illustrate the matter by cases somewhat similar to those from which Mr Turner had arrived at his interesting conclusions; but what had been now stated would doubtless lead to a different mode of dealing with the pathology and nature of many of the so-called extra-uterine fœtations.

In reply to Dr Keiller,

Mr Turner said, that the ovaries were examined, and that a corpus luteum was found in the ovary belonging to the impregnated horn, also that spermatozoa would travel along to some distance on a peritoneal as on a mucous surface. There was no reason why spermatozoa should not travel from one Fallopian tube to another. Mr Turner said both Fallopian tubes occasionally grasped one ovary.

On the motion of Dr Keiller, the thanks of the Society were warmly accorded to Mr Turner for his valuable communication.

III. DISEASED PLACENTA. BY DR KEILLER.

Dr Keiller again exhibited and made remarks on the pathological state of the placenta belonging to the case which was brought before the Society's last meeting. In the case referred to there was reason to believe that the death of the child, on this as on the former occasions, resulted from the morbid condition of the after-birth. Dr K. had requested Dr Grainger Stewart to examine the placenta, which presented the usual appearance of fatty degeneration so common in cases like the present, where the administration of the chlorate of potash is considered useful. Dr Stewart's note of examination states, that "the placenta contained a number of patches of pale tissue, which on microscopic examination were found to be intensely fatty, the vessels being extremely degenerated, and the intermediate tissue loaded with granules. The rest of the placenta was not quite healthy, its vessels being to some extent fatty. In the membranes also there were patches of pale fatty deposit."

IV. CASE OF TWINS.

Dr Keiller also exhibited a recent twin placenta which he (Dr K.) considered very interesting on the present occasion, seeing that it strikingly proved the view usually adopted in regard to the cause of foetal danger and death in such cases as those just referred to, in that the condition of the child is gravely influenced by the state of its after-birth, and that fatty degeneration is the usual pathological danger found in the placental structure.

The twin children were in this case born alive at the full time, one of them was plump and well developed, while the other was lanky and apparently very badly nourished. On looking at the placenta (which was of the usual double form), the cause of the difference between the appearance and weight of the children seemed very obvious, the half of the placental mass belonging to the well-developed child being thick and healthy looking, whereas that of the ill-fed twin was comparatively thin, pale, and fatty.

Mr Turner thought the half of the placenta fatty and lobulated, like cotyledons.

V. LOCAL ANÆSTHESIA.

Dr Black showed the spray-douche instrument for producing local anæsthesia, by the evaporation of sulphuric ether, introduced by *Dr Richardson*. It had been used in Edinburgh by *Sir James Simpson*, in excision of small tumours, opening abscesses, etc., and in extraction of a tooth by *Dr Smith*, hitherto with but irregular success. *Dr Black* had seen *Sir James Simpson* use it quite successfully in excising a small tumour, but it had failed in one or two other cases where there seemed every condition present to ensure its anæsthetic action.

VI. EXCISION OF THE BREAST.

Dr Black introduced a patient, where *Sir James Simpson* had excised the mamma nineteen days previously, and where the whole wound had healed by the first intention. Acupressure had been used, and very deep and wide sutures so as to embrace the base as well as the sides of the wound.

SESSION XXV.—MEETING VII.

28th February 1866.—*DR CHARLES BELL*, Vice-President, in the Chair.

I. TETANUS OF THE UTERUS. BY *DR DAVID GORDON*.

The patient referred to was a *Mrs Bain*, aged 38, the mother of several children. During her pregnancy she had much bearing-down pain; the abdomen was very pendulous and partially supported on the thighs. The veins of the labia and limbs were varicose. The labour was very tedious. She began to complain of pain about 11 o'clock on a Monday night, and she was not delivered till 3 P. M. on the following Thursday, when *Professor Simpson* applied the forceps, and without much difficulty extracted a living child. To obtain a thorough examination, the patient was deeply chloroformed, and the hand was passed into the vagina through the brim, and fairly beyond the head of the child. The obstructing cause was then discovered. Firmly enclosing the neck of the child there was felt a broad and strong band of uterine fibres in a state of tonic contraction, which effectually prevented the passage of the shoulders through it. Several moderate pulls with the forceps, during the intervals between the pains, sufficed to overcome this obstruction, and delivery was speedily accomplished with perfect safety to mother and child, both of whom have done well. After the birth of the child there was a sharp attack of flooding. In removing the placenta the cord broke, and it became necessary to introduce the hand. The whole of the membranes remained in the uterus, and their removal was very troublesome. The flooding continued for some time, notwithstanding the vigorous use of cold and pressure. It appears to me that this case illustrates very clearly the great utility of chloroform in midwifery practice. Without the aid of such an agent it would have been impossible to make the thorough examination in the course of which the contracted and obstructing band of uterine fibres was discovered encircling the neck of the child and preventing its descent. It also shows that in skilful hands forceps may be applied to the head above the brim and fairly within the cavity of the uterus, without injury to mother or child. And I am confident that the practice, in such circumstances, is much safer than that of turning. This case also reminds us that when labour is retarded by the special cause indicated, the extracting force must not be applied as usual during the pains, but in the intervals between the pains.

Dr Young said, the subject of hour-glass contraction of the uterus having been referred to, he would take the liberty to call in question the existence of such a condition of that organ, so generally believed and taught both in books and lectures. I can only say that, after a lengthened experience, I have never seen it, but have not unfrequently seen what I believe is often taken for hour-glass contraction, but which is nothing more than a firm contraction or

tetanic condition of the os and cervix uteri, after delivery and before the expulsion of the placenta, being still retained in the cavity of the uterus, where I have been not unfrequently obliged to introduce my hand for its removal. The os being so firmly contracted as hardly at first to admit one finger, and requiring a considerable amount of force till the whole hand was introduced, whereby, although considerable pressure was made on the uterus externally, yet from the soft and relaxed condition of the vaginal walls and stretching to such an extent, that it might have been believed the hand was actually within the cavity of the uterus.

Dr Stephenson said, that notwithstanding *Dr Young's* experience in this matter, he must say that he had seen one case at least of regular hour-glass contraction of the uterus, and he knew many others had met with it.

Dr Wilson had seen one case, where the os was open, and the body of the uterus contracted.

Dr Bryce had seen some cases identical with *Dr Stephenson's*. In one case, when the patient was under chloroform, he found regular hour-glass contraction after delivery.

Dr Pattison said he once believed in hour-glass contraction, but now he entirely agreed with *Dr Young*.

Dr Charles Bell coincided with *Dr Young* also, and said that he considered it a grave error to suppose that hour-glass contraction was a spasm of the body of the uterus, while it was only a contraction of the cervix; in fact, *Dr Bell* thought it difficult to imagine such a condition as a central band in the uterus.

Dr Young asked if any member had had cases where the membranes were left behind, and what was their experience?

Dr Cairns related a case where he could only remove a portion of the membranes, and no injury followed.

Dr Pattison thought that any portion of the membranes left were thrown off by the after-pains.

Dr Bryce quoted a case of abortion at three months, where the os contracted after the placenta was removed, and the membranes did not come away for four days, without any untoward symptom. In a case of miscarriage, the placenta was retained for some time, and fever resulted.

Dr Pattison mentioned a case of abortion at three months, where the placenta was left, which occurred on a Sunday, and hæmorrhage came on to such an extent on the subsequent Friday that it was necessary to plug.

Dr Carmichael related a similar case, where the placenta was retained for some days.

Dr M^cCowan spoke of a case which occurred at Duddingston, in 1849, where the patient miscarried at the fifth month. No placenta or membranes came away, although most carefully watched, and no evil followed.

II. CASE OF CONTRACTED PELVIS. BY DR BRYCE.

In the *Edinburgh Medical Journal* for August 1864, and among the transaction of this Society, may be found the notes of the first and second labours in this case, which possesses some interest not only in itself, but also on account of its having given a fresh impetus to kephalotripsy; the facility in the application of the instrument in this case, and its successful issue, having led *Sir James Simpson* not only strongly to recommend the operation, which he had then performed for the second time, but also to the invention of a much simpler and more portable kephalotribe than any hitherto in use. The present notes I offer to the Society, with the view of completing the case; but before doing so, I may remind the members that the difficulty in this case is the contraction of the antero-posterior diameter to about two and a half inches. In the present case, then, we begin with the 6th November 1864, when my patient last menstruated. Allowing a week from this date, I began my calculation from the 13th November, and arranged to induce premature labour on 20th of June 1865, but was prevented from doing so till Wednesday the 28th; on the after-

noon of which day I introduced a catheter for that purpose. On the evening of Thursday the 29th, slight grinding pains came on, and the labour progressed very slowly and irregularly, till midnight of the 2d of July, the cervix continuing throughout thick and undilatable, notwithstanding long intervals of rest, under the influence of chloroform. As the patient was at that time very restless, suffering very much when not under the chloroform, and feverish, I determined to apply the forceps, though I expected considerable difficulty from the undilatable state of the cervix, and waited only the arrival of my friends Drs Main and Macdonald, who had expressed a wish to see the issue of the case. As I anticipated, I had great difficulty in introducing the second blade of the forceps; and again, after I had got the instrument carefully adjusted, in inducing the head to enter the brim, on which it was resting, and projecting very much over the pubis, as on the former occasions. After entering the brim, it required my utmost strength to cause the head to descend, and by the time it had reached the bony outlet, my arms gave way, when I resigned my place to Dr Macdonald, who completed the delivery of a well-formed healthy looking little girl, at three A.M. of the 3d July, four and a-half days after the introduction of the catheter. The placenta was expelled in a few minutes, and the patient was about again in a few days. Not even the scalp showed the slightest indication of the rough usage to which the head had been subjected, and the child itself nursed well from the first: it is now eight months old, is of the average size of a child of that age, and has not as yet had a day's illness. The difficulty in extracting the head was, no doubt, due to the child being a week older than on the former occasion. The Society will remember that Sir James Simpson recommended that labour should be induced at the end of seven months and *one* week, as was done in the first instance, but this time it was unavoidably at the end of seven months and *two* weeks. I think this is a strong case in favour of the kephalotribe. Here we have a woman undelivered after three days and a-half of labour, and in whom the removal of the whole upper part of the skull and of the brain of the child is not followed by the slightest improvement in the progress of the case, delivered with great ease in twenty minutes by this powerful instrument, and now nursing her third child. It is not unnatural to speculate as to what would have been the result, had the Caesarean section or some other of the last resorts of operative midwifery been acted on on that occasion.

III. CASE OF PROLAPSUS FUNIS. BY DR BRYCE.

About eight years ago, Dr Thomas, of New York, introduced what he called "the postural treatment" for prolapsus of the funis. The subject was brought before this Society three years ago, by Dr Alexander Simpson. Since then, I think we have had no cases before us that have been treated on the plan suggested by Dr Thomas. Having tried "the postural treatment," in a case of this complication a few weeks ago, I have thought it right to mention it to the Society. On arriving at the house of my patient I found the head as low down as the bony outlet of the pelvis, with a loop of a few inches of the cord in advance. I immediately placed the patient on her elbows and knees, and allowed from five to ten minutes to pass before I interfered any further. At the end of that time I gently pressed back the cord, which in less than one minute glided out of reach, gave no further trouble, and the child was born alive.

Dr Young recommended that, in cases of difficulty, the forceps should be applied from face to occiput, and thus obtain a firmer hold, especially, as sometimes happened, that the cavity of the forceps was too wide.

Dr Inglis thought that forceps applied diagonally might cause paralysis of the facial nerve. He recommended, in cases of prolapsus funis, that the cord simply be pushed up into the uterus, the forceps immediately applied, and the head brought down.

IV. CASE OF DOUBLE PLACENTA.

Dr Wilson exhibited a placenta, which had the appearance of being double, and where there was only one child.

ARMY MEDICAL DEPARTMENT.

As usual, the Director-General of the Army Medical Department has presented the names of the fortunate gentlemen who recently passed through Netley to the editors of the various medical journals. Sixteen candidates in all have again passed through that Military Medical School. Twelve of them are from the Emerald Isle, three from the north of the Tweed, and there is one solitary Londoner.

A few years ago, both Scotchmen and Englishmen eagerly aspired for the service, carrying all before them; but, evidently, other and better paths have opened up. There is not now any great inducement for them to come forward. With such views of a long, tedious, and arduous subordinate position in perspective they do well to keep aloof; and when they are told that the authorities are well pleased with the good men from Dublin, Galway, and Belfast, it is well to let them remain in their belief.

The medical officers of the Army Medical Department have been agitating their claims for some time back, through the medium of the medical journals and other periodicals; but those who laboured assiduously for these improvements and amelioration of their position, which they had every reason to expect, have not been gratified in this respect to any considerable extent.

It will not be denied that the young gentlemen at present entering Her Majesty's service are much better remunerated than those who entered ten or fifteen years ago, as far as pay goes. But we would ask, has their position been improved? Has a better scale of retiring allowance been offered them? And what chance have they of ever gaining the Inspectorial ranks? As we showed in a late paper in these pages, and the case has not been altered since, we see no chance of any one of those now entering even attaining to the humble position of regimental surgeon, as promotion at present goes, before the period of eighteen years.

Considering their respective ages at the present time, and the colonial stations at which they will pass the better part of their life, they will be toil-worn and hoary with age ere promotion comes upon them; and during the whole of that time, after having received an expensive and scientific education, with an extra gloss from the luminaries at Netley, they remain for the above-named period classed amongst the "youngsters;" for, no matter how old a medical officer may be, or what relative rank the authorities may be pleased to confer on him, as long as he remains an assistant-surgeon, he continues a subordinate, and is only considered fit for minor duties. And, moreover, if he continues too long in this position, he becomes narrow-minded with regard to his duties, and at the proper time and season is unable to comprehend or grasp those extensive measures which are so needful for the safety and comfort of the men under his charge.

We by no means grumble at the title assistant-surgeon, in which rank we served for a series of years; but we assert, and with good hopes of being believed, that it is not good for the welfare of the department, that men should occupy that position—honourable though it be—for too long a period of time.

It was fully hoped that the late commission, from which so much was expected, would have granted an earlier and a competent allowance to old officers, as an inducement for them to retire, so as to make the period of serving in

the grade of assistant-surgeon shorter for those now entering, as also for those now occupying the subordinate ranks. These hopes have been shattered, and such aspirations blasted, if it was thought to thin the ranks of the senior executive by this means.

By the warrant of 1858, medical officers of 25 years' full pay service generally retired on 18s. 6d. per day. By the recommendation of the late commission, if carried out, they can retire on 18s. 9d.

We have no right to expect that old officers will be induced any more to retire than formerly, because the authorities have enlarged their retiring allowance by an extra 3d. per day.

The committee have been bountiful withal in their recommendations, as in another clause they "recommend that as special reward to officers of long and good service, who, owing to the comparatively small numbers of the Inspectorial ranks, have not been promoted to any higher position than that of surgeon-major, such officers of the rank of surgeon-major as have served for 25 years on full pay, should, on being compulsorily retired at 55 years, receive the half-pay of £1 per day, but such officers retiring voluntarily under 55 years of age should receive only 7-10ths of their full pay." In framing this claim the committee, with characteristic foresight, omitted, as an old and distinguished surgeon-major remarked the other day, the following very possible case:—Suppose that two men enter, the one at 22 and the other at 30 years of age respectively; that neither of them attains to the administrative ranks, and that each of them serves 25 years on full pay; the elder one, from his advanced age alone, will obtain £1 per day in accordance with the above; whereas the other, with the same service, would only receive 18s. 9d. per day, and in order to gain the £1 per day would require to serve other eight years, without in the end receiving any more additional half-pay for his extra service. The young gentlemen now entering may think over this, and they will in all probability come to the conclusion that their future is not so hopeful as they expected. We fully and confidently expected that a better and more generous earlier retirement would have been granted. It would have acted as a stimulus to those in the service, induced others to enter, and allowed the seniors to have made an honourable departure. And here we may remark, with regard to the recommendation of retirement after 20 years of service, and the generous sum allowed (11s.), that the committee have only fallen back on what that well-abused man, who received such an amount of vituperation from the public press—Sir Andrew Smith, late Director-general—recommended to the committee of the House of Commons in 1856. It would appear that even at this day his recommendations were not illiberal, and that the committee of the late commission, of which the present Director-general was a member, were not ashamed, even in those very liberal days, to borrow from those same recommendations for want of a better. We are not surprised at the dissatisfaction expressed regarding the very illiberal scale of retiring allowance recommended; but medical officers, if they ever wish to attain to the administrative branches, must, in addition to their other qualifications, combine a certain submissiveness of temper, and be prepared to bend as a sapling before the wind.

Considering the comparatively small number of the Inspectorial ranks, and the few who gain that position, those who do attain to it look back upon their executive brethren as mortals of an inferior sort of clay. A broad gulf now separates them, and having gained their object they no longer take any interest in the grievances of their juniors, discountenancing any improvements or even suggestions for

the amelioration of their condition. They are no longer of the material that the common doctor is,—all of a sudden they have become administrators. A strange and gratifying sensation pervades their frame; their words are of a sudden invested with a significance, and their person with a certain dignity, which they previously altogether failed to command. In every department there must be some individuals who, but for the fact that they are administrators, would command but little attention and attract little notice. Of all the great batch of men, promoted all of a sudden by the late Mr Alexander to the administrative ranks, we can call to recollection one, and one only, who has shown any marked ability, and has views of a comprehensive nature, and who has been able to gain the confidence of those serving under him.

Till there is a joint effort made, both by the administrative and executive medical departments of the army, for the elevating of their position, and raising their status in the world, it is out of all reason to suppose, that any body of men, either as a commission or otherwise, will be able to legislate for them. To effect this a simultaneous movement will require to be made: the one party assisting the other, and not as has been the case, the one party (administrative) looking on in gloomy silence, still with secret hope, that whatever advantages the executive might gain, they would also gain in a comparative degree.

We have heard it affirmed by army medical officers, that an antagonism of feeling, and a substratum of jealousy, exists in the minds of the combatant branch towards them; but this we have not found. We have often seen a combatant officer take up the cudgels in defence of the medical, while a brother medical stood aloof; and in numerous instances greater benefits are derived from the officer who commands than from the medical invested with Inspectorial power.

It has been hinted to us while writing the above, that it is probable a clause may exist in this new Warrant, allowing surgeons-major to retire after 25 years of service, and before the age of 55 years; if by medical board, on the same allowance as if they had arrived at 55 years, provided they be deemed unfit for further service.

MEDICAL CANDIDATES FOR THE ARMY MEDICAL SERVICE.

WE have heard that the promise of better things in our public services has not been without its effect on the London schools. It is well known that the students at our metropolitan hospitals have hitherto held aloof from those services, but there seems to be a general conviction that the recommendations of the late committee will be acted upon, and that a perfect equality in the two services must also follow in time. Many English students, we believe, have presented themselves for the competitive examination at Chelsea. It only remains for the Government to give effect to the hopes of our profession, and there will be no lack of good men from all schools. We are glad to perceive that Colonel North will repeat his question to the Secretary for War as to the adoption of the committee's report after the Easter recess.—*Medical Times and Gazette.*

THE LATE SIR ALEXANDER MORISON.

BUT a few days have intervened between the death of Dr Conolly and the death of Sir Alexander Morison, two names pre-eminently distinguished in the annals of improvement in the treatment of the insane in this country. Sir Alexander Morison was the older man, having attained, at the time of his death, on the 14th March, an age only six weeks short of eighty-seven years. Dr Conolly was fifteen years younger.

Sir Alexander Morison was born at Anchorfield, near Edinburgh, on the 1st May 1779. At the High School of Edinburgh he was in the same class with Lord Brougham. In 1798, he took the diploma of surgeon, and in the following year he obtained the degree of Doctor in Medicine from the University of Edinburgh. After a year's probation as a licentiate, he was elected a Fellow of the Royal College of Physicians of Edinburgh in 1801. In 1808, he became a licentiate of the Royal College of Physicians of London; but not till 1841 did he obtain the rank of Fellow of that College. In 1809, he was appointed medical superintendent of a private asylum for the insane in the county of Surrey. About the year 1816, he delivered a course of lectures in Edinburgh, on mental diseases, to the excellence of which some surviving members of the medical profession can still testify.

In 1816, he was appointed Physician-in-Ordinary to Her Royal Highness the Princess Charlotte of Wales; and after her marriage he obtained the like honourable office from her husband, Prince Leopold, the late King of the Belgians, who was further graciously pleased to stand godfather to his infant son. He also held the appointment of physician to the Duke of York.

In 1827, he was elected President of the Royal College of Physicians of Edinburgh, which office he held for the usual period of two years.

In 1832, he was appointed consulting physician to the Middlesex Asylum at Hanwell, and visiting physician to the Surrey County Asylum. In 1833, he gave his first course of lectures on mental diseases in London, which course was continued annually for a number of years. In 1835, he was appointed physician to the Royal Hospitals of Bethlehem and Bridewell, and consulting physician to several other asylums for the insane in different parts of England; and in these he laboured assiduously to promote the comfort of the insane poor.

In 1838, soon after the accession of her present Majesty, he received the honour of knighthood.

After his retirement from the office of physician to Bethlehem Hospital, he lived chiefly near Balerno, in the parish of Currie, visiting England occasionally in the performance of his duty as consulting physician to the Surrey and other asylums. This he did till within the last few years, when the effects of a severe illness rendered it more prudent to remain nearer home. Notwithstanding his great age, he retained his faculties to the last moment of his life.

Sir Alexander's works are numerous, the most remarkable of which is "The Physiognomy of Mental Diseases"—a very valuable book, in which, along with descriptions of the various forms of deprivation of mind, are illustrative portraits from drawings by eminent artists taken from patients in the several institutions with which he was connected.

In 1864, he instituted an annual course of six lectures on mental diseases, under the direction of the Royal College of Physicians of Edinburgh, the first course of which was given in Physicians' Hall last summer by Dr Sellar, appointed by him the first lecturer. He has also established an annual prize to the best recommended male and female attendant in the British asylums for the insane.

Sir Alexander was twice married. His sons have all died before him. He has left a widow, several daughters, and numerous grandchildren, to lament him. His funeral took place on the 20th March, in the churchyard of Currie; the Royal College of Physicians sent a deputation of their Fellows, as a testimony of the respect they entertained to the Father of their College.

THE NEW PHARMACOPŒIA.

WE have every reason to believe that the new Pharmacopœia is now in course of active preparation at the hands of Messrs Redwood and Warrington, who have been appointed by a sub-committee of the Medical Council to undertake the matter. No doubt the errors contained in the former editions will be rectified, and several valuable preparations which are used very largely by the majority of medical men, but have unfortunately been omitted, will be replaced. We may indicate, for example, the *acetum cantharidis*, the *liquor hydragryi bichloridi*, the *infusum aurantii compositum*, and the *infusum gentianæ compositum*; in addition, it is probable that some remedies which have been getting into favour of late, such as carbolic acid, may be likewise added. An attempt will also be made to secure a greater uniformity, not only in the arrangement of the contents, but the use of terms. The former edition contained some few contradictions which it would be well to avoid. This was, no doubt, due to the fact that the compilation was effected by several gentlemen who held different views upon similar subjects. The terms "sesquioxide" and "peroxide," for example, will be employed each in a uniform sense. The first and second parts of the Pharmacopœia, including the *Materia Medica* and preparations respectively, will be incorporated, and the contents arranged in alphabetical order. At present the descriptions of some drugs are separated in twain; some details of the mode of preparation, for instance, are given under one, and the properties under another head. In the forthcoming edition all that is to be said about any one preparation will be said in one place once for all. The Medical Council has expressed a desire at various meetings that, at least a month before the publication of the new edition, the proof sheets of the latter may be placed in the hands of its members, in order that the contents may undergo careful criticism, and that corrections may be suggested if necessary. We believe that the greater part, if not the whole, will be ready for this purpose.—*Medical Times and Gazette*.

SCIENTIFIC REPORT ON THE TRICHINA.

THE Prussian Government had intrusted investigations on the nature and mode of propagation of the trichina to Professor Kühne, of Halle, and zealously were they pursued from 1863 to 1865. The report has been sent in, and states, among other things, the following particulars:—Pigs affected with trichinæ do not present symptoms sufficiently obvious to be recognised by breeders. It was therefore incumbent on the reporter to elucidate facts connected with diagnosis. Animals were for that purpose fed with meat full of trichinæ, and daily watched by veterinarians. The latter could hardly detect any change in the health of the animals, nor did slight indispositions present anything which could be construed into a pathognomonic symptom of the disease; yet it was proved by post-mortem examination that trichinæ had formed in large masses in the muscles of the animals. Hence the breeder cannot be expected to recognise the existence of the malady, nor should he be punished as having sold diseased meat. No difference of race, sex, or age creates a greater or less aptitude to be invaded by the trichinæ. It is not true, as has been asserted, that trichinian disease is produced by feeding pigs with beetroot, by the ingestion of earthworms, or of moles containing trichinæ. The investigators have found that the parasites of the beetroot and the moles are not identical with the trichinæ which infest the pig. Not so, however, with rats and mice, which are sometimes devoured by pigs. Cats also may be thus contaminated: the latter are especially susceptible. It is therefore important to prevent pigs from eating dead animals. It is very likely that foxes and weasels may be very dangerous in this respect; but it is plain that pigs can but seldom feed upon them. Dogs are also very liable to breed

trichinæ, but birds are quite insusceptible. Trichinæ, which have passed into the stomach of pigs with their food, are therein completely destroyed; hence it may be inferred that the fecal matter of diseased animals is not noxious. The culinary precautions to be held in view when cooking pork are the following:—The process of boiling or roasting should be carried on with the greatest care. People should not partake of any meat that has not been sufficiently cooked, and avoid such roast pork the centre of which still presents traces of blood. By salting and smoking ham for about ten days, the trichinæ are quite destroyed.—*Lancet*.

PROFESSORS OF THE MEDICAL SCHOOL OF OHIO.

THE following passages form part of an address lately delivered by Dr M. B. Wright, descriptive of deceased professors of the Medical School of Ohio:—

John P. Harrison was the compeer of those already named, yet he was as unlike them all as they were unlike each other.

Lawson was about medium height, perhaps rather small; not clumsy in his movements, nor yet strikingly elastic.

Drake was slender, nervous, and when impatient to advance, took long and awkward strides.

Shotwell, after mature manhood, was inclined to corpulency, and while walking, an observer would be impressed with the idea that he was listening for the salutation of some boon companion, "How are you?"

Locke, in his head and frame, was large. He had no gait—in other words, his movements were in accordance with his ideas, straight or crooked, just as the subject occupying his mind was clear or abstruse. He generally knew where he was going, but he did not always know where he was. He accumulated locomotive power enough at the start to carry him where he desired to go, and then his intellect shot off among the stars, or bounded over the earth in quest of facts and illustrations from nature and art.

Harrison was tall, slender, and elastic.

Shotwell's style of delivery was neither attractive nor repulsive. It had a kind of every-day familiarity about it which made those feel at ease who became engaged in listening. He had neither the gift nor the inclination to multiply words, but his language was to the point, and his demonstrations were attractive. His voice had none of the smoothness and polish of a careful, practised speaker, but it was full-toned and distinct in its articulations.

The casual listener was seldom impressed favourably with the elocution of Locke, yet he secured attention by the freshness of his investigation, and the confidence pervading the minds of all, that in him science and truth had found a worthy representative. He worshiped nature with an enthusiasm bordering on idolatry, and when describing her, in all her simplicity, beauty, and grandeur, his enthusiasm became electric.

Whilst most men commenced their addresses calmly and cautiously, as if feeling their way, becoming more and more animated as the subject expanded into distinct proportions, Harrison enunciated his propositions with as much clearness, earnestness, and elevation of voice, as if exulting over success already achieved.

Lawson was calm in his feelings, condensed in language, and somewhat monotonous in his utterances. He witnessed life in all its phases of suffering and want, but his facts were compared, methodized, and estimated in solitude. Link after link was connected, until the chain was complete; the one exciting no more enthusiasm than the other, hence the quiet, self-possessed thinker, became the quiet, unimpulsive lecturer.

Drake commenced speaking slowly, with long-drawn words and prolonged intonations, and with an expression that seemed to reflect a degree of sanctity upon the subject. But after the basis of his argument had been substantially laid and clearly defined, he rose in animation, until he appeared to be sus-

taining his views as a part of his very self. Yet, his whole powers were very seldom manifested except in the midst of strong opposition. Then no man could fail to acknowledge his skill in the use of the rules of logic or the weapons of debate.

Words were always at command, sentences were abridged, lengthened and rounded at will. If his theories were not always right, they were presented with such zeal and honesty of purpose as to make us regret they were ever wrong. The man who desired to assail his opinions always found the sentinel at his post, always observed the fires of his genius burning, or in a condition to be easily kindled; always ready to meet opponents from whatever quarter they might come. In argument, he could with equal skill use any weapon justified by courtesy, or the importance of the subject. A standing sentiment with him was, "Make your antagonist afraid of you, and your triumph will have been half secured." By some of his admirers this sentiment has been grossly misconstrued. They are always effervescing with bad temper, always ready to provoke a quarrel. He did not intend to convey the idea that fear should be induced by an exhibition of pugilistic propensities, by an expression of brute force, but that young men should so prepare themselves with knowledge as to be always formidable in the defence of truth.—*Cincinnati Journal of Medicine*.

THE COLLEGE OF SURGEONS AND THE MEDICAL COUNCIL.

THE Medical Council will have at least one important piece of business to discuss at their May meeting. They will have to consider the reports of the visitors of examinations. We are sanguine enough to expect considerable advantage from the carrying out of this scheme, although it has not been done in that systematic and business-like way that we could have wished. No doubt the courtesies interchanged have been as delicate and agreeable as described; and when the *crème de la crème* is employed in mutual visitations, of course the most exquisite politeness must be expected. But we do not doubt that there will be some body beneath the froth; and we feel quite sure that none of the visitors can have brought away from the College of Surgeons a satisfactory report as to the extent of the examinations on which that august body now give diplomata of fitness to practise. We shall be greatly astonished if the reports of the Visiting Committee do not indicate, with the most refined and courteous delicacy,—such as we fear is sometimes found to be scanty in our comments on that body,—that their examinations are deficient precisely in the points which we have signalized; and that a diploma which declares a candidate to be fit to practise in this kingdom, and places its holder on the Register, without his having been examined in Medicine, Materia Medica, Clinical Medicine, Clinical Surgery, Chemistry, or Botany, requires remodelling, and discredits the body which issues it.—*The Lancet*.

GLASGOW MEDICO-CHIRURGICAL SOCIETY.

AT the meeting of the Society, held on Tuesday 13th March, in the Hall of the Faculty of Physicians and Surgeons, the following gentlemen were elected office-bearers for the present year:—*President*—Dr Allen Thomson. *Vice-Presidents*—Dr Robert Paterson; Dr John Coats. *Council*—Dr Naismith, Hamilton; Dr W. T. Gairdner; Dr Yeaman; Mr Robertson, Renfrew; Dr Dewar; Dr Tindal. *Secretaries*—Dr James Adams; Dr Robert Perry. *Treasurer*—Dr Howatt.

COMMISSIONS IN BRITISH MEDICAL SERVICE.

THE Director-General encloses a List of the Candidates of Her Majesty's British Service who were successful at the Competitive Examination in August last, and who have passed through a Course at the Army Medical School, showing the combined results of the Examination.

<i>Names.</i>	<i>Studied at</i>	<i>Marks.</i>	<i>Names.</i>	<i>Studied at</i>	<i>Marks.</i>
O'Reilly, J. J.	Dublin,	4485	O'Brien, H. J.	Dublin,	3680
M'Watters, W.	Dublin,	4310	Kilroy, P. le F.	Lond.&Dub.	3480
Mally, R. N.	Dublin,	4308	Alexander, W.	Belfast,	3468
Catherwood, W. A.	Belfast,	4260	Lowe, R. W.	Edinburgh,	3435
Clarke, E. J.	Dublin,	4183	Purden, J. E.	Dublin,	3423
Johnston, W.	Aberd.&Ed.	3963	Connellan, E.	Cork,	3176
D'Arcy, E. F.	Dublin,	3903	Mackinnon, H.W.A.	London,	3045
Mouat, G. B.	Edinburgh,	3776	Brodie, J. F.	Galw.&Dub.	2860

CORRESPONDENCE.

(To the Editor of the *Edinburgh Medical Journal*.)

42 Queen Street, Edinburgh, March 17, 1866.

SIR,—Dr Charles Bell has kindly recommended that I should publish my claim to the priority of the invention of a bandage with intra-uterine support, such as he described a month or two back in your Journal. The proofs of my claim are too long for insertion; and as he acknowledges the truth, if you will simply mention the fact that mine dates about four years anterior to Dr Bell's, and was known to some of our medical brethren, though not to him, you will oblige your most obedient servant,

JAMES A. SIDNEY.

PUBLICATIONS RECEIVED.

- Althaus,—*Progressive Locomotor Ataxy.* By Julius Althaus, M.D., etc. London, 1866.
- Beigel,—*On Inhalation.* By Hermann Beigel, M.D. London, 1866.
- Belcher,—*Neligan's Practical Treatise on Diseases of the Skin.* Second edition. By T. W. Belcher, M.D., etc. Dublin, 1866.
- Brown,—*On the Curability of certain Forms of Insanity, Epilepsy, Catalepsy, and Hysteria, in Females.* By Baker Brown, F.R.C.S., etc. etc. London, 1866.
- Canstatt's Jahresbericht. Vols. IV. and V., 1864. Würzburg, 1865.
- Dobell,—*Nature, Cause, and Treatment of Tuberculosis.* By Horace Dobell, M.D. London, 1866.
- Dobell,—*On Winter Cough.* By Horace Dobell, M.D. London, 1866.
- Drysdale,—*Prostitution medically considered.* By Dr Drysdale. London, 1866.
- Faye,—*Defectus Uteri et Vaginæ.* Ved Dr F. C. Faye. Christiania.
- Gamgee,—*The Cattle Plague.* By John Gamgee. London, 1866.
- Lee,—*Diseases of the Veins, Hæmorrhoidal Tumours, and other Affections of the Rectum.* By Henry Lee, F.R.C.S., etc. Second edition. London, 1866.
- Morgan,—*Danger of Deterioration of Race.* By J. E. Morgan, M.D. Oxon. London, 1866.
- Muter,—*The Alkaline Permanganates.* By John Muter. London, 1866.
- New Sydenham Society,—*Atlas of Portraits of Diseases of the Skin; Fifth Fasciculus.* Obstetrical Society of London, Transactions of the. Vol. VII. for 1865. London, 1866.
- Owen,—*The Anatomy of Vertebrates.* Vol. II., Birds and Mammals. By Richard Owen, F.R.S., etc. London, 1866.
- Reich,—*Unsittlichkeit und Unmässigkeit.* Von Dr Eduard Reich. Leipzig, 1866.
- Wood and Sharp,—*The Year-book of Pharmacy.* By C. H. Wood, F.C.S., and Charles Sharp. London, 1866.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Ligature of the Femoral Artery for the Thirty-fifth Time, with Remarks on the Treatment of Aneurism.* By JAMES SYME, F.R.S.E., Surgeon to the Queen in Scotland, Professor of Clinical Surgery in the University of Edinburgh, etc., etc.

WILLIAM M'D., æt. 26, was admitted into the hospital on the 18th of November last, for a large femoral aneurism of two months' duration. I tied the artery on the 21st, and the patient, who suffered nothing from the operation, which had been done under chloroform, was immediately relieved from the excessive pain previously endured with hardly any cessation. The wound healed by the first intention, except at the ligature, which was detached on the 13th of December; and everything went on well afterwards.

In the first paper of the first number of this Journal (1841), I offered some observations on ligature of the femoral artery for aneurism, with a view of showing that the alleged danger of this operation depended upon its faulty performance, and that it admitted of being employed nearly, if not entirely with safety. It may not be amiss to repeat here what was said there:—"It is established that the great sources of danger from the ligature of large arteries are undue laceration and separation of the connexions of the vessel, whence hemorrhage is apt to ensue, and injury to the coats of the veins, which is apt to occasion inflammation and an obstructing coagulation, causing mortification of the limb. The subclavian artery, when tied at the external edge of the scalenus, lies at some distance from the vein, and neither the carotid nor the external iliac artery adheres so intimately to its accompanying venous trunk as to render it at all difficult or dangerous to pass the needle. But the femoral artery has a closer connexion with the vein, and though it is felt by the operator's finger, after the fascia has been opened, round and distinct, and as if insulated from the surrounding parts except by the loosest connexions, any attempt to pass the ligature without further dissection, either proves abortive, or, if executed by force, exposes the patient to the greatest danger. I have seen a gush of dark-coloured blood proclaim transfixion of the vein. I have seen, on dissection, a portion of this vessel included in the ligature; and

I have also seen the external coat alone grazed as it were by the needle, but, nevertheless, excited to fatal inflammation. If, on the other hand, this danger be avoided by using blunt instruments, or the finger, to detach the artery from its connexions, the patient is exposed to the hardly less disastrous consequences of hemorrhage through ulceration or sloughing of the vessel.

To tie the femoral artery safely, the surgeon should be impressed with the conviction that the operation is one not of difficulty but of great nicety. He should make an incision between two and a-half or three inches long in the proper situation, cut through the fascia to a smaller extent, and expose the sheath of the vessels. So far he can hardly go wrong, but then, instead of hastening to pass the needle, he should by ligature, or the temporary application of spring forceps, close every little vessel that discharges enough of blood to obscure distinct vision of the object he has in view. Let him now seize the sheath with dissecting forceps, and, gently raising it, make a small opening by means of a straight narrow sharp-pointed knife. The cellular and fatty substances which envelop the vessels in variable quantity are next to be elevated and divided in successive portions until the external coat of the artery appears quite distinct and *white*, when the needle may be passed without the slightest difficulty or danger."

At the time when this was written, my experience did not extend beyond seven cases, all of which had proved successful; but since then there have been twenty-eight more, so as to make in all thirty-five, and in none of these has the operation proved unfortunate, with the exception of one, which terminated fatally from suppuration of the sac. But even on this solitary occasion the result could hardly be attributed to the operation, as the parts concerned with it were found perfectly sound and free from the morbid condition, which seemed distinctly referable to the derangement of health that had been induced by a prolonged attempt to cause coagulation through pressure at the groin. This treatment, continued for many days and nights, had disordered the patient's sleep, appetite, and mental composure, so as in all probability to excite a proneness to unhealthy action. Should such an explanation not be deemed satisfactory, the result of my experience in ligature of the femoral artery would still be one death in thirty-five cases.

The local means employed for the remedy of aneurism may be divided into those which are effectual and those which are tentative or uncertain. The former comprehend amputation, ligature on the cardiac side of the tumour, and laying open the sac, so as to secure the vessel at its seat of rupture; while to the latter may be referred pressure, injection into the sac of fluids calculated to promote coagulation, and the agency of galvanism. It is obvious that the effectual means should be preferred in practice to those that are doubtful, unless some good reason can be shown for choosing the

latter instead of the former ; and it may now be inquired how far this admits of being done.

Amputation can be justifiable only in cases altogether beyond the reach of any other remedy. Thus, a hundred years ago, before the Hunterian operation had been devised, amputation of the thigh was practised in cases of popliteal aneurism ; and Mr Wilmer of Coventry tells us that up to his time (1780) he was not aware of a single instance in which the patient had retained his limb. But now it would be considered unwarrantable to amputate, except when mortification had either taken place, or was evidently impending. Thus, in a case of axillary aneurism, where gangrene had extended from the fingers to the shoulder, I amputated at the joint through the tumour, and secured the artery close to the clavicle, with the result of a perfect recovery.

Laying open the sac and securing the artery at its seat of rupture, or the "old operation" as it has been called, is certainly in general the most expedient method for remedying aneurism of a traumatic origin, since the great principle established by Mr John Bell, and enforced by Mr Guthrie, of arresting arterial hemorrhage by means applied directly to its source, is no less applicable after than before the external wound is healed. In ten cases of brachial aneurism, resulting from wounds in venesection, I have pursued this course with success ; and when the circulation can be commanded by a tourniquet, as in that situation, nothing can be easier than the operation. But when the tumour is so near the trunk as to prevent constriction of the artery concerned, there are other means that may be employed. In a case of carotid aneurism, having its base rather lower than the sternum, which had been caused by the thrust of an assailant's knife, I opened the sac so as to admit my finger, and closed the orifice with it until the vessel was exposed and secured on both sides of the aperture. In a very large aneurism of the gluteal artery, resulting from a stab sustained seven years before the patient came under my care, I introduced one finger after another until my whole hand was in the cavity, where it detached the firmly adhering clots, so as to place the injured vessel within reach of the ligature, when the sac was laid freely open. In axillary aneurism, again, I have found it sufficient to make an incision above the clavicle to admit the finger of an assistant, and allow it to exert effectual compression of the artery where it passes over the first rib.

It has been supposed that the old operation, in its most extended employment, should be limited to traumatic aneurisms, but I have endeavoured to show¹ that there are some situations, and more especially the axilla, where it should be in general preferred on the ground of safety as well as facility. The disease usually originates here at some distance below the clavicle, in consequence of a sudden and violent action of the arm ; and although the sac may attain a large size, it does not denote an equivalent destruction of the artery,

¹ Trans. Med. and Chir. Soc. of London, vol. xliii.

which, on the contrary, may remain as sound within the cavity as when lying beyond it at an earlier stage of its progress. Encouraged by this consideration, I have repeatedly amputated with success at the shoulder-joint through axillary aneurisms, and still more frequently performed the old operation for their remedy, even when the tumour had attained an immense size. In the only case of this kind that proved fatal, the tumour had caused absorption of several ribs, so that the pleura formed part of the sac, and the patient died from inflammation thus induced, rather than from any effect directly due to the operation.

Whatever difference of opinion there may be as to priority in devising the modern operation for aneurism, there can be no doubt that we are indebted for its improvement and perfection, pathological as well as practical, to our own countrymen, and more particularly to Jones, Lawrence, Travers, Hodgson, Cooper, and Liston; who, instead of the rude and clumsy procedures previously in use, established the principle of employing a single small firm silk ligature, and passing it round the vessel without any further disturbance than what was requisite for doing so. It would appear that the progress of improvement on the Continent has not kept an equal pace, or, indeed, has rather stood still: since a last year's graduate of Edinburgh, who has just returned from a great German school, tells me that he saw the operation performed there precisely as I was taught to do it more than forty years ago by M. Lisfranc of Paris,—the dissection being performed, not with forceps and knife, but upon a grooved director, which, after boring through successive layers of superjacent tissue, was finally thrust under the vessel as a guide for the ligature. It is nowise surprising that the ligature so applied should not stand high in the confidence of Continental surgeons, or that distrusting it they should cast about for less dangerous means of treatment, such as the injection of coagulating fluids into the sac, and the transmission of galvanic currents through it. It is to be hoped that sooner or later they will follow our example; but, in the meantime, it is surely not desirable that we should follow theirs. So long as internal aneurisms threatening to prove speedily fatal are the subjects of experiment, the propriety of venturing on a forlorn hope may be left for consideration to the surgeon and his patient; but it would be sad indeed if we were to substitute questionable and uncertain expedients for the safe and effectual operations which have been established in our confidence by long and successful practice.

Of the uncertain means, pressure is the least objectionable, and in the treatment of popliteal as well as femoral aneurisms, when applied at the groin, has frequently proved successful; but it has also not unfrequently failed and produced bad consequences, as would more distinctly appear if the failures had been no less carefully recorded than the successes. This method, however, has the advantage of being practicable by those who are not prepared to tie

the artery with safety, and therefore may be employed when the circumstances do not admit of this being done. But while to this extent regarded as a useful substitute for the ligature, pressure must be held of inferior value to it when properly executed, on the grounds of facility, rapidity, painlessness, and safety: so that a surgeon who endeavours to cultivate the most beneficial exercise of his art, making excellence and not mediocrity the object of ambition, will hardly hesitate as to which of the two methods should be preferred.

ARTICLE II.—*On Malformations of the Organs of Generation.* Second Series. *Fœtation in a Rudimentary Uterine Cornu.*¹ By WILLIAM TURNER, M.B. Lond., F.R.S.E., Senior Demonstrator of Anatomy in the University of Edinburgh.

IN the months of January and February 1865, I recorded in this Journal several cases of congenital malformation of the organs of generation. Amongst these was a series of cases, obtained from the dissecting-room, of malformed uteri, all of which were in the unimpregnated state. During the past year I have had the opportunity, through the kindness of Sir James Y. Simpson, of dissecting two additional specimens of the uterus bicornis, in each of which one horn was in a rudimentary condition, but impregnated. I purpose describing these cases in this communication, and discussing the mode in which impregnation had been effected.

CASE 1.—*Uterus bicornis unicollis. The left horn rudimentary and pregnant. Rupture of the left horn, and escape of the fœtus into the cavity of the peritoneum.*

In the month of May 1865, I received for examination from Professor Sir James Y. Simpson a preparation consisting of a uterus and appendages, with the vagina and bladder attached, which had been sent to that gentleman by Dr Brotherston of Alloa. An inspection of the preparation soon satisfied me that it was one of much interest, and I requested Dr Brotherston to furnish me with some of the particulars of the case, with which request he has very kindly complied. His account is as follows: "I was called on the morning of the 11th of May to visit C. G., about 20 years of age, who had just been taken suddenly ill. I found her lying in bed with her clothes on; she was pulseless, very pallid, and drawing heavy sighs. She complained of sickness, swimming in the head, and ringing in the ears. On examining the abdomen, I could not discover anything unnatural, but there was slight pain on pressure. I ordered her brandy and warm water, with a large sinapism to the

¹ Read before the Obstetrical Society of Edinburgh, 14th February 1866. The preparations were at the same time exhibited.

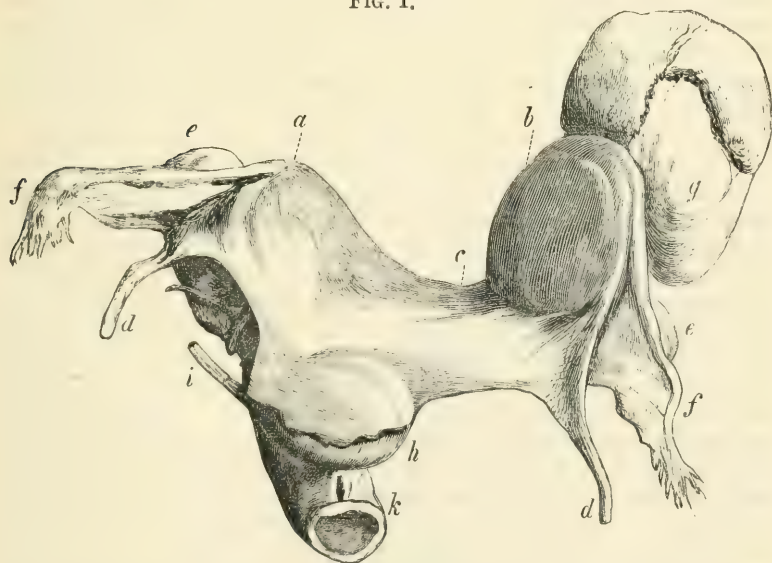
abdomen. She did not retain the brandy, but vomited it almost immediately. I examined the os uteri and found it a little relaxed. There was no vaginal discharge. I gave her more brandy and water and left her. I saw her again about half-past nine, and found no improvement in her symptoms; if anything she appeared weaker, and was evidently sinking. Dr Duncanson kindly saw her with me, but she died about noon the same day. By order of the procurator-fiscal, a post-mortem examination was made by Dr Duncanson and myself. The body presented nothing unusual externally. The scalp and pericranium were blanched, but the brain, with its membranes, and the thoracic viscera were all in a healthy condition. The cavity of the abdomen was next examined, and found to contain a large quantity of blood, to the extent of four and a-half pounds. This blood, the source of which was not at first apparent, we carefully removed, and a foetus was discovered lying in the cavity of the peritoneum, which had apparently reached the period of three months. The foetus was enveloped in its membranes, which were unruptured. The uterus itself was found to be a little enlarged, but empty, and displaced to the right side by the pressure of the foetus. Death had evidently been occasioned by hæmorrhage due to rupture and escape of the foetus into the cavity of the peritoneum."

A careful consideration of the anatomical characters of the preparation led me to form the conclusion that I had before me one of those interesting cases of malformation of the uterus in which the primary or embryonic subdivision of the organ into two distinct cornua had remained as a persistent condition in adult life. One of the cornua, viz., the left, not so fully developed as the other, was impregnated, and the hæmorrhage attendant on the rupture of this horn had evidently been the cause of the death of the patient. That this was the nature of the preparation may be gathered from the following description :—

Connected in the usual manner with the upper end of the vagina (*k*) was the cervix uteri, which was continued into a somewhat ovoid-shaped organ (*a*), looking at first sight like the body of the uterus, but which a close examination showed to be not the entire organ, but only the right cornu of a two-horned uterus. For, instead of lying in the same line as the vagina and cervix, it inclined obliquely upwards and to the right, and terminated superiorly in a rounded end. It was invested by peritoneum anteriorly, posteriorly, and on its left side, whilst from the right side the membrane passed away from it, forming the folds of the right broad ligament. The left margin was convex and free. The extreme length from the os externum to the apex of this horn was nearly four inches, whilst the greatest breadth of the cornu was about two inches. The right round ligament (*d*), Fallopian tube (*f*), and ovary (*e*), with its ligament were connected to the apex of this horn. Their relation to each other at their points of attachment

was as in a well-formed uterus. The Fallopian tube, including its fimbriæ, was four and three quarter inches long. Air blown in at the fimbriated end entered the cavity of the cornu. The right round ligament arose from the cornu by a broad expanded portion, some of the fibres of which extended as far as the junction of the horn with the cervix. The organ of Rosenmüller was well seen in the ovario-tubal fold of peritoneum. The walls of this uterine horn were thicker than those of the unimpregnated uterus, and the cavity contained a thick and firm decidua, but no foetus. The cervix was filled by a plug of tenacious mucus. The vagina, os, and cervix were single. The cavity of the right cornu was continuous with the cervical canal. The ovary was flattened on both its surfaces, and from the anterior a small pea-like pedunculated tumour projected. It contained no corpus luteum, but faint traces of old cicatrices were seen on its outer surface.

FIG. I.



Note of Explanation to FIG. I.—The parts are exhibited with as small a disturbance of their proper relations as possible. *a*, right; *b*, left uterine cornu. *c*, pedicle of latter covered by peritoneum. *d d*, round ligaments. *e, e*, ovaries. *f, f*, Fallopian tubes. *g*, foetus and membranes. *h*, bladder. *i*, ureter. *k*, vagina. FIGS. I. and II. are engraved from careful drawings of the preparations made by my pupil Mr Richard Caton.

Springing at almost a right angle from the left side of the right uterine cornu, close to its place of junction with the cervix, was a flattened band (*c*), which acted as a sort of foot-stalk of attachment for the dilated part of the left cornu, and may therefore be appropriately called the pedicle. This band extended upwards and to the left for somewhat more than one inch and a-half, and then

became continuous with, or rather expanded into, the dilated portion of the left uterine cornu (*b*). The pedicle measured one and quarter inch in circumference. It was invested by peritoneum, which was continuous on the one hand with the peritoneum covering the left,—on the other with that covering the right uterine cornu and bladder. When this membrane was removed, the pedicle was found to be principally composed of muscular fasciculi, but a number of dilated bloodvessels, both arteries and veins, ran along it to the dilated part of the left horn. This horn was somewhat pyriform in shape; its circumference at its widest part was about nine inches. Along the outer and posterior part was a rupture three inches long, through which a fœtus, invested by its membranes, together with a vascular placenta-like mass, was extruded. To the proper apex of this horn the round ligament (*d*), Fallopian tube (*f*), ovarian ligament and ovary (*e*) of the left side, possessing their normal relations to each other, were attached. The uterine end of the tube was about three and a-half inches from the spot where the pedicle expanded into the cornu. The Fallopian tube, measured from its place of attachment external to the dilatation, was, including its fimbriæ, five inches long. Air blown into its fimbriated end readily entered the cornu. The organ of Rosenmüller could be plainly seen in the ovario-tubal peritoneal fold; a cyst the size of a small hazel-nut, apparently a dilatation of one of its tubules, was connected to the outer end of this organ. The anterior surface of the left ovary was flattened, but the posterior presented a well-marked bulging at its outer part, which, on section, was found to be the seat of a corpus luteum. The ovary exhibited a cicatrized appearance externally. The round ligament was expanded at its origin, and extended from close to the Fallopian tube down to the pedicle. The wall of the left cornu was muscular, like that of a pregnant uterus, and at the place of rupture the placenta could be seen partially adherent to its inner surface. The pedicle was examined very minutely to see if any canal connecting the cavity of the impregnated horn with that of the right horn, or the cervix, or vagina, could be seen, but none could be detected, although, in the course of the examination, the muscular fasciculi of which the pedicle was composed were dissected from each other; neither could any orifice at either of the extremities of the pedicle be detected.

CASE 2.—Uterus bicornis unicollis. The left horn rudimentary and pregnant. Retention of the fœtus after the full period of utero-gestation.

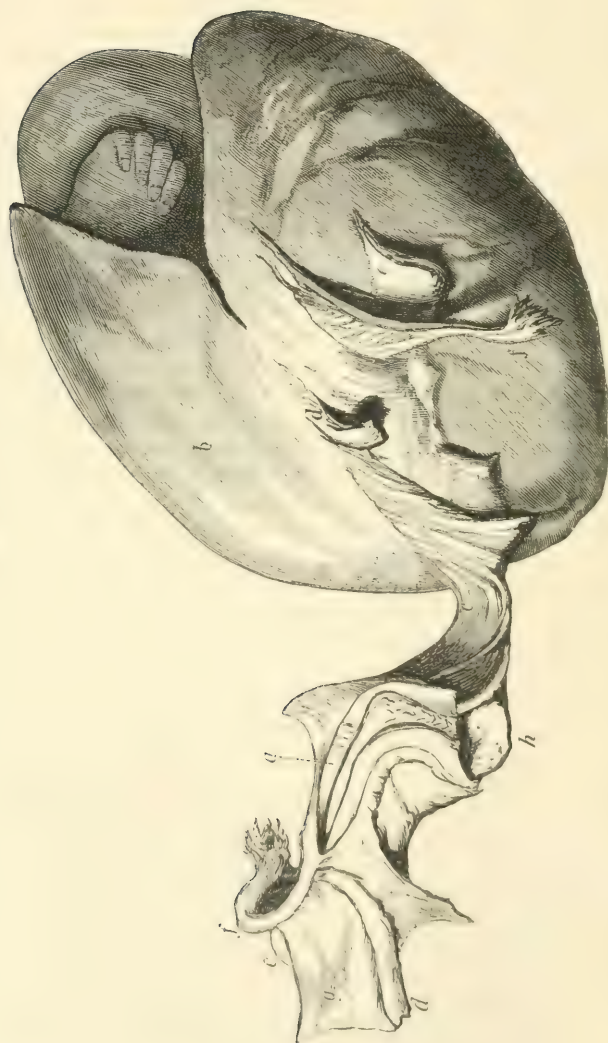
After completing the examination of the specimen just described, I asked permission of Sir James Y. Simpson to look through the large stock of preparations contained in his obstetrical museum, to see if it included anything of a similar nature. He very courteously acceded to my request, and has permitted me to give an account, in this place, of the very interesting and as yet unrecorded

specimen I shall now describe. The specimen had been sent to Sir James Y. Simpson by Dr Scott of Dumfries, and from the latter gentleman the following history has been obtained:—"I was called on to attend Mrs M'Q., æt. 35, a married woman, who was stated to be in severe labour and at her full time. On examination I found the os uteri low down in the vagina, and I then could easily find the uterus of a natural size and unimpregnated. There was an enlargement of the abdomen extending a little to the left side, and nearly of the same size and shape as a uterus, containing a foetus at the full period of utero-gestation. The foetal heart was easily heard, and the motions of the child were strong. The pains were very severe, and complicated with convulsions for a whole day, in spite of a free use of chloroform, which certainly modified them. The pains continued for several days, and then the patient began to go about as usual to the astonishment of her friends and neighbours. The enlargement of the abdomen became gradually less, so that at the time of her death (which took place from phthisis, six months after the date of the supposed labour) it was not more than one third of its original size when I first saw her. The parts, including the empty uterus and appendages, were carefully removed on a post-mortem examination, and sent to Professor Simpson."

The preparation had been lying for a considerable time in spirit, and the textures had consequently lost their normal colour. The most prominent feature in the specimen was a large irregularly ovoid sac, measuring about twenty-seven inches in its greatest circumference. This sac contained a male foetus, apparently about the full time, which was attached by a funis, one foot in length, to a shrivelled placenta, which, in its turn, was connected to the inner surface of the sac. One side of the sac was affixed by a pedicle to the cervix uteri, from which a structure, looking at first sight like the body of the unimpregnated uterus, proceeded. The problem which had to be solved, therefore, was the nature of the sac in which the foetus was contained. Was it something altogether foreign to the uterus? Was it a dilated Fallopian tube? Or, was the case one in which the primary division of the uterus in two cornua, persisting to adult life, had permitted foetation to take place in one of the horns, leaving the other in the unimpregnated state? From a careful examination of the anatomical characters of the preparation, I have satisfied myself that the last named is the correct view to be taken of the case.

A portion of the uterine end of the vagina (*h*) had been preserved, and from its upper part the cervix uteri (*i*), one inch in length, proceeded in the usual manner. Continuous with the upper part of the cervix was an ovoid-shaped organ (*a*), one inch and a-half long, which, instead of possessing the normal direction of the uterus, inclined obliquely upwards and to the right, and terminated in a somewhat pointed extremity, from which the right round ligament (*d*), Fallopian tube (*f*), and ovarian ligament with its ovary (*e*), proceeded. Its

FIG. II.



Note of Explanation to FIG. II.—The letters indicate the same parts as in FIG. I., except that *g* is the broad ligament, *h* the vagina, and *i* the cervix uteri.

left margin, convex and free, was invested, together with its anterior and posterior surfaces, by peritoneum, which membrane passed away from the right margin of the organ, and formed the broad ligament (*g*). The organ was evidently, therefore, the right uterine cornu. When cut into, the canal in its interior was seen to be continuous with the upper part of the canal of the cervix, the two forming an obtuse angle with each other. By its opposite extremity, the canal communicated with the interior of the right Fallopian tube (*f*), which was pervious throughout. Several bands of false membrane were affixed to the posterior aspect of this uterine horn, and the Fallopian tube, about two and a-half inches in length, was firmly tied down and permanently curved on itself, by adhesions of a similar nature. There was no mucous plug in the cervix uteri, and no decidual structure in the right cornu, and the walls of the latter presented no increase of thickness over what is seen in the unimpregnated state.

Attached to the left side of the cervix was a strong muscular band or pedicle (*c*), two inches long by one broad, which passed to the left and ended in the wall of the foetus-containing sac (*b*), already described. The fibres of which this band was composed were continuous with those of the cervix and right horn on the one hand, and with those of the sac on the other. The band was invested by peritoneum, which was also continuous with the serous covering of the right horn and the sac. Dilated arteries and veins ran along the pedicle to the sac. The left Fallopian tube (*f*) with its peritoneal fold was connected to the proper apex of the ovoid sac: two and a-half inches from and posterior to the point of attachment of the tube, the ovarian ligament with its ovary (*e*), and at about the same distance from, but anterior to the tube, the round ligament (*d*) arose from the wall of the sac. The distance between the place of termination of the pedicle and the point of attachment of the Fallopian tube was a little more than four inches. From these relations there can be no doubt but that the sac was the dilated and impregnated left uterine cornu; the increased distance between the points of attachment of the various structures connected to the apex of the cornu, as compared with the corresponding parts on the right side, being due to the growth of the walls of the sac, which had occurred during gestation. The left Fallopian tube, measured from where it arose from the sac, was five inches in length; and a fine probe could be passed along it up to the wall of the sac, but its inner orifice was obstructed. No adhesions interfered with its mobility. The organ of Rosenmüller was distinctly seen in the ovario-tubal peritoneal fold; but on the right side, from the thickened state of the membrane, this structure was not visible. The surfaces of both ovaries were flattened, and showed faint traces of old scars. No corpora lutea were to be seen.

As it was of importance to ascertain the nature of the communication between the interior of the impregnated left uterine and cornu

the canal of the vagina, cervix, or unimpregnated horn, a careful examination of the pedicle was instituted; but neither at its cervical end nor where the pedicle expanded into the sac of the left horn could any opening be detected. The peritoneum was then carefully dissected off the pedicle, and with a sharp knife a section was made through the structure at right angles to its long axis. When the surface of this section was examined, several small openings were seen. The question now was, Could any of these openings be a divided canal extending along the pedicle, or were they all divided bloodvessels? To determine this, I introduced an injecting pipe into more than one of the vessels already described as extending along the pedicle, and forced a coloured fluid along it. The fluid oozed out at the various openings, and thus established their connexion with the vascular system. Through the larger openings, also, it was possible to pass bristles into the blood vessels. I was thus forced to the conclusion that the pedicle did not contain any canal connecting the impregnated horn with the vagina, cervix, or cornu of the right side.

The appearance presented by the fœtus, placenta, and walls of the sac, indicated very clearly that all active changes had been suspended in them some time before the death of the mother. Absorption of the foetal tissues had commenced, more especially in the dorsal region, where a large patch of skin, with the subjacent muscles, had disappeared, and the ribs and laminae of the vertebrae were exposed. The placenta was tough and shrivelled, —much more so than would have been produced by the action of the spirit of wine with which the specimen had been preserved. The walls of the uterine sac were thin and atrophied. In places the muscular fibres had almost entirely disappeared, and large calcareous plates, indicating an extensive degeneration of the walls of the sac, were abundantly distributed over the inner surface.

In the following remarks, I shall confine myself to a consideration of those features in the two cases I have just related, which possess an anatomical and physiological interest; for the symptoms, diagnosis during life, and treatment of cornual gestation, fall more within the province of the accoucheur than the anatomist. In the first place, then, it may be as well to consider what the anatomical characters are by which cases of cornual pregnancy may be distinguished from tubal pregnancies, with which, on a hasty inspection, they are apt to be, and indeed have not unfrequently been, confounded. And it may be the more advisable to point out what these diagnostic characters are, because, in this country at least, the subject has received but little attention.

In forming the diagnosis, the structures to which the attention should especially be directed are the round ligament and the Fallopian tube of the impregnated side, and their relative position and place of origin should be carefully determined. In a normal

uterus, the round ligament, as is well known, springs from the upper angle of the uterus immediately in front of the Fallopian tube. If the pregnancy be tubal, the round ligament will, as a matter of course, be found attached to the body of the womb on the inner or uterine side of the dilated sac containing the embryo: if, on the other hand, the pregnancy be cornual, then the point of attachment of the round ligament will be on the outer side of the dilated sac in which the foetus is situated. In both the specimens I have just recorded, the latter relation was the one observed. In tubal pregnancies, also, the length of the Fallopian tube on the impregnated side, external to the dilatation, is necessarily less than that of the entire extent of the unimpregnated tube, and the diminution in length is more strongly marked the nearer the sac lies to the fimbriated extremity. In cornual pregnancies, on the other hand, no diminution in the length of the tube on the impregnated side occurs,—nay, as both these cases show, the tube on that side external to the embryo-containing sac may even be longer than that on the side which is unimpregnated.

The period up to which gestation may be carried on, and the size which the sac and the foetus it contains may acquire, appear also to possess certain distinctive features in the two forms of pregnancy. In a true tubal conception the period of gestation seldom continues beyond the third or fourth month, except in those rare cases where the ovum is detained in the trumpet-shaped mouth of the tube, when the gestation may go on to the full time.¹ The sac also in which the embryo is contained is generally of a globular form, and seldom attains a greater size than a large orange, when it bursts and occasions the death of the mother from the hæmorrhage consequent on its rupture. Moreover, the enlargement of the Fallopian tube is not general, but confined to the part where the impregnated ovum is lodged, the walls of the tube are not hypertrophied, and the canal remains pervious both on the distal and proximal sides of the dilatation. In a cornual conception, again, the period to which gestation may be carried on varies with the state of development of the horn in which impregnation has taken place. When it is well formed and communicates freely with the canal of the cervix and vagina, then gestation may go on to the full period, and the child may be born in the usual manner. When the impregnated horn, however, is rudimentary in its development, more especially if the canal of communication between it and the cervix and vagina is partially or wholly obstructed, then the expansion of the horn necessary to complete the full time of pregnancy may be interfered with, and rupture at the third or fourth month may occur. This mode of termination of a cornual foetation is well illustrated by the case occurring in Dr Brotherston's practice, which I first detailed. But even with a rudimentary horn and an

¹ A very illustrative case of this kind is related by Mr Louis R. Cooke in the Transactions of the Obstetrical Society of London, vol. v. p. 143.

obstructed passage, as the very remarkable case met with by Dr Scott proves, not only may gestation go on to the full time without rupture, but the fœtus may even be retained for several months within the dilated cornu without occasioning much inconvenience.¹ In a cornual fœtation, also, the impregnated horn is not globular, but rather pyriform or ovoid in shape, corresponding, in fact, more to the form of the unimpregnated cornu; and instead of the enlargement being confined to one spot, the entire horn, sympathizing with the process taking place in its interior, participates in the change, and the placenta, fœtus, and membranes completely fill the cavity.

It will not be without profit if we now turn to the literature of the subject, and glance at some of the cases of cornual fœtation which have been recorded by previous writers; and in my inquiries into this department of my subject, I have been very materially assisted by the valuable treatise of Professor Kussmaul on Malformations of the Uterus.² The cases to which I shall more especially refer are those in which one of the horns of a two-horned uterus has been more or less rudimentary, and in which pregnancy had occurred in this rudimentary or accessory cornu (*nebenhorn*).³ Commencing with a case recorded so far back as the year 1681, Kussmaul relates eleven cases by Dionis, Canestrini, Pfeffinger and Fritze, Tiedemann and Czihak, Joerg and Güntz, Drejer, Ingleby, Heyfelder, Rokitansky, Scanzoni, and Behse, in which undoubtedly pregnancy had occurred in a rudimentary uterine cornu. The nature of the fœtation had been recognised in several of the cases by the original describer; but in those recorded by Pfeffinger and Fritze, Tiedemann and Czihak, Joerg and Güntz, Drejer, Ingleby, Heyfelder, and Behse, the pregnancies had been looked upon as tubal, and their true nature had not been correctly stated until Professor Kussmaul's work appeared.

Since its publication, other cases have been put on record in which fœtation in a rudimentary cornu had been found. In 1860, Professor Virchow⁴ related a case in which a rudimentary left uterine cornu, attached to the cervix by a tolerably thin pedicle, was enlarged to a globular swelling, in which a fœtus was situated. Three years ago, Professor Luschka⁵ described a case, the nature of

¹ The case recorded by Pfeffinger and Fritze (Kussmaul, p. 132) furnishes still more striking evidence of retention of the fœtus developed in an accessory uterine cornu. From the history, it would appear that the child was retained so long as thirty-one years after the impregnation. A distinct canal does not seem to have been recognised in the pedicle connecting the rudimentary with the more fully developed horn in this case of Pfeffinger's.

² Von dem Mangel, der Verkümmern und Verdopplung der Gebärmutter. Würzburg, 1859.

³ In Kussmaul's work these cases are related in the ninth chapter under the head of pregnancies in a one-horned uterus with a stunted accessory horn, but it seems to me that the description employed in the text more exactly expresses their nature.

⁴ Abstract in Canstatt for 1860, p. 420.

⁵ Monatschrift für Geburtskunde, vol. xxii. p. 31; Berlin, 1863.

which he fully recognised, where a right rudimentary horn was pregnant, and where rupture took place about the third month. In the year 1863, Dr Haydon exhibited to the Obstetrical Society of London, as an example of tubal foetation, a preparation which was reported on by Drs Tyler Smith and Braxton Hicks, and described and figured in the 5th volume of the Transactions of the Society as a case of extra-uterine foetation. From the drawing and description, I am of opinion that it is a case of pregnancy in a right uterine cornu. The organ which the reporters call the uterus is evidently only the left uterine cornu, enlarged in sympathy with the pregnant horn on the opposite side, for only one Fallopian tube and one ovary are connected to its upper part. The structure which they describe as the right tube passing off from the uterus at one and a-half inch below the fundus, is evidently the right rudimentary cornu, and corresponds to the part I have termed the pedicle in the description of the cases given in the early part of this paper. Beyond the pedicle is the dilated sac, in which the foetus had been situated before the rupture which occasioned the death of the patient had occurred. This sac corresponds in position to the dilated part of the right horn, and to it the ovary is described as being connected. The reporters consider that the right tube is not only much longer than the left, but displaced in its position; but this apparent discrepancy in the arrangement on the two sides is due to the circumstance, that they have included in their measurements of the right tube not only that structure but the right rudimentary cornu also. A case recorded in the 6th vol. of the Transactions of the same Society by Dr Edwin Day as a tubal foetation, appears also to be an example of pregnancy in a right rudimentary cornu: for the structure described as the Fallopian tube of the right side is stated to have been attached to the base of the uterus, and in it a tumour containing an organized clot and remains of deciduous membrane are said to have been situated. The place of attachment of this tube inclines me to form the conclusion that it was a rudimentary uterine cornu.¹

In the cases which have been recorded by different authors, the exact point of connexion of the rudimentary cornu to the more perfectly developed horn presented some variations. In both of my

¹ In the British Medical Journal, Dec. 17, 1859, a case is reported by Dr Hancox as tubal pregnancy, which seems in some respects to correspond more with a cornual than tubal foetation. The anatomical details are, however, too meagre to enable one to give a positive opinion. The child had reached nearly the full period before rupture and extrusion into the abdomen had occurred. The walls of the cavity in which the child had been situated were thin at the point of rupture; but below this spot they were much thickened, like the substance of the heart. Body of the uterus not enlarged and healthy. The pregnancy is described as in the right Fallopian tube, and the communication between it and the uterus was perfectly occluded. The sac containing the foetus is described as presenting all the appearance of a gravid uterus at the full period of gestation, which circumstance, together with the thickness of its wall, points rather to a cornual than a tubal pregnancy.

cases, and in those of Canestrini, Ingleby, and some others, it was attached to the cervix; but, in the preparation originally described by Tiedemann and Czihak, and re-examined with great care by Kussmaul, it was connected with a considerable portion of the side of the body of the developed cornu, and reached almost to the top of its fundus; whilst, in the case of Heyfelder, the attachment was to the lower part of the body of the developed horn, but above the cervix.

In cases of uterus bicornis unicollis, where both horns are symmetrically developed, their cavities communicate freely with the canal of the cervix, as may be seen in Fig. III. of my first series of cases. But when one of the horns has been retarded in its development, or if it has become more or less atrophied, it then assumes an accessory or rudimental character. As a consequence of this, the size of the cavity at its cervical end may become considerably diminished, and even in some cases, as in those just described, may, I believe, no longer exist. The specimens which have been recorded illustrate the gradation from a well-marked open canal to a perfectly closed condition. In a case recorded by Cruveilhier,¹ a rudimentary right cornu communicated by a narrow but distinct canal with that of the cervix. In Scanzoni's case,² also, a distinct canal connecting the cavities of the two horns with each other extended along the pedicle, and opened high up into the cavity of the fully-developed right uterus. In Rokitsky's case,³ the canal of the pedicle opened above the internal os, with an orifice the size of a millet seed. In the preparation described by Canestrini, one or perhaps two fine canals, large enough to admit a bristle, seemed to traverse the pedicle. In the case originally observed by Heyfelder, and re-examined with much care by Kussmaul (p. 149), a fine canal, commencing just above the internal os of the developed horn, extended for a short distance into the pedicle, but could not be traced into the cavity of the left impregnated cornu; and though Kussmaul inclines to the belief that it is the outlet canal of the left horn, he is unable to say so with certainty. In the cases described by Dionis, Tiedemann and Czihak, Ingleby, and Luschka, as well as in those now recorded by myself, no canal could be seen in the pedicle; and it is expressly stated by Luschka, with whose specimen my dissections in this respect coincide, that no opening was found at the extremities of the pedicle, either in the rudimentary or fully developed cornu.

The questions may very naturally now be asked, Had this impervious condition of the pedicle existed before impregnation? or, Had a canal existed in the unimpregnated state which, during the process of gestation, had become obliterated? On the way in which they are answered will depend our conception of the mode

¹ Anat. Path., vol. i. liv. 4, Pl. 5, Fig. 2.

² Beiträge z. Geburtskunde, bd. i. h. i.

³ Lehrbuch der Pathologischen Anatomie, 3d Ed. p. 450; Ed. 1861.

in which the fertilization of the ovum had been effected in these cases. The only authors, so far as I am aware, who enter into the consideration of these questions are Ingleby and Kussmaul. Ingleby¹ considers that the time of occurrence of the impervious state of the canal is quite a matter of speculation, "if a congenital malformation, seminal contact is out of the question, and we raise the almost exploded doctrine of absorption." Kussmaul (p. 164) thinks "it is improbable that such a canal had not existed before the conception, and it is equally improbable that the connecting band would have been originally solid. It is more probable that it had become closed up in consequence of the pregnancy, the closure occurring partly through pressure of the widened blood-vessels, and partly through a decidua-like growth of the mucous membrane of the connecting canal, as was observed in the case of Rokitansky."

I am by no means disposed to accept Kussmaul's argument on this matter as final and conclusive, for there does not seem to me to be sufficient evidence to show that, during the performance of any physiological process in which enlargement of the bloodvessels occurs, the pressure which they may exercise on surrounding parts is sufficient to occasion closure of such a canal as may have been supposed on Kussmaul's theory to have traversed the pedicle. In cases of true tubal conceptions, for example, where considerable enlargement of the vessels occurs, the closure of the tube does not necessarily occur beyond the immediate seat of the sac containing the embryo. If external pressure is to be regarded as a cause capable of producing closure of the canal, supposing one to have been present, then I should think the hypertrophy of the muscular fibres of the pedicle taking place during gestation would be a much more efficient agent in the process than the pressure of surrounding vessels, as the canal would, in all probability, have been more immediately invested by them than by the vessels. But here, again, there is a want of evidence that such an effect would have been produced, for hypertrophy of the walls of a gland-duct does not necessarily occasion obstruction of its canal. Neither does Kussmaul's theory of the blocking up of the canal by a decidua-like membrane, so that it could no longer be recognisable, seem to me to be tenable. For the question is not one of simple closure of orifices, such as so often occurs during pregnancy by the formation of the decidua at the uterine ends of the Fallopian tubes, but of the complete obliteration, so that no trace could be discovered after a most careful examination, of a canal between one and two inches in length. The case of Rokitansky, which Kussmaul advances in support of his theory, does not, indeed, bear it out; for though it is stated that the canal was lined by a decidua, yet Rokitansky makes no mention of its being obliterated, and he distinctly describes its orifice as situated above the internal os. Again,

¹ Edinburgh Medical and Surgical Journal, vol. xlii. p. 350, 1834.

there is positive testimony, as in the case by Scanzoni, that the formation of deciduous lining membrane within the canal in the pedicle of a rudimentary uterine cornu is by no means a necessary occurrence during impregnation.

From a consideration, then, of these various circumstances, and from the fact that in the cases which have been recorded, a regular gradation has been traced from a distinctly recognisable canal in the pedicle to that condition in which none could be detected, I am disposed to think that the pedicle must have been solid before impregnation was effected.¹ If this supposition be correct, the mode in which the ovum became fertilized may form a topic for further inquiry. It is obvious it could not have been by the direct channel, for closure of the canal of the pedicle necessarily cuts off all direct communication between the vagina and cervix, and the cavity of the rudimentary cornu. Neither would a revival of the old Harveian doctrine of absorption, as Ingleby suggests, help us out of the difficulty; for modern physiological research has clearly shown that it is necessary for impregnation, not only that the seminal fluid should come in contact with the ovum, but that the spermatozoa should penetrate into its interior. There remains, then, but one course which could have been taken by the seminal fluid in these cases, viz., along the cornu and Fallopian tube of the more perfectly developed side into the Fallopian tube of the rudimentary cornu, and then into the cavity of the latter.

But though such a theory may at first sight appear not a little strange, yet there are certain circumstances which give countenance and support to the possibility of a roundabout passage of the seminal fluid in these very exceptional cases. That the spermatozoa possess a remarkable power of spontaneous movement, and that during or after coitus they pass into the cavity of the uterus, there is, I suppose, no difference of opinion. That spermatozoa may also pass into the Fallopian tube there can be little question, as the numerous cases of true tubal conception which have now been put on record sufficiently testify. And without entering, on this occasion, into the much disputed subject of ovarian gestation, there is, I think, sufficient evidence before us to show that these bodies may pass through the fimbriated mouth of the Fallopian tube into the peritoneal cavity. For, in addition to the support which is given to this opinion by cases of abdominal gestation, Martin Barry and Bischoff have furnished positive testimony of the occasional occurrence of spermatozoa on the surface of the ovary in the rabbit and the bitch when examined shortly after coitus.²

¹ It may be objected to this view that, if the pedicle had always been impermeable, there would have been an accumulation of blood within the rudimentary cornu due to the pouring out of that fluid from its mucous membrane during successive menstrual periods. But we, as yet, possess scarcely any definite information respecting the conditions under which menstruation takes place in these rudimentary uterine horns.

² As additional evidence of the travelling power of the spermatozoa, I may

If the possibility of the passage of the spermatozoa up to, and even through the mouth of the fimbriated end of the Fallopian tube be admitted, we may next inquire if there is any evidence that the mouths of the two tubes can be so approximated as to permit of the transit of the seminal fluid from the orifice of one into that of the other. It seems to me that, in the two cases I have personally examined, and, so far as I can judge from the engravings which have been furnished by previous writers in illustration of their cases, an anatomical arrangement exists in connexion with the rudimentary cornu, which permits of such an approximation as I have referred to; for the pedicle which attaches it to the cervix or body of the more perfectly developed horn is so slender as would, I believe, permit to a considerable extent of the movement of the rudimentary horn towards the cornu on the opposite side. But quite independently of the special provision which seems to exist in these cases, there are various recorded facts which prove that in a well-formed uterus even, and still more so when conjoined, as in these cases, with a mobile pedicle, an approximation of the mouths of the two Fallopian tubes to each other is not impracticable. In the second of the two well-known cases of abdominal pregnancy recorded by Dr Oldham,¹ there is reason to believe that the left Fallopian tube must have received its ovum from the right ovary, for the corpus luteum was situated in the latter, whilst the fimbriated end of its corresponding tube was closed in by adhesions; the left tube, on the other hand, was open at its fimbriated extremity. In a case examined by Dr G. C. Watson,² the fimbriated mouth of the left Fallopian tube grasped a placenta-like mass, to which a three-months' foetus was connected by a funis. There was no corpus luteum in the left ovary, but one was apparently contained in the right ovary. In this case, it would seem as if the left tube had attempted to seize the ovum from the right ovary, but had not completely succeeded in the attempt. Dr Arthur Farre³ states that a preparation (No. 722) exists in the Cambridge University Anatomical Museum in which both the tubes grasp the same ovary, to which their extremities are fixed by morbid adhesions. Rokitansky⁴ also describes a case of pregnancy in which a corpus luteum was situated in the left ovary; but the corresponding tube was so constricted, twisted, and invested by adhesions, that

adduce the observations of Dr A. Clark (London Hospital Reports, vol. i. p. 223, 1864) on the occurrence of these bodies in the sediment of the urine taken directly from the bladders of dead men. If during or shortly after connexion, such a patent condition of the Fallopian tubes as Dr J. Matthews Duncan states (Edinburgh Medical Journal, November 1865) to be a not very rare occurrence, had existed, the passage of the spermatozoa upwards to the peritoneal cavity would necessarily have been materially facilitated.

¹ Guy's Hospital Reports, vol. iii. p. 272, 1845.

² Clay's British Obstetric Record, vol. i. p. 70, 1848.

³ Article "Uterus," in Todd's Cyclopædia, vol. v. p. 622.

⁴ Allg. Wiener Mediz. Zeitung, No. 20, p. 155, 1860.

its canal was impermeable. Now, if this closure of the left tube had occurred before impregnation, the passage of the ovum must have been from the left ovary through the right tube into the uterus.

The numerous cases which have now been recorded by obstetric writers of tubal and other forms of pregnancy, in which the corpus luteum was situated in the ovary of the side opposite to that on which the embryo was lying, show that a migration or wandering of the ovum from one to the opposite side is not impracticable.¹ But those who have inquired into this subject by no means agree as to the channel along which this migration is conducted. Some are of opinion that the ovum is seized by the Fallopian tube corresponding to its own ovary, along which it is conveyed into the uterus or cornu of that side, and then within the uterus across into the cornu or Fallopian tube of the opposite. This migration has been called intra-uterine. Others again hold that the wandering is due to the Fallopian tube grasping, or lying with its mouth close to, the ovary of the opposite side; so that the ovum enters it directly, then descends to a greater or less distance along it. This migration has been called extra-uterine. Of these two theories I am disposed to regard the latter as the more probable.

In discussing this subject, it must be kept in view that the ovum of itself possesses no power of independent movement. Any change of place which it may undergo is due to the action of external forces. Its transit along the Fallopian tube and uterus is caused partly by the action of the ciliated epithelium covering the free surfaces of their mucous membranes, and partly by the contraction of their muscular walls. If, after having entered the uterus, internal migration of the ovum into the cornu or Fallopian

¹ There is no need why I should, in support of the statement made in the text, quote the cases in illustration which have been collected with so much industry by Kussmaul, as they may be consulted in his work, already so frequently referred to. It may be of service, however, if I give the references to other cases recorded since the publication, in 1859, of Professor Kussmaul's treatise. Dr A. Farre (op. cit.) states that he has noticed this circumstance in repeated instances. Luschka, in the case already quoted, of pregnancy in a right rudimentary cornu, met with the corpus luteum in the ovary of the opposite side. Schultze (*Monatschrift für Geburtskunde*, vol. xxiii. p. 222, 1864) records a case of tubo-uterine foetation on the right side, with a corpus luteum in the left ovary. Dr M. T. Sadler (*Medical Times and Gazette*, 5th August 1865) relates a case of left tubal foetation, with a corpus luteum in the right ovary. Von Wiedersperg (*Prager Vierteljahr*, Part iv. 1865, p. 23) describes a case of pregnancy in a right Fallopian tube, with the corpus luteum in the left ovary. Kussmaul himself (*Abstract in Brit. and For. Med.-Chir. Rev.*, Jan. 1863) has met with a case in addition to those he formerly described, in which a corpus luteum in the left ovary was conjoined with a right tubal pregnancy. This case seems to have materially modified Professor Kussmaul's opinion respecting the mode of passage of the ovum in these cases. In discussing the subject in his treatise, he supports the intra-uterine theory of transmigration, whereas, in his comments on this case, he advocates an extra-uterine mode of passage of the ovum.

tube of the opposite side were to take place, not only would the cilia clothing the epithelium of that side have to work in a reverse direction to their accustomed mode of action, but the contraction of the muscular wall of the tube or cornu would have to be reversed also, so as to send the ovum up the tube or horn, which, to say the least, is not very probable. Then the smallness of the uterine orifice of the tube would hinder the passage of the ovum from the uterus into it, unless some unusual patent state of that opening existed in such cases.

On the other hand, in support of the extra-uterine theory of migration, not only is there the positive testimony furnished by the preparation referred to by Dr Farre, that the Fallopian tube can grasp the opposite ovary, but the specimens examined by Dr Oldham and Professor Rokitsansky show the possibility of an ovum passing into the uterus, or the opposite tuba, although the canal of the tuba belonging to the ovary from out of which the ovum proceeded was blocked up by adhesions. Hence, I am disposed to conclude that this latter theory presents fewer difficulties than the former.¹

Now, if we grant the possibility of an extra-uterine passage of the ovum from an ovary to a tuba or cornu of the opposite side, there is yet stronger presumptive evidence in favour of an extra-uterine or intra-peritoneal passage of the spermatozoa in such very exceptional cases as those especially discussed in this memoir. For not only does there seem to be a special provision in the mobile pedicle, to permit the approximation to each other of the mouths of the two Fallopian tubes, but the power of movement possessed by the spermatozoa themselves would facilitate their transit from one to the other, should an exact coaptation of their peritoneal orifices not have taken place.



ARTICLE III.—*Notes on the Prison Dietaries in Scotland.* By J. B. THOMSON, L.R.C.S.E., Resident Surgeon to the General Prison for Scotland. PART I.

THE present appears to be a very fit time for inquiry into the dietaries of our prisons. During the last ten years there have been

¹ As bearing on the extra-uterine theory of the migration of the ova, not only in man but in other mammalia, I may just allude to the fact, that in some of the lower vertebrata, the amphibia more especially, the mouth of the Fallopian tube lies at some distance from the ovary, and the ova have to traverse a considerable distance before entering it. For an account of the mechanism by which the transit of the ova is effected, see the section on Generation in Dr Baly's translation of Johannes Müller's Physiology, and the paper by Mr Newport in the Philosophical Transactions for 1851.

very material changes in our prison discipline, called for chiefly by the abolition of transportation. Our convicts have been detained in our prisons under long sentences of penal servitude; the question pending whether such long sentences can be carried out with safety to their physical and mental condition. These changes in prison discipline have now been so long in operation that the results can be satisfactorily determined.

Accordingly, in England we have had a "Correspondence between the Secretary of State for the Home Department and the Inspectors of Prisons relating to Dietaries in England, and the Report of a Committee appointed by the Secretary of State to inquire into the Dietaries of County and Borough Prisons; also Reports of a Committee appointed to inquire into the Dietaries of Convict Prisons," dated respectively 20th May 1864 and 8th July 1864. The time seems equally opportune for reviewing the prison dietaries in Scotland; and the Managers appointed under "The Prisons (Scotland) Administration Act" were instructed by the Secretary of State for the Home Department to procure the opinion of Professor Christison and the writer of these notes on the best dietary table for the several classes of prisoners in Scotland, taking into consideration whatever instructions we might derive from the recent proceedings in England. A joint Report has been forwarded to Sir George Grey, and copies thereof laid on the table of the House of Commons; but we believe that all who are interested in the subject, consider that the information embodied in that report should not be confined to the pages of a blue book, but be made available for the benefit of the public and of the medical profession. With this view I have been induced to forward these notes to the Medical Journal.

My object being to give full details of the history and progress of the prison dietaries in Scotland, I have not hesitated to make use freely of the joint Report by Professor Christison and myself, but even to adopt and intersperse with these notes considerable passages from that Report.

The dietary of Scottish prisons is a very extensive and important subject of inquiry. It comprehends sixty-seven local prisons and the general prison for Scotland; embracing a population whose daily average annually amounts to from 2000 to 3000 prisoners. The highest number of prisoners in any year for the last quarter of a century was, in 1849, when they culminated to 3143, being coincident with and probably related to the Irish famine, political disturbances, and manufacturing distress. Taking, however, the twenty years ended 1864, I find the average daily criminal population of Scotland to be 2471. It need not then be matter of surprise to learn that the regulation of the dietaries of such masses of men should, from time to time, have called for much anxious care and consideration on the part of the directors and managers of our prisons.

The dietary question in Scotland as to prisoners admits of being

tried by complete and precise information, very different from what exists in England. In the English county and borough gaols great diversity of dietaries prevailed, each gaol having its dietary fixed by the local magistrates, who readily received the sanction of Government for their proposals. The result was a want of uniformity in practice; no fixed standard was enforced, and no scientific principles were followed. It is said that little more than one-half of the English gaols adopted the scale sanctioned by the Government since 1843; but which the committee of the House of Lords, of which Lord Carnarvon was chairman, affirmed to be unsatisfactory and unscientific. The other gaols had dietary scales totally different, but equally unsatisfactory and unscientific. Happily, our Scotch dietaries have been long on a better footing.

The local prisons, as well as the General Prison for Scotland, are under one code of rules with respect to diet and prison discipline. This has been the case since 1842, and the Annual Reports of the late General Board of Directors and of the present Managers of Prisons show that the dietary rules and prison discipline have been uniformly enforced; and their Reports, for upwards of twenty years, embody statistical results applicable to all classes of prisoners, short sentenced prisoners in local prisons, and long sentenced prisoners under penal servitude in the General Prison for Scotland at Perth. These sources of information afford, therefore, valuable material for enabling us to reach sound conclusions in regard to our scale of dietaries.

Scotland has also, we think, had a great advantage in having a national diet to follow, the staple food of the people being oatmeal and milk; which in nutritive value, cheapness, and simple nature, have been found well fitted for Scottish prisoners, as well as for sustaining the hardest working agricultural labourers of this country. These articles will be found, modified by sex, age, confinement, or hard labour, to be the substratum of all our Scottish scales of diet. Previous to the dietaries of which the fundamental scale was sanctioned by the Secretary of State, the dietary table for the different classes of prisoners in Scotland has undergone repeated adjustments, in consequence of certain diseases proved to be the result of defective alimentation; and the dietaries now in use were improved and adjusted, with the especial object of preventing the recurrence of similar accidents. We often learn more from our struggles and failures than from our successes; and what I propose in this paper is by way of preliminary to the revisal of the dietary question, to arrange our information under the following heads, viz. :—

I. Of the history of our prison dietaries in Scotland until the adoption of those in present use.

II. Of the comparative health, sickness, and death-rate under the former and present improved dietaries.

I.—*History of the Dietaries in Scottish Prisons.*

Very soon after entering upon their duties the late General Board of Directors of Prisons in Scotland appointed a committee to arrange a wholesome and sufficient dietary to be adopted in every prison. For this purpose inquiries were made as to dietaries in use in the various prisons, and opinions upon the best dietary were obtained from two eminent physicians, Dr W. P. Alison and Dr A. Combe, whereupon three rates of diet, with relative rules, were brought into operation in all the Scottish prisons.

The following were the original Dietaries introduced in 1842:—

FIRST RATE.			Estimated Equivalent of Wheaten Bread.
BREAKFAST.	DINNER.	SUPPER.	
6 oz. of oatmeal made into porridge, with $\frac{3}{4}$ pint of milk.	1 $\frac{1}{2}$ pint of barley broth, with 6 oz. of wheaten bread; or, 3 lb. of potatoes, with $\frac{3}{4}$ pint of milk.	1 $\frac{1}{2}$ lb. of potatoes; or 4 oz. of oatmeal made into porridge, with $\frac{1}{2}$ pint of milk.	24 oz.
SECOND RATE.			
8 oz. of oatmeal made into porridge, with $\frac{3}{4}$ pint of milk.	2 pints of barley broth, with 8 oz. of wheaten bread; or, 3 lb. of potatoes, with $\frac{3}{4}$ pint of milk, and 4 oz. of wheaten bread.	1 $\frac{1}{2}$ lb. of potatoes; or 4 oz. of oatmeal made into porridge, with $\frac{1}{2}$ pint of milk.	30 oz.
THIRD RATE.			
8 oz. of oatmeal made into porridge, with $\frac{3}{4}$ pint of milk.	2 pints of barley broth, with 12 oz. of wheaten bread; or, 3 lb. of potatoes, with $\frac{3}{4}$ pint of milk, and 8 oz. of wheaten bread.	2 lb. of potatoes; or 6 oz. of oatmeal made into porridge, with $\frac{1}{2}$ pint of milk.	36 oz.

Relative rules were added as to the preparation and contents of the broths, soups, etc., differing very little from those in present use, and which we may give in another part of this paper or series of papers. Very much fault cannot be found with this dietary scale, but the application of the rates was left to the governors and surgeons, under the control of the county boards; whereas definite instructions should have been given to what classes of prisoners

each rate should apply, according to age, sex, length of imprisonment, and light or severe labour. One of the relative rules certainly led to very serious results, viz., Rule 6, allowing the alternative of milk or treacle-water, viz.—“Where milk is scarce, an equal quantity of treacle-water may be substituted, made with $1\frac{1}{2}$ ounce of treacle to 1 pint of water.” This rule showed an entire ignorance of the scientific analysis of milk and its nitrogenous constituents in which treacle is deficient. Unscientific and empirical, however, as certain portions of these rates were, some excellent principles were set forth by the medical advisers of the Board, viz.:—

That the food given to prisoners should be sufficient, and not more than sufficient to maintain health and strength during confinement.

That there should be no intention, either before or after trial, to add to the evils of imprisonment, any injury to health, or increased liability to disease.

That the three rates of diet should be applied to prisoners according to age, sex, sentences, and labour.

That the dietary should be varied.

That the effects of each rate should be tested by the weights of prisoners on admission, during imprisonment, and at liberation.

That there should be no diet-rate amounting to less than 20 oz. of solid food in the day.

That medical officers of prisons should be at liberty to alter the diet in cases where it is necessary to maintain health and strength.

That attention be specially directed to young persons between fourteen and eighteen, who grow rapidly, whom it might be necessary to advance to higher rates from the first.

These rates and relative rules were subjected to careful trials in the different prisons, but by no means with satisfactory results.

Reviewing the surgeons' reports, and the other returns in the blue books, it is quite evident that prisoners suffered much more than the civil population; diarrhœa, phthisis, and scrofula being manifested often and in severe forms. Dr Baly, in his paper on the Mortality of Prisons, published in 1845, says,—“The proportion of scrofulous individuals has been far greater amongst the convicts sent to Millbank from the prisons of Scotland than amongst those who come from the poorest districts of England.” That statement I believe to have been correct at the time; and the prevalence of scrofula and phthisis did attract attention repeatedly. However, standards of comparison were wanting to determine the proportion of suffering in and out of prisons.

In 1846, a general failure in the weights and physical condition of prisoners in certain prisons was so distinct as to call for careful investigation. Dr Malcom, surgeon to the General Prison at Perth, in his annual report for 1846, says,—“Scurvy has visited the General Prison. The diseases at first were British cholera and diarrhœa, but it finally settled down to scurvy. Scurvy is a disease which

has long been known as attacking persons living entirely on salt provisions, without any proper quantity of fresh or vegetable diet. It likewise attacks those almost exclusively fed on vegetable or farinaceous substances; and this I consider to have been the cause here. I have also to mention scrofula as a disease which prevails in this prison, and that in most aggravated forms. In addition, considerable sickness prevailed at the time in the prison. The Board, by an order of date the 7th November 1846, increased the diet of all prisoners who had been in confinement more than five months." These visitations demanded careful scrutiny into the prison discipline and dietaries, which led to the disclosure of grave errors in the dietaries; and here it must be stated as a practical lesson, that the worst sickness in the General Prison, and in the local prisons of Scotland, was clearly traced to an attempt to accommodate our dietaries in one respect to that of the English prisons at the time, viz., by substituting treacle-water for milk. In the Eighth Report of the Directors of Prisons for Scotland, pages 7 and 8, we have statements of much significance. A careful inspection of the General Prison by Mr Hill and a special committee, aided by two eminent medical practitioners, Dr Maclagan and Dr Christison, led to various remedial and preventive measures, and among these to additions to the prison dietary. These dietary changes consisted in extra allowances of meat, milk, etc., the surgeon being instructed to diminish or increase the allowances as he saw fit, until there should no longer be any appearance of scurvy in the prison. These and other active measures restored the health of the prisoners, and towards the end of the year the General Prison was reported to be in a good sanitary condition.

A very valuable paper "On Scurvy" was published by Professor Christison in the *Monthly Journal of Medical Science*, in which this epidemic in the General Prison is treated of at large. Previous to this time, scurvy was little known on land, except in the Millbank penitentiary, and some other English prisons, in which it assumed an epidemic character. There can be little doubt that the Millbank epidemic of 1823 was due to the same causes. The nitrogenous food constituents were low, and not adequate to sustain the health; and there large masses suffered;—out of a population of 858, no less than 448 were attacked.

In Professor Christison's paper, the cause is made out distinctly to be from defective dietary,—and especially the want of milk, treacle-water having been substituted night and morning for milk; prisoners in other prisons having a conjunct population of 1016 in Edinburgh, Glasgow, and elsewhere, showed not a single case of scurvy; while on the visit of Drs Maclagan and Christison to the General Prison, they saw 39 well-marked cases, 11 suspicious, and a few others with obscure symptoms of the same disease. The conclusions come to were that a dietetic error was being committed as the fundamental cause of scurvy. The diet was essentially saccharo-farinaceous.

The *breakfast* of the majority of prisoners on the highest rate consisted of 8 oz. oatmeal, made into porridge, with salt and water, and 15 fluid oz. of treacle-water, containing $1\frac{1}{10}$ oz. of treacle.

Supper, 6 oz. of oatmeal, made into porridge, and 10 oz. of treacle-water, containing $\frac{3}{4}$ of an ounce of treacle.

Dinner, on four days of every week, consisted of 12 oz. of wheaten bread, 2 pints of barley broth, which contained 1 oz. of meat, 4 oz. (decorticated) barley, about $2\frac{1}{2}$ oz. turnips, carrots, cabbage, leeks, and onions, with a due proportion of salt.

On one day of every week each prisoner on the third rate had for dinner, in lieu of barley broth, 2 pints of pea-soup, containing $4\frac{1}{2}$ oz. of peas, 1 oz. of meat, and $1\frac{1}{4}$ oz. of succulent fresh vegetables, duly seasoned with salt and pepper.

On another day of every week the dinner consisted of 12 oz. of bread, 2 pints barley-milk, made with 4 oz. of barley, 10 fluid oz. skimmed milk, and a little salt. And on another day it consisted of the same quantity of bread, and 12 oz. of white fish.

The great defect in this dietary seemed to be the omission of milk, this article being especially valuable when the food is chiefly farinaceous. The restoration of milk arrested and cured the disease; and milk cured the disease without any other antiscorbutic remedy.

The standard third rate of the Scottish prisons, as used in the General Prison in healthy years, is given by Christison,¹ thus, in oz. avoirdupois :—

Total.	NON-NITROGENOUS.			NITROGENOUS.				
	Starch.	Sugar.	Fat.	Gluten.	Legum.	Album.	Casein.	Musc. fib.
25·2	17·8	1·32	0·11	3·96	0·13	0·03	1·36	0·55—6·03

Again, in 1851, the local prisons of Scotland exhibited the prevalence of similar diseases from dietetic defects.² The condition of prisoners in several prisons was not satisfactory; and even where actual disease had not broken out a general falling off in weight, and in health, strength, and general condition, showed that the diet was not adequate, and that diseases from defective dietary were not far off. This was especially noticed in the prisons of Perth, Paisley, and Dundee, and these prisons were ascertained to differ in one essential respect only from other prisons, viz., that (as in the General Prison in 1846) treacle-water had been substituted for milk at breakfast and supper; whereas, in more healthy prisons, milk had been retained.

In the Monthly Journal of Medical Science for May 1852, there is an excellent paper by Professor Christison, containing an "Account of the Experiments and Observations made by himself and Dr Mac-lagan on the Diet of Prisoners." The observations were made on 696 males and 724 females in the prisons of Edinburgh, Glasgow, Aberdeen, Dundee, Stirling, Paisley, Ayr, and the County Prison of Perth.

¹ Vide page 20 of Dr Christison on Scurvy.

² Vide Thirteenth Report of Board of Directors of Prisons in Scotland.

Some very valuable conclusions are come to in this paper, viz. :—

That on scientific grounds the total nutriment given in certain prisons was insufficient from the simple change of treacle-water for milk.

That in Edinburgh, Glasgow, Aberdeen, and Stirling, where milk was retained, the real nutriment consisted daily of 4·05 oz. dry nitrogenous nutriment, 12·87 oz. carboniferous, for sixty days' prisoners.

The Edinburgh diet contained 28 oz. of dry nitrogenous diet weekly; that of Perth and Dundee $9\frac{1}{2}$ oz. less.

That this explained the average loss of weight, per prisoner, in Dundee, to be 5 lbs. under sixty days' confinement.

These defective dietaries, and their experiences in loss of weight, loss of general health, suffering from epidemic scurvy, and other prison diseases, are in our dietary inquiries valuable lessons for future guidance, and prove,—

That some portion of our dietaries up to 1851 was insufficient, and induced failure of health and positive disease among large numbers of prisoners.

That even a slight depreciation in a barely sufficient dietary is productive of serious results, injurious to the sufferer and expensive to the country.

That a general failure in weight among numbers of prisoners is a sure indication of approaching disease of a severe character.

That the general health of prisoners, even under short sentences, of two months or rather less, and with no hard labour, cannot be sustained on less than about 17 ounces daily of well-selected food, of which nearly 4 ounces must contain flesh-forming or nitrogenous principles.

So much for our first head of inquiry, viz., *The History of the Dietaries of Scottish Prisons*, and the general corollary upon the whole is, that unless a margin be allowed beyond a mere subsistence, there is danger to the health of prisoners; and an allowance beyond a mere minimum for support is certainly the safer economy.

II.—*Of the Comparative Health, Sickness, and Death-Rate under the former and present Improved Dietaries.*

Having examined into our experiences under a defective dietary, it occurred to me that these should be compared with the results of existing and improved dietaries, to see how far the comparison confirmed the conclusions arrived at under our first head of inquiry. The Directors having, by extensive reports, ascertained that there existed much disconformity in prisons as to the practical application of the different rates of diet, and looking to a more uniform and safe order of dietary, issued supplementary rules of the following tenor :—

“Convicted prisoners under sentence of imprisonment for periods

exceeding ten days and not exceeding two months, and not employed at hard labour, shall be put upon the first or lowest of the three rates of diet, unless in any special case where the governor and surgeon shall, on due consideration, be of opinion that a higher rate is proper and necessary.

“Particular attention to be paid to the large-bodied men, and those previously accustomed to ample food and active employment in the open air.

“No convicted prisoner sentenced to imprisonment for a period exceeding two months shall, in any case, be put upon the said first or lowest rate of diet.

“The use of treacle-water as a substitute for milk to be disallowed.”

These few but very important changes in the dietary were sanctioned and certified in September 1851, by the Secretary of State, as proper to be observed and enforced. Referring to the rates of dietary found to be defective, we proceed to the examination of the condition of prisoners in Scotland under the improved dietary.

According to present returns from the different prisons of Scotland, the general health of prisoners has been uniformly good, and remarkable for the entire absence of all prison diseases known formerly to have originated in defective nourishment. This has been the case since the introduction of the dietaries now in use. Epidemics are conspicuously absent, even in localities where they prevail often and virulently; they are scarcely known within the prison walls.

The contrast, indeed, was so great betwixt our experiences during the latter ten years of the improved, and the previous ten years of a defective dietary, that it seemed desirable to have a distinct comparison betwixt these two decennial periods. Tables were, therefore, carefully drawn up from the Reports of Prisons in Scotland, showing the total average daily number of prisoners in the several prisons of Scotland, the amount of sickness, and number of deaths, etc., in the two decennial periods, ended 30th June 1853 and 1863 respectively:—

TOTAL AVERAGE DAILY NUMBER OF PRISONERS, for the Ten Years from 1844 to 1853, and from 1854 to 1863, . . . }	1844-1853.		1854-1863.	
	27,239		23,481	
	Number.	Per-centage.	Number.	Per-centage.
Cases of sickness, ...	17,715	65	10,709	45
Deaths, ...	335	1·23	250	1·06
Diseases contracted <i>before</i> admission, .	10,282	38	7,129	30
Do. ” <i>after</i> ”	7,433	27	3,580	15
Prisoners off work from sickness, .	11,529	42	7,204	31
Days such prisoners were off work, .	124,292	...	76,395	...
Days per prisoner, on total prisoners, .	4·6	...	3·3	...

The comparative sickness is shown to be,—

1844 to 1853, inclusive, ...	65 per cent.
1854 to 1863, „	45 „
In favour of latter period, . . .	20 „

Observe that the prison rule in regard to sickness enjoins, “that the surgeon shall enter in his register every case of illness which is sufficient to prevent a prisoner from working, or which is infectious.”

The comparative death-rate shown in the above abstract gives,—

Deaths, 1844 to 1853, inclusive, ...	1·23 per cent.
„ 1854 to 1863, „	1·06 „
In favour of latter period, . . .	0·17 „

All this is the more remarkable when we keep in mind that this reduction of sickness and death-rate has been coincident with the abolition of transportation, and during the years when the worst classes of criminals have been thrown back upon the country to undergo penal servitude, and who of all others of the criminal population contribute most to the registers of sickness and death. In the foregoing comparison we have included the General Prison for Scotland, where, since 1855, nearly all the convict females of Scotland have been confined; and if we exclude them, we find the per-centage of deaths in local prisons during the latter ten years only 0·86 per cent.

But it is necessary to take into calculation prisoners liberated on medical grounds; and one-third of them being added as probable deaths, we arrive at the following death-rate throughout all our Scottish prisons, including the General Prison:—

Death-rate, including one-third of those liberated on medical grounds,—

1844 to 1853, inclusive, ...	1·41 per cent.
1854 to 1863, „	1·15 „
Reduction of death-rate in latter period, . . .	0·26 „

The registers of sickness take note of what diseases appear *before* and *after* admission, in order to bring out what effect imprisonment may have in producing diseases in prisons. The two decennial periods stand thus:—

During 1844 to 1853,—

Contracted <i>before</i> admission, ...	38 per cent.
„ <i>after</i> „	27 „

During 1854 to 1863,—

Contracted <i>before</i> admission, ...	30 per cent.
„ <i>after</i> „	15 „

The number of cases of disease contracted *before* admission being

not widely different in both periods ; but the number which took place *after* being nearly double in the former of what occurred in the latter period.

Pursuing this comparison a little further, we examined into the comparative loss of prisoners' work from sickness, and the number of days lost by being off work ; and, as might be anticipated from the preceding details, the balance shows greatly to the advantage of the latter decennial period, viz. :—

1844 to 1853, off work by sickness, . . .	42 per cent.
1854 to 1863, " " " . . .	31 "
In favour of latter period, . . .	11 "

Number of days off work by sickness,—

1844 to 1853, per prisoner, on total prisoners, about	4½ days
1853 to 1863, " " " about	3¼ "

Under this second head of preliminary inquiry we find,—

That the general health has been uniformly good under the dietaries in present use.

That the diseases formerly arising from defective nutriment have not appeared during the latter ten years.

That the amount of sickness has been reduced from 65 to 45 per cent. under the improved dietaries.

That the death-rate (including one-third of those liberated on medical grounds), notwithstanding the substitution of long imprisonment for transportation, has fallen from 1·41 to 1·15 per cent.

That the number of diseases contracted *after* admission to prisons has greatly decreased, viz., from 27 to 15 per cent.

That the number of prisoners off work from sickness has decreased from 4½ to 3¼ days on the total average daily population since the improved dietaries were in operation.

We are quite willing to give due consideration to various salutary changes in prison hygiene, which have emerged during the last ten years ; nevertheless, we must ascribe the improved condition of our prisoners chiefly to the existing prison dietaries.

It is intended to continue these notes under the heads,—

Of the recent returns of governors and surgeons of prisons in Scotland to queries issued to them in regard to dietary.

Of the comparative values in nutrition and cost of the English and Scottish prison dietaries.

Of the especial value of our national dietary of oatmeal and milk. And,

Of the conclusions come to as to any dietary changes.

ARTICLE IV.—*On Rupture of the Heart.* By ANDREW DUNLOP, M.D.

THE following case of rupture of the heart occurred in the practice of Mr Armstrong of South Shields:—

M. W., a widow, æt. 65, had enjoyed good health, with the exception of being troubled with an old ventral hernia, until the end of June 1865, when she began to complain of dyspnoea on exertion. This continued without any additional symptoms until the night of the 10th of July, when, after having been some time in bed, she was seized with oppression of breathing, tightness across the chest, and severe pain in the precordial region. This suffering lasted till morning, when the pain and oppression left her. She was seen on the 11th, when there were no marked symptoms of any kind observable. Pulse 90, soft and regular; heart sounds normal. Shortly after retiring to bed on the evening of the 11th, the symptoms of the previous night returned rather more severely, and they again disappeared in the morning. On the night of the 12th, another and still more severe attack came on; she suffered greatly until 5 A.M., when, after a severe paroxysm of pain, she exclaimed that something had given way at her heart, and was rushing upwards to her head; then, after one or two gasping respirations, she expired.

The body was examined twenty-eight hours after death. Appearance little altered; body well nourished, with a considerable amount of subcutaneous fat. There was a hernial tumour the size of an adult head protruding below the umbilicus. On raising the sternum, the anterior mediastinum was found to be occupied by a considerable quantity of loose adipose tissue. The pericardium was distended with fluid, on the surface of which a mass of air-bubbles was observed floating. On opening the sac, about a pint of dark-coloured fluid escaped, leaving exposed a smooth and apparently recent clot of dark colour and homogeneous consistence. It was about three quarters of an inch thick, and covered nearly the entire surface of the heart. The heart itself was rather small, light coloured, and friable. On the anterior surface of the left ventricle, close to the septum, and nearly midway between the apex and base, were observed two openings about two lines distant from each other. One was large enough to admit a crowquill; the other could only give passage to a probe. The larger opening passed obliquely through the visceral layer of the pericardium and the wall of the heart into the cavity of the left ventricle; while the smaller opening joined the other immediately beneath the pericardium, which was separated from the heart for a distance of several lines around the openings. The parietes of the heart were thinned throughout, the valves were healthy; the lungs were normal; the liver was slightly enlarged, lighter in colour than natural, and very friable; the hernia was found to be incarcerated by adhesion to its sac. The muscular fibres

of the heart were found in a state of moderately advanced fatty degeneration; in most of them the transverse striae were perceptible; but where the change was most advanced they could not be recognised.

The chief points of interest in this case are, 1st, The entire absence of symptoms of cardiac disease until about a fortnight before the patient's death; 2d, The slight nature of the symptoms till three days before death, and the fact that the severe paroxysms occurred only during the night; and, 3d, The suddenness of her death.

The most interesting facts observed at the post-mortem examination were, 1st, The position of the rupture on the anterior surface of the left ventricle; and, 2d, The condition of the heart, that of fatty degeneration.

In speaking of the usual seat of the lesion, Bouillaud says that there are six ruptures of the left side of the heart to four of the right. Rokitansky states that the laceration is most commonly found in the left ventricle; that it is almost invariably in its convex or anterior wall, and generally near its middle, and close to the septum. Mr Bayle, in the *Lancet* for September 1824, states that out of 19 cases which he had collected, the left ventricle was ruptured in 14, and generally on its anterior surface; the right ventricle was ruptured in three instances; in one case perforation took place at the apex, and in another the septum ventriculorum was ruptured. Laennec was of opinion that rupture occurs most frequently in the wall of the left ventricle, and that it rarely takes place at the apex. Out of 52 cases collected by Gluge, the left ventricle was the seat of the lesion in 37, the right ventricle in 8, the left auricle in 3, the right auricle in 2. Out of 9 cases, mentioned by Dr Aitken in his *Practice of Physic*, rupture occurred in the left ventricle in 5; in three of these the laceration was across or along the anterior wall, and in the other two at the apex or in the posterior wall.

Rokitansky holds that fatty degeneration is by far the most common morbid condition of the heart predisposing to rupture, and this opinion is endorsed by all modern observers. He is also of opinion that it may be due to points of inflammatory softening, and that contraction of the aortic orifice may be included among the remote causes. Böttiger (Sydenham Society's Year Book for 1864) states that out of 62 cases of rupture of the heart, fatty degeneration of the organ was found in 32.

Interesting cases of spontaneous rupture of the heart are given by Harvey, Haller, Portal, Cruveilhier, Rostan, Bland, and Andral; and many others are scattered throughout the annals of medicine. Dr Olmi, of Florence, published in 1803 the case of a man, æt. 38, of an enfeebled constitution, who was suddenly seized, after a debauch, with severe pain along the curve of the sternum. His anguish soon became extreme, and a cold sweat broke out over him; his pulse was small and irregular, but his respiration was unem-

barrassed. These symptoms continued till he died the following day. On making a post-mortem examination the pericardium was found to be distended with fluid, and on opening the membrane a quantity of black grumous blood flowed out. On the posterior surface of the left ventricle, at about the distance of half-an-inch from the septum, was found a rupture of nearly an inch in extent. The heart is stated to have been otherwise healthy.

In the *Journal der Practischen Heilkunde* for December 1817, Dr Fischer gives the case of a gentleman, 68 years of age, who had spent the greater part of his life at court, and, with the exception of suffering from gout, had enjoyed good health until the 16th October 1817, when he was suddenly seized with violent pain, which he supposed to be cramp of the stomach; this attack lasted an hour. Next day he had another seizure of the same kind. On the 18th he was free from pain; but on the 19th, on coming home from church, he experienced a rigor, with spasmodic contraction of the extremities, and great pain in the region of the stomach. The abdomen was tender on pressure, but free from tumefaction or hardness; the extremities were cold, and the pulse was regular, except at times, when it was for two or three beats somewhat contracted. In the afternoon he was rather better, and he passed a quiet night. On the morning of the 20th he was seized with a violent paroxysm of pain, and in an hour or two considerable vomiting of bile and mucus took place. A short time afterwards another and more severe paroxysm occurred, the pain now extending all over the body, and in addition there was numbness of the left arm down to the tips of the fingers. This attack soon abated a little, but after about an hour's relief he suddenly sprang up in bed, seized his attendant by the throat, crying out to him to hold his head. On this being done he lay quiet for about half-an-hour, and at the end of that time he turned himself in bed and died without a struggle. A post-mortem examination was made eighteen hours after death, when on opening the pericardium a quantity of reddish serum escaped, followed by florid blood to the amount of two or three pounds. The heart was surrounded by a coagulum weighing more than three pounds, and, on this being cleared away, a rupture was found on the anterior surface of the left ventricle extending from the apex about one and a half inch upwards. Its borders were fringed, and in accurate contact, except at the middle, where for about half-an-inch they were separated by an intervening coagulum. On opening the ventricle the internal wound was found to be about half-an-inch in length, and its lips were as wide again asunder as those of the external breach. The heart was somewhat whiter than natural; it was surrounded by fat at the base, and, like the other muscles of the body, softer in texture than it ought to have been.

It is curious that in this case the symptoms were nearly all referred to the stomach. The rupture was apparently due to fatty degeneration.

A case is given in the *Journal Universel des Sciences Médicales* for April 1819. The patient was a female twenty-two years of age, long addicted to intemperate habits, who had for some time complained of slight fleeting pains all over her body. A day or two before she died she was deserted by her lover, which distressed her a good deal, but on the night immediately preceding her death she supped heartily and retired to bed, where she was found dead next morning. At the post-mortem examination the pericardium was found distended with about ten fluid ounces of coagulated blood, and two ounces of serum. There was a rupture about half-an-inch in diameter in the wall of the left ventricle. The parietes of the ventricle around the perforation were much thicker than in the normal state, and on close examination a very sensible fluctuation was distinguished to the extent of an inch on one side of it. On pressure being made upon this point, flocculi of a cheese-like matter were discharged into the perforation. The pericardium presented traces of inflammation in the part opposite the rupture, and a little nearer the base it was adherent to the heart. That portion of the aorta which is invested by the pericardium also exhibited traces of inflammation.

A case almost exactly similar to the above is given by Dr Mott, in the *Transactions of the Physico-Medical Society of New York* for 1817. The patient, a young woman, was found dead in bed; she had been quite well the preceding evening. The pericardium was distended with coagulated blood, and the rupture, which was situated in the upper part of the left ventricle, was found to communicate with the cavity of an abscess, the size of a pigeon's egg, embedded in the wall of the ventricle, and which opened internally as well as externally. In these two cases, the probability is that the abscess first burst externally, and that its thin internal wall, left unsupported by the evacuation of the pus, was unable to resist the pressure of the blood driven against it by the action of the heart. A case of abscess in the heart, not leading to rupture, is given in the *Revue Médicale* for 1832. The patient was a young soldier, who died of confluent smallpox on the fifty-fifth day of his illness. On making a post-mortem examination, an abscess, the size of a hazel-nut, was found in the muscular substance of the heart, at the base of the left ventricle. It was enclosed in a cyst, and did not communicate externally or internally.

Rostan, in the *Nouveau Journal de Médecine* for April 1820, gives four cases of rupture of the heart. The first is that of a female, æt. 70, who was suddenly seized with cough, dyspnœa, and epigastric pain. Next day the symptoms increased in severity, and she died in the evening. At the post-mortem examination, two days after death, the pericardium was found distended, with a large quantity of coagulated blood, and two irregular fissures were observed in the anterior wall of the left ventricle. They were situated near the apex, and were about half-an-inch distant from each other. One

was an inch in length, and the other three or four lines. The parietes were two lines in thickness at the apex, but more than an inch thick at the base. The aortic orifice was obstructed by numerous rugged ossifications. The structure of the heart is said to have been otherwise normal. The second case is that of a woman, 75 years of age, who died suddenly. On examining the body, an irregular rupture was found at the apex of the left ventricle. The other alterations found in the heart were identical with those of the preceding case. The third case he gives is that of a woman aged 78, who had enjoyed good health up till within five months of her death, when she sustained a violent fall, and from that time her health declined. On being admitted into hospital on the 11th March 1820, she complained of slight catarrh, severe lumbar pain, and constipation. Next day she was rather better, but she died on the morning of the 13th. At the post-mortem examination on the 14th, the pericardium was found filled with coagulated blood, and there were two irregular fissures on the anterior surface of the left ventricle; one seven or eight lines in length, communicating with its cavity, the other longer, but quite superficial. The parietes of the heart were hypertrophied. The current of blood appeared to have been obstructed by ossification of the aorta. The fourth case is a remarkable one. It is that of a widow aged 71, who had for fifteen years suffered from severe pain in the left thoracic region and epigastrium, extending into the dorsal region, and recurring at intervals. Reclining induced a feeling of suffocation, and she had frequent attacks of palpitation, followed by syncope. She entered the hospital on the 7th of February 1820, and died on the 24th. On examining the body, the surface of the pericardium was found to be irregular, and this appearance was caused by its being adherent to the anterior surface of the heart, by means of several albuminous layers of different density. The posterior part of the sac contained a quantity of coagulated blood. There was an evidently recent rupture, an inch and a-half long, in the wall of the heart; and on its left side, to the extent of five or six lines in every direction, the muscular substance was replaced by a fibrous concretion precisely resembling that observed in an aneurism. The ventricular wall was thinned at this point, but thickened everywhere else.

It is very probable that, in the first three of these cases, the heart had undergone a certain amount of fatty degeneration,—an amount too small to be appreciated by the deficient means of observation of that time, but sufficient to render the ventricular wall unable to resist the pressure of the blood, increased, as it was, by the obstruction to the current. In the third case, it is improbable that the decline of the patient's health, subsequent to the fall she sustained, had any connexion with the ultimate cause of her death; it was merely the constitutional disturbance that such an accident was likely to produce in a person at such an advanced period of life. The fourth case is a very curious one. Rostan himself seems to have believed that the

fibrinous alteration of the heart-substance had been caused by rupture taking place at the commencement of the patient's illness, fifteen years before her death, and that this perforation had been plugged up by a coagulum, of which the fibrinous concretion was the altered remains. But is it not rather more likely to have been the result of myocarditis without any subsequent rupture, but accompanied by the pericarditis which caused the adhesion? And it is probable that the rupture, which was the cause of death, was consequent upon a second attack of partial carditis, to which also might be due the exacerbation of her symptoms which induced her to enter the hospital.

Dr Ashburner, in the *Medical and Physical Journal* for December 1822, relates the case of a gentleman far advanced in life who had had an attack of paralysis three years previous to his last illness, but who had otherwise enjoyed good health. He complained one morning of having passed a bad night, and suffered from slight pain above the clavicle. He was rather better next day, but on the evening of the third day of his illness, he was seized with a fainting fit in the water-closet, where his servant found him breathing stertorously, his face livid, and his pulse almost imperceptible. He rallied a little, and was removed; but about an hour afterwards, while attempting to swallow some liquid, he fell back dead. At the post-mortem examination, forty-three hours after death, it was found that the arachnoid and pia mater were in a state of chronic inflammation, and separated from each other by a quantity of yellow-purulent fluid. The brain substance was soft and pulpy, the blood-vessels empty, and the arteries considerably ossified. There was a cyst about the size of a pea in the pons varolii. The pericardium contained twelve fluid ounces of coagulated blood, and on the anterior surface of the left ventricle there were two or three small openings, each admitting a probe into the cavity of the ventricle, which was not quite empty. On the posterior surface of the right ventricle there was a laceration large enough to allow the fore-finger to be passed into the cavity, which was quite empty. The parietes of the heart were thin, the wall of the right ventricle being not quite one-third of its usual thickness. The muscular substance was so friable that it tore like wetted paper. The aorta was enlarged, and contained several spots of ossification.

Another interesting case of multiple rupture was related by Andral, junior, to the Royal Academy of Medicine of Paris, in April 1824. The patient, who had been long subject to painful digestion, died suddenly, after some mental agitation. On examination, a large quantity of coagulated blood was found filling the pericardium. The posterior wall of the left ventricle exhibited five oblong perforations, whose greatest diameter was in the long axis of the heart. The substance of the heart is said to have been healthy. The stomach presented traces of chronic inflammation, and near the middle of its posterior wall there was a large circular perforation with soft and even edges. It is remarkable that the peritoneum was not the seat of any effu-

sion. Multiple rupture of the heart is rare. Ollivier states that it occurred in only 8 of the 48 cases which he had collected. Another case given by Dezeimeris will be found further on, where there were five lacerations, only two of which, however, penetrated into the cavity of the heart. Portal has given one case, and Morgagni another, where there were three openings in the left ventricle. In Andral's case, as in those given by Rostan, the texture of the heart is said to have been unaltered; but it is easy to imagine that a considerable amount of fatty degeneration may have passed unrecognised at a time when pathological histology had little or no existence.

Dr Abercrombie gives two cases. One is that of a man, aged 35, who died suddenly; and on making a post-mortem examination, the heart, which is stated to have been otherwise healthy, was found ruptured. The other is that of a man, 77 years of age, who, while at work, was suddenly seized with pain in the chest, accompanied by a sense of great weakness, and a feeble pulse. He lived ten days in this state, and after death it was found that the left ventricle had been ruptured.

Mr Bayle, in September 1824, related in the *Lancet* the case of a female, aged 68, who, after suffering for some time from paroxysms of palpitation and general uneasiness occurring during the night, was attacked with slight dyspnoea and cough, unaccompanied by expectoration. The respiratory and cardiac sounds were normal. On the evening of the ninth day of this illness she died suddenly. On making a post-mortem examination six days after death, and four days after interment, it was found that the pericardium contained two clots of blood, each weighing about three ounces. The heart was large. In the anterior wall of the left ventricle there was a rupture of a somewhat round shape, and about a quarter of an inch in diameter. Its edges were ragged and torn, and the internal opening was lined by a brown fibrinous concretion, and intersected by the columnæ carneæ. The parietes of the heart in the neighbourhood of the perforation were considerably softened. Morand recorded, in 1732, the case of the Duchess of Brunswick, who died at stool. On examining the body, there was found an ulcer perforating the wall of the right ventricle. It had evidently commenced externally, and gradually penetrated into the cavity. Morgagni records two cases; one that of a woman who had been subject to palpitation, and who died suddenly. The heart was found perforated at the apex by a small ulcer. The other patient was a gentleman, 65 years of age, who had suffered for some time from pains of a rheumatic character, and who was seized one night with pain under the sternum and extending down the arms, accompanied by some confusion of mind. During the following day he was restless and anxious, and complained of a sensation of fumes rising to his head, along with a feeling of tightness across the chest. After these symptoms had continued for an hour or two, he sud-

denly expired. On examination after death, a fissure half an-inch long was found in the wall of the left ventricle.

Bertin gives a case of rupture of the aorta at its origin from the left ventricle, evidently the result of ulceration. The pericardium contained four or five pints of serum, and there was a clot several inches thick, and weighing eighteen ounces and a-half, completely enveloping the heart. For four or five months previous to her decease, the patient had been subject to dyspnœa, and on the morning of the day on which she died, after being excited by the visit of a relative whom she had not seen for forty-two years, she fell into a fainting fit, from which she recovered slightly, and was carried to bed. A medical man was sent for, who found her lying on her side, her face pale, and bathed in perspiration. She breathed with difficulty, and the beating of her heart was scarcely perceptible. An anodyne draught was prescribed, and she was sent to the hospital; but as she was being carried into that institution, an hour after the first access of the attack, she was seized with sudden oppression of breathing, and died in a few minutes. It is difficult to say whether, in this case, the rupture took place when the first paroxysm occurred, an hour before death, or whether it was coincident with the fatal attack on the hospital stairs. From the quantity of serum found in the pericardium, and the large coagulum surrounding the heart, the evidence seems to be rather in favour of the first supposition.

Dr Garteloup, a physician in Bordeaux, recorded, in 1821, a case of rupture of the right auricle. The heart was large and fatty. The patient had been suddenly seized with shivering, nausea, and vomiting, on lying down in bed, and a few minutes afterwards he died.

A case is given in the *Lancette Française*, of a woman, aged 57, who entered the Hôtel Dieu in Paris, on the 8th March 1831, complaining of dyspeptic symptoms so slight and obscure that little attention was paid her. On the 19th she was found dead in bed. On examination, the pericardium was found adherent to the heart in several places, and the interspaces filled with coagulated blood. A perforation, apparently the result of inflammatory softening, was found near the middle of the posterior wall of the left ventricle. The muscular substance around the rupture was considerably softened.

Dr Hallowell, in the *American Medical Journal* for November 1835, relates the case of a woman, 76 years of age, who was found dead in a privy. Her neighbours stated that she walked with difficulty, and that her body was constantly bent forward, and to the right. She had supped with good appetite the night before her death, and did not complain of pain. On making a post-mortem examination forty-eight hours after death, it was found that the falx cerebri contained three or four spots of ossification, but there was nothing else abnormal within the cranium. The pericardium

was distended with serum, and a clot weighing about twelve ounces was found enveloping the heart. On the anterior surface of the left ventricle there were two linear openings, three or four lines in length, and distant from each other about three lines. On further examination, it was found that these openings passed inwards, and joined each other near the centre of the ventricular wall, whence they formed one passage into the cavity. The thickness of the wall was diminished around the seat of rupture, and the heart-substance in the same neighbourhood was paler than natural. The whole muscular tissue of the organ presented a marbled appearance, due to the presence of yellow streaks running parallel to, and amongst the fibres, and its consistence was as friable as that of a liver. This condition was very evidently the result of fatty degeneration and infiltration. The muscles of the back were pale, and appeared to be entirely converted into fat. The extensors of the legs were in the same condition, the degeneration being, however, more advanced in those of the left side.

Another case of rupture, apparently due to fatty degeneration, is given in the *Bulletin Général de Therapeutique* for August 1835. The patient was a man 60 years of age, employed in watering the streets of Paris, who died suddenly. On examining the body at the Hôpital Necker next day, an irregular rupture about three lines in length was found near the middle of the posterior surface of the left ventricle. The cavity was dilated, and its parietes were thinned and softened. There were some points of ossification on the mitral valve.

Some cases given by Dezeimeris are quoted in the *Medico-Chirurgical Review* for 1839. One patient was a barrister aged 57, who, after suffering for some time from a train of symptoms evidently due to heart-disease, died suddenly. On examination, two apertures were found in the left ventricle. The heart was in a state of fatty degeneration. Another case is that of a man, 86 years of age, who, having been previously healthy, was suddenly seized with pain, heat, and oppression in the precordial region, and died almost immediately. On examination after death, an irregular fissure, one inch long externally, but of less extent internally, was found in the anterior wall of the left ventricle. The texture of the heart was soft and flabby. The cavity of the ventricle was not dilated, nor were its walls thinned.

Another man, 58 years of age, was seized with painful constriction at the precordial region, on the 20th of November. This attack passed off, but it returned again at irregular intervals till the 28th, when he died. A rupture was found at the apex of the right ventricle, and another in the anterior wall of the left ventricle. There were three other incomplete ruptures, not extending into the cavity of the heart, two of them being in the left ventricle, and one in the right. The ventricular walls were of the normal dimensions, but soft and easily torn. Another patient was suddenly

seized with violent oppressive pain in the chest, extending down the left arm. He had several attacks of this kind during three days, and on the third day he died, after a most severe paroxysm. On examination, a rupture nearly an inch long was found at the base of the left ventricle. The substance of the heart around the rupture was attenuated, and of a dark brown colour, "as if it had been gangrenous."

In this last circumstance, this case differs from the others given by the same observer; the first three were apparently connected with fatty degeneration, but here there seems to have been inflammation of that part of the heart where rupture took place. Dezeimeris relates another case, where the patient was a female advanced in age, recovering from a fever, and who was suddenly seized with severe pain in the chest, quickly followed by loss of speech, convulsions, stupor, and death. On practising auscultation during the attack, the heart's sounds were found to be confused, rapid, and irregular. On examining the body, a rupture an inch long was found in the anterior wall of the left ventricle. The internal opening exhibited traces of ulceration, which had destroyed the columnæ carneæ and muscular substance to a considerable extent.

A rare case of death from rupture of a cardiac aneurism is given in the *Medico-Chirurgical Review* for April 1827. The patient was General Kyd, 73 years of age. His death was sudden. The aneurism was situated on the left ventricle, close to the origin of the aorta. A similar case is given by Dr Bignardi, in the *Annali Universali de Medecinæ* of Milan for 1829. The patient, a young lady, in the enjoyment of perfect health, died suddenly, and, on making a post-mortem examination, it was found that death had been caused by the rupture of an aneurism situated at the junction of the aorta with the left ventricle.

In reviewing the recorded cases of rupture of the heart, the conclusion will be arrived at that the lesion is invariably associated with and dependent on some alteration in the texture of the organ; for there can be no doubt that in those few cases where the heart is said to have been healthy, the more efficient means of observation of the present day would have detected some morbid condition of the tissue. By far the most common cause of rupture is fatty degeneration, but it may also be dependent on inflammation, ulceration, abscess, or aneurismal dilatation, having for its seat that part of the heart where the perforation takes place. In 8 of the 29 cases collected in this paper, the rupture evidently depended upon fatty degeneration, and in 5 more there is strong ground for supposing that better means of observation would have discovered the same morbid alteration. In 5 cases it was due to ulceration, in 3 to inflammatory softening, in 2 to abscess, in 2 to cardiac aneurism, and in 4 cases the heart is stated to have been healthy. The symptoms vary very much in their duration: in 10 of the

foregoing cases death was absolutely sudden; in one, there was a death struggle of a few minutes' duration; in one, death followed in a few hours after stupor and convulsions; two died on the second day of their illness, three on the third, one on the eighth, one on the tenth, and another on the twelfth or thirteenth; in this latter case severe symptoms were present only during the last three nights of the patient's life. Two suffered "for some time" from fleeting pains all over the body; two were subject to dyspepsia, and other two to palpitation and symptoms of heart-disease during the same indefinite period. Of these six cases, two were found dead in bed, and other two died suddenly without any previous exacerbation of their symptoms; while in one, there was cough and dyspnoea for nine days previous to death, and another was seized the night before he died with severe pain in the chest, extending down the arms. One patient had suffered for four or five months from dyspnoea, and on the morning of her death she had a fainting fit, followed an hour after by another and fatal paroxysm; another had sustained a fall five months before her death, after which her health declined, and who, when she entered the hospital three days before she died, complained of catarrh, lumbago, and constipation. One old woman suffered for fifteen years from occasional paroxysms of severe precordial pain, and during the last seventeen days of her life all her symptoms were exaggerated. When the patient does not die suddenly, the symptoms observed in cases of rupture of the heart vary considerably in character as well as in duration; but they seem, in most cases, to have consisted of pain in the chest and oppressed breathing. Of the 29 cases, 15 were females, 12 were males, and in two the sex is not given. There was one patient between 20 and 30 years of age, three between 30 and 40, none between 40 and 50, two between 50 and 60, five between 60 and 70, seven between 70 and 80, and one between 80 and 90.

Mr Bayle, in his remarks on his case published in the *Lancet*, says, that in the majority of cases of rupture, the heart was found remarkably soft, and sometimes there was a brownish colour around the perforation. Out of ten patients, he says that one was between 50 and 60 years of age, another between 60 and 70, six between 70 and 80, and two between 80 and 90. Eight of these ten patients died instantly, one in two hours, and another in fourteen hours. To sum up, it would seem, 1st, That fatty degeneration is the most common cause of spontaneous rupture of the heart; 2^d, That death is, in the majority of cases, instantaneous; 3^d, That in those cases where the patient has not died suddenly the symptoms have generally been oppression of breathing, and more or less severe pain in the chest, frequently of a paroxysmal character; 4th, That most of the deaths took place between the ages of 70 and 80; and, 5th, That most of the patients have been females.

ARTICLE V.—*Disease of the Maxillary Antrum, involving the Brain.*
By R. S. MAIR, M.D., F.R.C.S.E., Madras.

DISEASE of the maxillary antrum terminating fatally by involving the brain is so rarely met with as to induce me to place on record the following case in point.

It illustrates the necessity of pronouncing a very guarded prognosis, even in those apparently simple forms of affections of the antrum known as simple abscess. The symptoms of the latter most commonly originate in the same simple manner as they did in the case about to be related, namely, pain or uneasiness in one of the molar teeth, followed shortly afterwards by a sense of fulness and tenderness over the cheek, and a discharge from the nostril.

I was first called to see Mr J. L., aged thirty years, on the 22d March 1861. He complained then, and for some days previous, of a copious fetid discharge from the left nostril, severe pain in the left cheek, extending upwards round the corresponding orbit. There was no swelling over any part of the nose or cheek; the third molar tooth of the left side was loose and painful, and oozing from its side was a free fetid discharge the same as from the nostril.

Suspecting these symptoms to be probably produced by some mischief in the maxillary antrum, the loose tooth was without difficulty removed, and with immediate relief. The discharge from the nostril disappeared, and the pain in the cheek and round the orbit almost entirely ceased.

Four days afterwards (26th), the same severe pain returned, but of distinctly intermittent character; there was still no discharge from the nostril or tooth-socket.

On the following day (27th), the patient had a sharp rigor, followed by fever, which continued for some hours, and the pain in the face and round the orbit continued unabated, notwithstanding the local application of anæsthetic anodynes. On the 28th, in addition to the above symptoms, there was threatening acute inflammatory action in the left eye. Leeches were ordered for this, but were not applied, owing to some prejudice in the minds of the relatives in attendance. Next day (29th), the whole eye was very much inflamed, the conjunctiva of upper and lower eyelids was congested and swollen, and the entire globe of the eye appeared to be fuller than natural.

Two leeches were then applied (the friends would permit no more), but even these, together with the application of ice over the eye, much relieved the pain and reduced the swelling.

On the 30th, the former symptoms returned as severe if not more so than before, and, on the following day (31st), the upper and lower eyelids were very œdematous, and the conjunctiva in a state of great chemosis. On raising the upper eyelid the globe was found full, tense, and protruding; the cornea very hazy, and vision almost entirely gone.

On the evening of this day (31st), Dr J. Shaw saw the case with me, and suspected abscess deep in the cellular tissue of the upper eyelid, behind the eyeball. An incision was made in the upper eyelid, close under the supra-orbital ridge, which gave vent to a discharge of some sanguineo-purulent matter.

There was immediate relief to the sense of fulness in the eye; the eyeball could be moved more easily, though vision was not perceptibly improved. The patient slept better that night than he had done for several nights previous, but otherwise, on the following morning (1st April), his symptoms were most unfavourable. The eyeball was enlarged to nearly double its natural size, and was protruded considerably forwards, while the upper eyelid was again very much swollen, and the lower one everted, exposing the chemosed conjunctiva, and leaving about one-half of the eye itself uncovered and exposed.

The removal of the tent placed in the wound after the incision was made was not followed by the escape of matter, nor was there any discharge after a probe had been inserted to the bottom of the wound. On the following morning (2d April), the general appearance was much the same; if anything, perhaps, the fulness of the eye and swelling of the eyelids were greater.

A probe was again inserted to the bottom of the wound in the upper eyelid in search of matter, but none was found. The original incision was then enlarged, still no matter escaped. Simple, soothing treatment was then adopted.

The patient complained of little pain, and remained tranquil up to 11 o'clock forenoon, when suddenly, and without a single premonitory symptom, he had a most violent convulsive fit, of an epileptic form and tetanic character. This fit, which was followed by two others of the same kind on the same day, was preceded by a peculiar scream, or howl, followed immediately by rigidity of the whole body, opisthotonos, foaming at the mouth, and complete unconsciousness. These fits each lasted about five minutes, but consciousness did not return till some time after.

This was the first indication of cerebral complication. It should be here noted that, prior to the first fit, in consequence of the great distention of the eyeball, and as no matter was found in the incision, which was made deep into the cellular tissue of the orbit, a seton was introduced into the left temple, and, subsequent to the fits, a cantharides blister was applied to the nape of the neck.

The case now assumed a most unpromising aspect, and the prognosis was accordingly very unfavourable. The patient remained dull, and sometimes apparently unconscious during the day. Nothing was done on this and the following day (3d and 4th April), beyond applying simple cold or tepid water to the eye, as was most grateful to the patient, and administering small doses of calomel. He occasionally appeared cheerful, and talked rationally; at other times he became incoherent, and lapsed into a state of low muttering delirium.

On the 5th April, the eye continued much swelled, and some pus escaped from the wound over the eyelid. The probe was again introduced to give free vent to the matter, but none came away. During this day the patient had a recurrence of the same fits as before, and while in one of them the pulse flickered and fluctuated so much as to threaten extinction every moment.

On the following day (6th April), he appeared very little better, talked now and then, and recognised some of his friends, though there was an evident tendency to lapse into a stupor or semi-comatose state.

He continued in the same condition all the 7th and up till the evening of the 8th April, when he became completely comatose. From this he never rallied, but gradually sank, and died early on the morning of the 9th April, sixteen days after he first consulted me.

The eye during the last three days of his life remained unchanged ; a small quantity of pus escaped from the wound in the eyelid, but there was little or no decrease in the swelling of the globe. The discharge from the nostril ceased after the globe began to swell, and that from the tooth-socket disappeared after the tooth was extracted.

The patient had always enjoyed good health prior to his last illness. He had none of the usual indications of the strumous diathesis, and there was no reason to suppose that he had any syphilitic taint in his constitution.

Post-mortem appearances.—Head only examined. Purulent matter in considerable quantity flowed from the cavity of the arachnoid, and from between the hemispheres on the removal of the falx. There was a layer of more consistent pus on the visceral surface of the arachnoid in some parts of both hemispheres, which, on removal, did not leave the arachnoid roughened.

At the anterior margin of the left hemisphere there was a rugged, excavated, and ulcerated surface, rather larger than a florin, covered with thick purulent matter, and appearing to be the source of the pus found in the arachnoid ; but on turning up the anterior edge of the hemisphere from the roof of the orbit, there was found on its lower surface, about an inch from its anterior extremity, a small opening, with dark-coloured edges, from which a thin serous and discoloured fluid was exuding. This opening led to a cavity large enough to contain a good-sized walnut, lined with a dark-greenish investing membrane, of at least half a line in thickness, which could easily be peeled off from the surrounding cerebral substance.

The brain was now removed, and, on being sliced into, presented a larger number of red spots than are usually met with. Otherwise the brain-substance was perfectly healthy, except at the two situations already specified, around which the cerebral substance was of a rose colour.

The left lateral ventricle contained a quantity of a dirty, muddy fluid, with small flakes of purulent matter floating in it, somewhat

similar to the fluid found in the peritoneal cavity in some fatal cases of puerperal peritonitis.

The crura and optic thalami presented nothing peculiar.

The examination of the investing membranes, and of the bones of the cranium, was now proceeded with. The roof of the orbit was first carefully examined, but presented no signs of disease, neither was its covering membrane detached at any point. Pus was found on the arachnoid, concreted at two points corresponding to the two abscesses in the brain above mentioned, but the membrane itself was not perforated at these points, and the pus was easily wiped away. A drop of purulent matter surrounded the optic nerve in the optic foramen, but there seemed no continuous flow either into or from the orbit by this channel.

The dura mater was now torn from the bones; it was everywhere adherent, and the bones themselves surrounding the optic foramen, and constituting the orbital roof, were perfectly healthy and of natural colour.

The orbital roof was next removed by two vertical saw-cuts, the inner of which also opened the frontal sinus of the left side. The latter was free from disease, but the orbital contents, the cellular tissue, fat and ocular muscles, were bathed in pus. The globe was flaccid and partially emptied.

On introducing the finger into the orbit, and passing it along its inner boundary, the latter was found diseased—the ethmoid bone crumbling before the finger, which passed readily into the upper part of the nose. Here all the osseous structures yielded readily to the touch of the nail, and portions of the ethmoid bone were removed by it with great facility.

They were in a state of caries, of very fetid odour, and bathed in pus; broken-down scrofulous matter on both.

The contents of the orbit being removed, the antrum was opened from above, when its cavity was found filled with a white soft substance of the appearance and consistence of firm blanc mange, and also very fetid. This substance, subsequently examined under the microscope, was found to consist mainly of tuberculous matter, interlaced with very delicate fibres, and showing an abundance of pus corpuscles. The membrane lining the antrum was entire, considerably congested, and streaked with red lines. The cavity of the antrum did not appear to be enlarged.

Remarks.—The form of disease, of which the above case affords an example, is one of uncommon occurrence and of exceptional complications, while it affords much interest, both practical and pathological.

Its apparently simple origin (the first symptom complained of being *ozæna*), and its rapid progress to a fatal termination, are the most noteworthy features.

The fetid discharge from the nostril, and from the side of the

loose undertooth, led me to suspect a morbid state (abscess) of the antrum of Highmore, and induced me to remove the tooth. The opinion then formed was verified by the post-mortem examination. Had the offensive discharge continued from the nostril, or from the socket after the tooth was removed, the indication for further treatment would have been obvious enough—viz., to puncture the antrum and evacuate its contents. But the original symptoms then almost entirely disappeared, the discharge and uneasiness in the cheek ceased, and the patient felt so comparatively well as to be able to attend to his ordinary office duties for five days afterwards, when the other symptoms set in, involving the entire eye and its appendages. These symptoms, which were only partially relieved by treatment, steadily and progressively advanced up till the 2d April, or eleven days after he first consulted me, when the first indication of cerebral complication showed itself by the violent convulsive fit, followed by others of the same kind, which speedily terminated in fatal coma and put an end to the patient's sufferings.

The examination after death revealed an amount of disease unexpected, and such as could not have been predicted during the lifetime of the patient.

The appearances then found suggest three modes in which the disease may have originated. 1st, By disease in the brain, terminating in abscess,—the purulent matter passing through the optic foramen, destroying the ethmoid and other bones of the left cheek, and filling the antrum; 2d, By caries of the ethmoid bone itself; 3d, By disease in the antrum.

The history, symptoms, and progress of the case during life, taken in connexion with the morbid appearances found after death, all tend, I think, to point out the last named as the probable origin of the disease, and the pathological sequence of events was in all likelihood as follows:—Disease of the antrum, originating in degeneration of the mucous membrane lining its cavity, or perhaps connected with the soft tumours which grow from the apex of the tooth, and from the lining membrane of the fang; secondarily involving the ethmoid, lachrymal, palatine, and inferior turbinated bones of the left side, causing suppuration and disintegration; the purulent matter filling the cavity of the antrum, extending towards the left nostril, causing ozaena, and upwards into the orbit, behind the globe of the eye, pushing the eye outwards and forwards; the matter finding its way through the optic foramen to the anterior surface of the left hemisphere of the brain, there acting as a foreign body, exciting inflammatory action, terminating in cerebral abscess, causing convulsions, coma, and death.

That the disease originated in the antrum, and that it probably commenced there some time previous to his consulting me there can, I think, be no doubt. 1st, Because all the earlier symptoms pointed to some local mischief in the cheek; 2d, No symptom indicating cerebral disease showed itself till some time afterwards; 3d, Be-

cause the membrane lining the antrum was found congested and streaked with red lines, and its cavity filled with a *semi-solid substance of a cheesy consistence*.

If the patient had any symptoms previous to my first seeing him, they may have been so slight (perhaps simple toothache) as to pass unnoticed until the offensive discharge from the nostril began.

The morbid matter found in the antrum having a soft, brain-like, or cheesy consistence, and showing, under the microscope, mainly a tubercular appearance, might lead to the suspicion that there was a malignancy about the case sufficient to account for its rapid course and the speedy destruction of tissue.

With reference to treatment, the case just related is suggestive. Were a similar case to occur to me again, I think it would be advisable to puncture the antrum, either through the tooth-socket or through the maxillary bone, inside the mouth. If no matter then escaped, and the symptoms continued, the cavity of the antrum might be freely injected with tepid water, so as to break down and soften the thick curdy or cheesy matter, and permit of its free escape through the puncture.

A case in some respects similar to the foregoing will be found related in the *Lancet* for 6th July 1862.

ARTICLE VI.—*Case of Intussusception, terminating in Sloughing and Discharge of the Intussuscepted Portion of Intestine; and Recovery.*
By JAMES BRYDON, M.D., Hawick.

INTUSSUSCEPTION is one of the most hopeless and distressing diseases for the cure of which medical art is called upon to exercise its skill. Its accession is sudden, its course is short, its termination is generally death, and its victims are for the most part in the prime of life, or only approaching to it. When fairly established it almost invariably terminates in strangulation and consequent mortification of the intussuscepted portion of bowel; and this, in its turn, almost as invariably terminates fatally. There are, as far as we know, only two ways in which this untoward result is avoided, either the invaginated portion sloughs and is thrown off per anum, the two ends of healthy intestine come together and become permanently united, and the patient recovers; or, by direct interference, the invagination is undone and a normal state is restored. No doubt cases of ileus are occasionally cured by, or at least recover after, insufflation and other remedies; but, when caused by intussusception, that it is curable by any such means is extremely doubtful. More probably, in these instances, it has arisen from some other and less obstinate of the twenty-one causes given by Copland.¹

Direct interference, with reasonable hopes of success, is appli-

¹ Medical Dictionary, vol. i. p. 367.

cable only when the disease is recent, and its site is the lower part of the large intestine, and can be reached per anum. Enterotomy has occasionally been performed, but its results have been such as to render its repetition scarcely warrantable. Wachsmuth¹ records two cases, both of which terminated fatally. Duchaussoy² gives five cases, only two of which were successful, and their subsequent history is not given. It is not a recognised operation in Britain: Sir William Fergusson, in his standard work on Practical Surgery, does not even mention it. Dr Greig,³ of Dundee, comes very near the truth when he says, "It is difficult to reduce one by manipulation after death, and I have no doubt it would be much more so to do it during life." Why it should be so, the following passage sufficiently explains:—"On dissection, we generally also meet with peritonitis to a great extent, and occasionally partial gangrene, at the seat of the intussusception, and coagulated lymph fixing the invaginated portion of intestine in its unnatural situation, the coats of which are more or less thickened."⁴

Termination by exfoliation, and recovery, is of very rare occurrence if we are to judge from the number of cases that have been recorded. There may be others, but I have been able to find only one in the British journals of the last nine years. This occurred in the practice of the late Dr Paul, of Elgin, and was communicated to the Medico-Chirurgical Society of Edinburgh, by Dr Warburton Begbie.⁵ Watson, Craigie, Copland, *Monro tertius*, and other writers I might mention, whose spheres of observation have been most extensive, seem never to have met with it at all. Duchaussoy, writing in 1863, was able to collect from all sources only sixty-five cases. The great rarity of the following case, rather than any practical benefit to be derived from its narration, is the reason why I have thought it worthy of publication.

On the forenoon of the 27th of November last, I was called in to see A. Scott, a millworker, æt. 35. He was suffering from pain in the abdomen, intense and constant, and recurring every four or five minutes in exquisitely severe paroxysms, causing him to cry out like a woman in labour. The pain was principally about the region of the umbilicus, and was not in any marked degree aggravated by pressure. There was considerable general tympanitis, but no local tumour. He said that he felt as if his bowels were standing still, and being crushed to pieces. His countenance was pale and anxious-looking, and now and then he felt faint. He complained much of coldness, the skin was cold and moist, but he had no distinct rigors. He was retching often and violently, but nothing was rejected. The appearance of the tongue was quite natural.

¹ Virchow's *Archiv*, vol. xxiii. p. 119.

² *Mem. de l'Acad. de Médecine*, vol. xxiv. p. 97.

³ *Edinburgh Medical Journal*, 1864, p. 315.

⁴ *Monro, Morb. Anat. Gullet, Stom., and Intest.*, p. 63.

⁵ *Edinburgh Medical Journal*, 1857, p. 665.

The pulse was very small and about 80. I learned from himself and his wife the following facts bearing on the history of the case:—His parents, brothers, and sisters were very healthy people. He had nearly all his life been subject to heartburn, flatulence, and other symptoms of dyspepsia, but, nevertheless, considered himself a strong man, having never so long as he could recollect missed a day's work. His bowels had generally been very regular, and opened every night. On the 26th, the day previous to his illness, he was in his usual good health, and partook heartily about 3 o'clock, after coming home from church, of a tea dinner, the last meal he had that day. Between nine and ten he took a walk, and had his bowels freely opened. Shortly afterwards he went to bed, and slept soundly all night. He arose about half-past five and went to his work at six. About half-an-hour after he was seized with pains about the navel, coming and going, but always increasing in severity. There were frequent and urgent calls to go to stool, but no feces were passed. The pain and tenesmus still increasing, he was obliged to go home, which with difficulty he accomplished. In a short time I saw him, and found him as described above. He was ordered to have warm applications, and an antispasmodic anodyne mixture frequently repeated. Obtaining no relief, in about four hours he was ordered to have three drops of croton oil, and an assafoetida injection, containing two drachms of oil of turpentine.

28th.—The bowels have not been moved, and the pain and retching are unabated. To have thirty drops of solution of muriate of morphia every hour. In the forenoon he passed a small quantity of feces, dark-coloured and very fetid. 10 P.M., no improvement. Solid opium to be substituted for the solution of morphia.

29th.—He remained nearly *in statu quo* till 11 o'clock this forenoon, when he had a copious evacuation of the bowels. The stool was intensely fetid, and consisted principally of blood, dark and unclotted. The pain has ceased in a great measure, but the retching continues. The countenance looks shrunk and anxious; the tongue is white and sodden, and the pulse is 90, small. To have bismuth and hydrocyanic acid, and brandy and arrowroot in small quantities.

30th.—Feels much exhausted, and has slept almost none since he took ill. Complains greatly of thirst. The stomach has retained a little of the brandy and arrowroot.

1st December.—Another restless night. The bowels were again opened this forenoon. The stool was scanty and composed mostly of blood. The stomach still feels irritable, but the retching is less frequent and nothing is vomited.

2d.—*In statu quo*. Bowels not moved.

3d.—A little discharge from the bowels; dark brown, very fluid, but not so fetid.

4th.—More restlessness. Much tympanitic distention of the

abdomen. Stomach more irritable. To have a fetid enema, and to continue the hydrocyanic acid and bismuth.

5th.—Had a very restless night. The injection brought no feces away. The tympanitis is increased. To have an ounce of castor oil. 10 P.M., the oil operated freely in about three hours. The discharge was very copious and fluid, and of a brownish black colour. It was not so fetid as formerly, and was accompanied by a considerable amount of flatus, which greatly diminished the tympanitic distention.

6th.—Rested better through the night. It was observed this morning that his trunk, arms, and thighs were covered with a measly eruption. The bowels are very open, the stools coming away almost involuntarily. Pulse 75, very small. To take twenty drops of pernitrate of iron four times a-day.

7th.—*In statu quo*, except that the eruption has increased.

8th.—Slept well, and feels better. Passed this morning, along with a watery stool, a piece of bowel, with the following characters: It is eighteen inches long, and, when slit up, two and a-half broad. Both internally and externally its colour is dark brown, with some patches almost quite black, and others ashy grey. It is darker at the extremities than towards the middle. Its ends are smooth and regular, and nearly as straight as if they had been cut by a knife. It contained a small quantity of greyish fluid, quite different from the stool with which it was discharged, and destitute of fecal odour. There is no peritoneum attached to it, but the line of its former insertion is quite apparent, from the lightness of its colour, owing probably to the absence of the peritoneal coat. The lining membrane is destitute of *valvulæ conniventes*. Discontinue all medicine.

9th.—Has had a good night. The bowels have been opened only twice since yesterday, and the stools are of a lighter colour. Has a pretty good appetite for food.

10th.—Was very restless all night. The tongue is red and dry, and the pulse 94. Complains of pain in the right iliac and lumbar regions. There is considerable flatulent distention, but no localized swelling.

11th.—Had a better night. Only one passage since yesterday; stool still quite watery, but of a lighter colour.

12th.—Had through the night a severe attack of diarrhœa, which was stopped by an injection of starch and laudanum.

13th.—Bowels not again moved. The flatulent distention and the eruption have disappeared.

18th.—There had been passage from the bowels pretty regularly once every day up till last night, when he had another attack of diarrhœa. The stools are of a natural colour. His appetite is good, but he is much annoyed with flatulence and consequent pains, and there is still a soreness in the right side. To have infusion of quassia and bicarbonate of potass thrice a-day.

27th.—The appetite is good, and he feels much stronger; but

the bowels are very irregular, and the stools watery. Since the 22d there has been considerable œdema of the feet and ankles. The urine contains no albumen, and the heart's sounds are normal. The pain on the right side has quite disappeared when he is in the recumbent posture, but when walking or standing he frequently feels a very uneasy feeling, as if his bowels were suspended by a string.

His recovery from this time, although not very rapid, was steady and uninterrupted. The œdema had disappeared by the 11th of January, and he was able to resume his work on the 24th, feeling nearly as well as when he left it off two months before.

Remarks.—The causes of intussusception are not well understood. Copland says, that “one of the most common causes of invagination of the intestine is the inappropriate use of drastic purgatives;” and again, “costiveness is often a prelude to this change.”¹ In this case, neither of these agents was in operation. The patient had taken no medicine whatever for some years, his bowels were freely opened the night before the seizure, and no masses of hardened feces were passed afterwards. He was, however, much and habitually troubled with flatulence; and this, on the present occasion aggravated by long fasting, would probably have something to do with it. This is all the more likely, if we are to believe that the conditions of the bowel, immediately preceding invagination, are inordinate dilatation of one portion and spasmodic contraction of another.

That the usual termination of invagination should be unfavourable is not to be wondered at, when we take into consideration the nature of the disease and of the parts involved. There is a constant passage of matter through the tube of the intestines, absolutely necessary to the wellbeing of the system; and anything interfering with this, in a short time gives rise to serious inflammation, or even in a much shorter time causes death by the shock given to the vital powers. The immediate causes of this obstruction in invagination are,—the mechanical obstacle offered by the invaginated portion, and by the swelling arising from inflammation of it, of the invaginating portion, and of the surrounding structures; and the vital obstacle arising from the disordered action of all the parts affected. The natural movements may be increased, diminished, or altogether suspended. The predisposing cause may also still continue in operation. If the obstruction continue complete after a certain time, the time which the system is able to bear up under it, death is inevitable. In this I believe we have the explanation of cases of elimination and recovery. In these, at first, the passage may be quite occluded, but long before there is time for separation of the invaginated part, it again, from causes of which we are ignorant, becomes permeable. Less severe inflammation is thus excited; and while nature is accomplishing successfully the detachment of the mortified bowel and the union of the healthy ends, the general

¹ Med. Dict., vol. i. p. 368.

functions of the parts, although disordered, yet go on. In the present case, it is probable that up to the forenoon of the third day, the time during which the severe suffering lasted, the passage was quite shut up. After this it is pretty evident that it was open. About this time, and when mortification had probably taken place, there was a copious discharge of blood. This, by diminishing the swelling and by allaying irritation, would no doubt materially assist in restoring permeability. The stomach continued irritable, but little or no food was rejected, and there was frequent passage from the bowels. Indeed, that the passage was open is demonstrated as clearly as, under the circumstances, it is possible for it to be, by what took place on the 5th December. It was considered necessary to give the patient a dose of castor oil, and this operated in three hours, and without causing any particular uneasiness. This could never have taken place had the occlusion remained complete.

A point of great interest and importance is the rapidity with which the healthy ends of intestine unite. This is hardly what we might have expected, considering the unfavourable circumstances under which they are placed. In this case, union must have been complete, or at least strong enough to resist the usual tension to which they are exposed by the eleventh day. This can best be explained by the readiness with which serous membranes become attached to each other by the products of inflammation. In intussusception we have peritoneum applied to peritoneum, and the necessary amount of plastic exudation. The union of the other coats takes place subsequently, and the raw surfaces exposed during this process will, in a great measure, account for the localized pain and other symptoms described as occurring during recovery.

On the ninth day of his illness there appeared a copious measly eruption. The explanation of this probably is, the vicarious relation which is well known to exist between the skin and the lining membrane of the intestines.

As to treatment, very little need be said, as very little that is beneficial can be done. When invagination is completely established, and this is always to be suspected when we have the symptoms of sudden obstruction followed by a discharge of blood from the bowel,¹ the only hope of recovery is exfoliation of the invaginated portion. Rest of the parts, and, as we have seen, a patent condition of the enclosed bowel are the necessary conditions for the accomplishment of this. Opiates, therefore, should be administered, and almost absolute starvation enforced. Cathartics are always pernicious, and, when the first dose has failed, ought never to be repeated. In the earlier stages they only increase the causes of obstruction and consequent suffering; and at a later period they must have a great tendency to destroy the conservative adhesions which have begun to form.

Even when the portion of intestine has been successfully eliminated, and the patient has recovered from the immediate effects of

¹ *Vide* Watson's Princip. and Prac. of Physic, vol. ii., p. 437.

the disease, the chances of life are very much diminished. The severity of the shock to the general system is shown in this case by the rapid emaciation it caused, and also by the œdema. Dr Paul's patient was seized with acute phthisis in about a month after his recovery. Abnormal conditions are very apt to be established at the seat of the disease, which eventually bring about a fatal result. In seven of the cases given by Duchaussoy, contraction of the intestine followed (*loc. cit.*).

ARTICLE VII.—*Vital Statistics of Iceland and Reykjavik during the Last Decennium.* By JOHN HJALTELIN, M.D., Inspecting Medical Officer of Iceland.

SINCE Dr Schleisner, in the year 1849, wrote his book on the Medical Statistics of Iceland, little has been done in this respect but what may be found in the general statistics of this island, which are written down in a year-book, named “*Skýrslur um landshagi á Islandi*” (Statistical Reports of Iceland); but although that work is exceedingly well compiled, and of great value as regards different economical and political affairs, it cannot, from its plan, be expected to go deeply into medical statistics.

Having now practised in this country for more than ten years, I have collected vital statistics for this last decennium, and arranged them into the following tables. One of those includes the whole island, and the other the little town of Reykjavik. Both those tables are built on reliable reports, and are in that respect, perhaps, more accurate than the Icelandic vital statistics of Dr Schleisner, which, although they are collected with great care, are, for want of accurate antecedent reports, more to be looked on as a probable calculation than as true vital statistics of the country.

TABLE I.—*Showing the Population, Births, Deaths, and Number of Still-born over the whole Island during the Decennium of 1854–1863 inclusive.*

Year.	Population.	Births.	Deaths.	Deaths in the 1st year.	Deaths from the 1st to the 15th year.	Still-born.	Death-rate per cent.
1854	63,874	2,557	1,509	740	150	75	2 ⁴ / ₁₅
1855	64,483	2,499	1,890	657	155	55	2 ⁴ / ₁₅
1856	65,836	2,477	1,485	605	194	60	2 ³ / ₁₅
1857	66,929	2,748	1,655	629	348	82	2 ⁵ / ₁₅
1858	67,847	2,937	2,019	685	382	80	3
1859	67,954	2,680	2,573	832	461	80	3 ⁷ / ₁₅
1860	66,839	2,460	3,326	849	1,028	70	5
1861	66,973	2,525	2,391	708	645	80	3 ⁸ / ₁₅
1862	66,792	2,693	2,871	895	395	90	4 ³ / ₁₅
1863	67,326	2,649	2,115	580	253	97	3 ¹ / ₈
	Sum.....	26,225	21,834	7,180	4,011	769	

TABLE II.—*Showing the Population, Births, Deaths, and Number of Still-born in the Parish of Reykjavik during the Decennium of 1855-1864 inclusive.*

Year.	Population.	Births.	Deaths.	Deaths in one year.	Deaths from the 1st to the 15th year.	Still-born.	Death-rate per cent.
1855	1774	61	50	10	5	1	$2\frac{3}{10}$
1856	1763	55	55	20	19	2	$3\frac{1}{11}$
1857	1792	57	51	13	5	5	$3\frac{1}{6}$
1858	1839	50	50	13	8	2	$3\frac{7}{10}$
1859	1893	62	59	12	6	1	$3\frac{1}{3}$
1860	1877	59	61	9	12	3	$3\frac{1}{4}$
1861	1916	56	36	4	4	0	$1\frac{3}{10}$
1862	1916	47	54	8	8	1	$2\frac{8}{10}$
1863	1960	72	50	14	6	2	$2\frac{5}{10}$
1864	1990	58	42	10	0	0	$2\frac{1}{11}$
	Sum.....	577	508	113	73	17	

I find it necessary to make some remarks relating to these tables. The decennium here spoken of is very remarkable, not only on account of many epidemics which made their appearance, but also for the severity of our climate during this period. Cold winters, with a heavy drift of polar ice, much fall of rain and snow, even sometimes during the summer months, have tormented this island, and been very destructive, not only of sheep and cattle, but also of human life; and in order to show this clearer, the following remarks on climatology and epidemiology for each year seem to be necessary.

The year 1854 was remarkably cold and stormy; summer season rainy—no epidemics.

The year 1855 was very favourable; winter mild, and hot summer. A mild influenza was very common during the summer months, but occasioned little mortality among the inhabitants.

In the year 1856 there was a cold winter and spring; an outbreak of diphtheria in Reykjavik and surrounding parishes; sporadic typhus in the north part of the island.

In the year 1857 the winter was very severe; a heavy drift of polar ice to the north part of the island; diphtheria still raged in Reykjavik and its neighbourhood; typhus now began also to be more frequent than formerly.

The winter of 1858 was very severe, with still continuing polar ice-drift; diphtheria now began to spread to the north and western part of the country.

The winter of 1859 was extremely severe, with a drift of polar ice to the north, east, and west part of the island; hard freezing, even during the spring, so that sea-water in some firths of the west part of the island froze to a depth of more than 50 feet. Typhus now began to spread over the whole country, accompanied by typhoid fever and dysentery.

The winter of 1860 was mild, but the spring extremely cold, with tremendous ice-drift to the north part; loss of sheep and cattle; scarcity of food. Typhus and typhoid fever raged with tremendous severity through the whole country.

The winter of 1861 was mild, and the spring warm; and the summer season unusually mild and fine. Many sporadic cases of diphtheria; severe rheumatic affections, and fevers very common.

The winter of 1862 was rather cold and stormy; the spring and the summer exceedingly cold and dry. About the 13th of May, a very common and malignant influenza broke out in Reykjavik, and spread from hence during the two following months over the whole country. It was one of the most dangerous and malignant epidemical catarrhs that ever visited this island, augmenting the mean death-rate in the whole island by nearly 2 per cent.

The winter and spring of 1863 were cold, and the summer cold and rainy. Epidemical pneumonia was very common over the whole island, and occasioned, in many places, great mortality amongst the inhabitants where no medical aid could be obtained.

The winter of 1864 was very stormy and rainy, but spring and summer warmer than usual. No epidemics.

It is accepted by Dr Schleisner that the great mortality amongst the inhabitants of Iceland is founded on two principal causes—viz., 1st, The great mortality of infants in the first year; and, 2d, The epidemics, which, on account of the want of medical aid, become more dangerous here than in any other part of Europe.

The mortality of infants in the first year has, according to the general registers, often been enormously high; thus we find in the five years, 1851 to 1855 inclusive, the death-rate of this age to be no less than 40 per cent.; in the five years of 1856-60 inclusive, it has been found 39 per cent.; but taken on the whole in the last ten years, the death-rate during the first year is equal to nearly 33 per cent., or about 8 per cent. more than the mean death-rate of this age is accepted to be in England and most other countries of Europe, where the average mortality of infancy during the first year is calculated to be from 24 to 25 per cent. For the town of Reykjavik the mortality of infancy has been very low, and little more than 19 per cent.; and supposing that no higher mortality had been in the whole country during this ten years, 15 or 16 per cent. of infant life would have been spared every year, which might have augmented the population in the same ratio. The death-rate from the first to the fifteenth year has for the whole country been 11.91, or one-half of all deaths in this period. For Reykjavik it has been far more favourable, or about two-fifths of all the deaths.

The greatest destruction of infant life in Reykjavik during this period was occasioned by diphtheria, and a considerable mortality occurred during the years 1856, 1857, and 1858, when the mortality of children rose to 4 per cent. The greatest mortality for the whole island was in the year 1860, when it is found to have been 5 per

cent. of the whole population; for Reykjavik it was far less, viz., 3·50 per cent. In the year 1862, the mortality for the whole island was 4·30 per cent., for Reykjavik 2·80 per cent., and therefore far more favourable. This has, however, not been the case in the former epidemics of this kind, for, in the year 1834, the mortality from influenza is found to have been both for Reykjavik and the whole country about 5 per cent. of the whole population; and in the year 1843, when an epidemic of the same kind here was raging, the mortality of that epidemic is found to have been about 5 per cent., as well for the town of Reykjavik as for the whole country. I can therefore not help thinking that my treatment of the latest epidemical influenza, as described in the *Edinburgh Medical Journal*,¹ has been very successful; and it seems to me that I might make the same claim for my treatment of the last typhus epidemic, as the mortality for Reykjavik was far less, or nearly 2 per cent. lower than for the whole country. It might certainly be supposed that our little town was more favourable for longevity than the country, but this cannot be demonstrated, and is contradicted by experience; on the contrary, we find the town of Reykjavik more unhealthy than the country itself, not only on account of its low situation at the seashore, but especially on account of the surrounding hills, and a lake at the south side of this town with stagnant muddy water, which very often occasions unhealthy emanations. Besides this, the largest portion of the population consists of fishing-people, who live in small, badly ventilated, and often not very clean huts, having very little milk or meat diet, which is abundant in the country, but living on all sorts of fish, both salted and dried, without due mixture of vegetables and condiments. Some of these people are also inclined to the overuse of strong liquors, which often produces delirium; and other maladies occasioned by alcohol are not of unfrequent occurrence.

While investigating the mean death-rate in the different parishes of Iceland, I found out a parish uncommonly favourable for longevity; this is the parish of Þingvalla, where the old Alþing (diet) assembled for nearly ten hundred years. This parish is of course very small, and has about 130–140 inhabitants, and out of this population I find only twelve deaths during the last ten years, one-half of these more than sixty years old, and a third nearly ninety years old. The geological feature of this parish is very interesting, the whole of it being situated on a lava current, which in ancient times flowed from the large but now extinct volcano “Skjaldbreið.” This lava stream is now all overgrown with small birch-trees, and has the most clear and pure drinking water I ever saw in my life. The elevation of the lava stream above the sea-level is very considerable, for although I have not as yet exactly measured it, I think it must be about 800 feet. There are no bogs to be found in the whole parish, and it is therefore free from all bad emanations.

¹ February 1863.

I am now just going to visit this place in order to find out from the parish books the mean death-rate of the population as far back as possible; and when I have done with that task, I'll send my account to the Edinburgh Medical Journal, and hope it will be interesting, as I have heard from old people that the healthy state of this parish has for a long time been known and remarked by its inhabitants.

REYKJAVIK, 3d July 1865.

Part Second.

REVIEWS.

On the Nature, Cause, and Treatment of Tuberculosis. By HORACE DOBELL, M.D., M.R.C.P.L., Physician to the Royal Infirmary for Diseases of the Chest, etc., etc. London: Churchills: 1866.

DR DOBELL appears as the proposer of a new theory to explain the phenomena of tuberculosis. This theory is thus stated by its author:—"Tuberculosis is due to defect in the action of the pancreas on the fat taken as food (especially the solid fat). The supply of properly prepared fat is cut off from the blood: 1, by the fats not being brought into a proper condition by the pancreas; 2, by loss of absorbing power in the small intestine, due to the contact of unhealthy pancreatic juice and of defectively prepared food with its mucous membrane. Thus, the blood becomes deficiently and defectively supplied with fat-elements from the food; is unable to afford those required for direct combustion; does not replace those taken up during interstitial nutrition; but, on the contrary, takes up more to compensate the deficient supply from the food. This having gone on up to a certain point, the fat-elements of the albuminoid tissues are seized upon, and these tissues are minutely disintegrated in the process. This disintegrated albuminoid tissue is nascent tubercle; and this process of disintegration is tuberculation.

"Tuberculation will take place first wherever the following combination of conditions is most marked.

"1. Greatest activity of interstitial nutrition.

"2. Smallest amount of fat-elements able to be spared by the tissues.

"3. A double process going on, consisting of: (a) ordinary interstitial nutrition in albuminoid tissue; (b) interchange of oxygen and carbonic acid or carbonaceous matters through this tissue.

"The tubercle thus formed may be allowed to remain on the spot where it is formed, constituting a *primary deposit at the point*

of origin, and this will especially occur when formed under the three conditions just specified; or, it may not be allowed to remain on the spot where formed, but be at once carried away by the lymphatics in a minutely divided condition, and either arrested in the lymphatic glands or carried on into the blood to be deposited from it, constituting a *primary deposit distant from the point of origin*; or, having been primarily deposited in either of these ways, it may be taken up by the lymphatics and deposited in the lymphatic glands, or carried into the blood and deposited from it, constituting a *secondary deposit*.

“In the *advance* of these diseased processes, any part of the body in which nutrition is going on in albuminoid tissue may become both the source and seat of tubercle; and any part, whether albuminoid or not, if supplied with lymphatics or bloodvessels, may become the seat of tubercle.

“Pure tuberculosis commences when fats, properly acted upon by the pancreas, cease to pass in normal proportions into the blood.

“Tuberculisation commences when albuminoid tissue is abnormally seized upon for its fat-elements.

“A secondary state is superadded when tubercle has been carried into the circulation, which constitutes tuberculæmia or tuberculous blood-poisoning.”

The defect in the function of the pancreas may, according to Dr Dobell, be produced in four ways: *first*, by any cause which for a length of time reduces its activity, by diminishing the normal demand for carbonaceous matter in the blood; *second*, by powerful or prolonged depressing influences acting on the nervous system; *third*, by inflammatory and other abnormal conditions of neighbouring parts; and, *fourth*, by loss of absorbing power of the small intestine by which the function of the pancreas is rendered useless.

We are by no means prepared to adopt this theory, although Dr Dobell says of it,—“I feel safe in asserting that there is hardly a well-established fact connected with the origin, course, terminations, and therapeutics of tuberculosis, which cannot be more satisfactorily explained by this hypothesis than by any other yet suggested.”

We do not believe that any complicated process, such as tuberculosis, is to be explained by the presumed diseased condition of a single organ; and we do not agree with Dr Dobell in his views as to the origin of tubercle, which we believe to be always of local origin, and not to be transported, as he supposes, in many cases, by means of the blood. Plausible enough arguments can, no doubt, be brought forward in favour of this theory as of many others, but when we come to look for real *proof* of its correctness, the evidence in our opinion breaks down. In the first place, Dr Dobell admits that, after a careful examination of the pathological reports at St Bartholomew's and elsewhere, he failed to discover a coincidence of disease of the pancreas and tuberculosis. “In every case of

uncomplicated phthisis in which the pancreas was mentioned, it was reported 'healthy.'" He continues: "In consequence of this disappointing result, I set down as subjects for future work, the questions:

"1. What evidence can be found that the pancreatic secretion can be deprived of its normal properties without leaving traces of structural disease in the pancreas?

"2. Are there not affections of the pancreas competent to deprave its secretion, but which have not hitherto been recognised as disease?

"Both of these questions may now be answered in the affirmative, almost with certainty."

All we have to say is, that Dr Dobell is much more easily satisfied than we are inclined to be. In order to answer the first question, Dr Dobell appeals to Claude Bernard's experiments, and to the experience of a "shrewd and observant" butcher. Bernard has shown that when the nerves of the pancreas are acted upon, either by excitation of the cerebro-spinal or by section of the sympathetic fibres, the secretion of the gland becomes abundant and uninterrupted, while profuse diarrhoea is established. At the same time the character of the secretion is changed, the characteristic principles being no longer produced. This undoubtedly shows that the pancreas, like other secreting organs, is under the control of the nervous system; but what support does it give to Dr Dobell's hypothesis? The testimony of the "butcher" is to the following effect:—

"I have been informed by a butcher who deals largely in calves' sweetbreads, and is a shrewd and observant man, that there is a great difference in the qualities of sweetbreads, dependent upon the previous state of the nervous system of the animal. If the calf has been brought to London comfortably in a cart, the sweetbread is generally in good condition; but, if it has been 'worried about,' he is often obliged to throw the sweetbread away, as not good enough for sale."

Dr Dobell farther endeavoured, in answer to the first question, to bring forward absolute proofs of the nature and extent of the perverted or arrested function of the pancreas in tuberculosis, by the result of direct experiments upon the pancreatic juice of persons dying of phthisis. These experiments were carried on for some months, but the results proved valueless, because there was no standard of healthy pancreatic secretion for comparison, and because the attempts to obtain such a standard failed.

In answer to the second question, all Dr Dobell has to say is this,—“I am permitted by Dr Fenwick to state in general terms, that the results of a large number of examinations made by him of the pancreas in man and the lower animals, show a peculiar liability of this organ to different forms of degeneration. But as it is probable that Dr Fenwick will himself make public his observations, I forbear from giving them in more detail.”

Until we obtain the results of Dr Fenwick's observations, we must continue to consider the second question unanswered.

A difficulty which Dr Dobell does not attempt to explain is this; supposing his theory to be correct, how does it happen that where there really is organic disease of the pancreas, tuberculosis is rarely met with? Of course, if tuberculosis is the direct result of disease of the pancreas, where disease of the pancreas exists, we should look for tuberculosis. This, however, is not the case; organic disease of the pancreas is not very common, but tuberculosis is not met with in such cases in greater proportion than in cases of organic disease of other organs.

Dr Dobell next endeavours to show that his hypothesis explains in a very simple manner a mass of apparently incongruous facts in the etiology of tuberculosis. We cannot follow him in this branch of his inquiry, but can merely quote his conclusions as to the mode of accounting for the variable course of tuberculosis in different cases.

"1. A sudden, almost complete, or total suspension of normal pancreatic secretion, accounts for acute tuberculosis.

"2. A less complete suspension or perversion of the function of the pancreas accounts for chronic tuberculosis.

"3. Either of the above changes of pancreatic secretion, occurring intermittently, accounts for recurrent tuberculosis."

These conclusions, it is needless to say, are entirely unsupported by proof.

Dr Dobell's therapeutical conclusions are the following:—1st, That in recent cases where the symptoms are only premonitory, the indication is to restore the function of the pancreas as speedily as possible, by placing the patient under these conditions which call for pancreatic action, such as a cold dry atmosphere, rich in oxygen, good mixed diet, and so on; 2d, Where the disease has advanced so far as to make it be considered hopeless to restore the function of the pancreas, we must endeavour to supply the greatest amount of fat to the blood that is possible by other means than through the action of the pancreas, and by saving the albuminoid tissues to the greatest possible extent.

The volume concludes by reports of cases treated by pancreatic emulsion at the Royal Infirmary for Diseases of the Chest. That the pancreatic juice is of importance in digestion is certain; that it has something to do with aiding the absorption of fats is probable; and we are not surprised that the administration of the pancreatic emulsion of solid fat prepared with the entire juice of the pancreas of a pig just killed should do good in certain cases, just as pepsine or inspissated ox-gall is useful in certain cases. It will, however, require evidence of a very different kind from that adduced by Dr Dobell to satisfy us that the cause of tuberculosis is to be found in an abnormal condition of the pancreatic secretion.

Report on the Sanitary Condition of the City of Edinburgh, with Relative Appendices, etc. By HENRY D. LITTLEJOHN, M.D., F.R.C.S.E., Medical Officer of Health for the City. Edinburgh: 1865.

WE owe an apology both to our readers and to Dr Littlejohn for having so long deferred a notice of this excellent work. Our excuse must be, that the great interest which it excited from the very day of its publication brought it under the notice of all who take an interest in sanitary improvements. Dr Littlejohn's work, while treating especially of Edinburgh, is by no means exclusively of local interest. It treats of various large questions which are important in the case of every town or city, and forms an important contribution to sanitary science.

The impression conveyed by a perusal of this work is decidedly painful. We are in the habit in Scotland of boasting of the intelligence of the inhabitants; of the general diffusion of education; and of the practical tendencies of the national character. In many points, however, we are sadly behind. Overcrowding, with its accompanying demoralization, prevails to an alarming extent; habits of personal and domestic cleanliness among the lower orders are almost ignored; whilst even in a higher grade there still lingers amongst its members a certain reluctance to avail themselves freely of the benefits derived from fresh air and pure water.

Dr Littlejohn's work contains an interesting chapter on the density of the population in Edinburgh. The total Parliamentary area is 4191 acres, which, taking the population as in 1861, gives a ratio of 40 persons to the acre. At first sight this might seem satisfactory, for the ratio of the population to the acre in Liverpool and Glasgow is respectively 93·3 and 83·7. But it must be borne in mind, that in Edinburgh are large vacant spaces consisting of hilly grounds and country fields. By abstracting these the ratio of Edinburgh at once rises to 60 persons to the acre. But it is only when the acres and populations of special districts are determined, that the real amount of overcrowding is seen. It is then found that in the most densely populated district, "The Tron," the proportion of population per acre actually rises to 646! Even this, however, does not fully illustrate the manner in which human beings are in certain localities packed together. The lofty tenements which add so much to the picturesqueness of the old town of Edinburgh are, like ant-hills, actually swarming with inhabitants, while too many of these are nearly or altogether destitute of the requisites of ordinary decency. Thus, Dr Littlejohn tells us of a house in the Cowgatehead, consisting of five flats, having a population of 179 persons (24 children, 155 adults), and provided with two sinks and one water-closet. Even this, however, is surpassed by another tenement, which must be well-known to all who have ever been

engaged in dispensary practice in Edinburgh, "The Middle Meal-market Stair." This building consists of five flats, contains 248 inhabitants, and is utterly unprovided either with sink or water-closet. Such a condition of things is a disgrace to our vaunted civilisation; what can be expected of human beings huddled together like the lower animals, and living in a state of filth in which no farmer or grazier would allow his stock to be. This leads us to allude to the proposal to render the introduction of water-closets into all dwelling-houses compulsory. In reference to new houses, this should, of course, be the case; but in regard to the old tenements, there is a difficulty dependent at once upon the nature of the buildings and the habits of the occupiers. The following quotation from Dr Littlejohn will explain our meaning:—

"Centuries ago, when drains were unknown, and when the inhabitants, to keep themselves within the protection of the city walls, were compelled to add to the height of their houses so as to provide for the increasing population, the only way in which filth could be disposed of, was either by allowing it to accumulate within the houses, or by summarily getting rid of it by bringing it to the street or close. The dangers attending the first of these plans, soon became apparent in the presence of disease, and the recurrence of epidemics; while the second rendered Edinburgh notorious for its odours, especially at night. The great sanitary movement in England, which the successive epidemics of cholera were so instrumental in stimulating, was intimately connected with the perfecting of the drainage systems of large towns. This consisted in placing each dwelling-house in communication with the public sewers, and thus permitting the rapid escape of all offensive material. There can be no doubt that this was a sanitary measure of great importance, and as applied to English towns, effected the best results. The necessity of improved drainage was also felt in Scotland, and our chief towns were speedily drained, that is to say, the main sewers were formed. When it was attempted, however, to bring each house in our large tenements or 'lands,' as they are called, into communication with the sewers, the difficulties arising from our system of house accommodation at once became apparent. Our poor inhabit the dwellings of the wealthy classes of former days, necessarily subdivided into single rooms, which are occupied by separate families. There the inhabitants live crowded together, without distinction of sex, and in many instances we have the rooms, generally opening one into the other, still further subdivided by partitions, so as to allow of closer packing. The houses, too, are old, and in a state of decay; and their construction is very imperfect, so far as sanitary requirements are concerned. Under these circumstances, to enforce the introduction of conveniences, would be attended with the worst results. In England, where the poorest houses are self-contained, and necessarily small, with a court behind, in which the convenience is placed, the system works admirably. Any sewage emanations which may escape, at once pass into the atmosphere, and the air in the house is not contaminated; but the houses in the densely peopled districts of our Scottish towns are not supplied with such courts, and were conveniences placed outside the houses, from the large population which would use them, they would closely resemble those which are scattered throughout the city for the use of the public. Where such are largely frequented, it is a matter of great difficulty to preserve anything like decent cleanliness; the air becomes tainted; what is intended as a public benefit is very apt to become a nuisance to the surrounding neighbourhood. If, however, as has been proposed, they were placed on each flat of our tall lands, all the evils already referred to would exist in a confined atmosphere, and close to the sleeping apartments of our

crowded poor. It must be remembered that the simplest form of the convenience, as supplied for houses, is a piece of mechanism which must be handled with some degree of care, otherwise it is apt to get out of order—it becomes no longer air-tight, and a leakage of foul gas takes place. This occurs to a notorious extent in the best parts of the New Town, and although productive of discomfort and disease, these evils are greatly modified by the freer ventilation, and the less dense population. But in the houses of the poor this leakage would be constantly occurring in an already vitiated atmosphere, where the inhabitants are overcrowded, and prone to disease. The consequences can easily be imagined. Those forms of illness which are generally considered to be intimately connected with effluvia from drains would soon show themselves, and in such localities would quickly spread in an epidemic form; and it must be remembered that such debilitating diseases as fever and dysentery tell heavily upon the labouring population, and entail a large expense upon the community in the shape of increased poor-rates. The experiment has been tried again and again in our city, and always with similar results,—the production of much annoyance, and, ultimately, the removal of what was considered a sanitary improvement. A few months ago, an excellent opportunity was afforded of putting the matter to a practical test, not in an old house, but in one specially built for the purpose, in Dickson's Close, High Street; and the experiment is at present going on. Of the two conveniences on each flat, one was intended for males, and the other for females and children. No sooner were the rooms tenanted, than a large pool of sewage, both solid and liquid, collected at the foot of the main passage, in consequence of the choking of the pipes. Day after day was this sewage removed, but as constantly was it renewed. At length, after the authorities had repeatedly interfered, several of the conveniences were closed, and the number of parties frequenting the remainder curtailed; but in spite of these precautions, a state of matters is produced, by overflow and leakage, of the most nauseous description. No doubt the tenants were for the most part Irish, paying rents from £2, 8s. to £7, 10s., but it must be remembered that this is precisely the class who were supposed to receive most benefit from this sanitary measure. My experience of the poor of this city, and of their house accommodation, has led me to the conclusion that they are not as yet prepared to make a proper use of conveniences. The poor require preliminary education in keeping their houses and stairs clean, before they can be trusted in the manner proposed. Of this they themselves are well aware; and I have been informed that, in some instances, apprehensive of their neighbours, they have requested their landlord to withhold from the tenement the benefit of the so-called boon. One dirty neighbour disgusts another, and the sequel can easily be pictured. The houses, too, in the great majority of cases, were never intended for this sanitary improvement. They are of great age, many of them as strong as fortresses, and others so old as already to be falling to pieces, with walls hardly able to maintain themselves erect, much less to stand the operations of the sanitary engineer. With a population inhabiting such tenements innumerable difficulties must arise."

Such a state of things is by no means satisfactory. We do not see, however, how it can be very speedily remedied; the inhabitants must be educated, so as to become alive to their present degraded condition; while old dwellings must be gradually removed in which sanitary improvements are impossible.

Dr Littlejohn's Report treats of many other points of nearly equal importance; we can, however, only allude to the suggestions he proposes for the sanitary improvement of Edinburgh. These are of two kinds: those which it is the duty of the authorities to carry into execution, and those which should be adopted by the

inhabitants themselves. The first division includes the following points:—

“1st, The satisfactory paving and draining of the closes.

“2d, The improvement of the house accommodation of the poor, by insisting on the introduction of water and gas, the cleansing of common stairs, and the performance of necessary repairs.

3d, The diminution of overcrowding of the population, by limiting the number of persons in each apartment, by lowering the houses in height, and by removing all tenements in a ruinous condition.

“4th, The opening up of the worst localities by the widening of such contracted thoroughfares as the Cowgate and St Mary's Wynd, and by the formation of new streets, which should pass at right angles to the long and unwieldy closes, and give increased facilities for their thorough cleansing. Such new communications would also afford sites for improved dwellings for the poor where there are most required, and would form an era in the sanitary history of Edinburgh.”

We are glad to find that Dr Littlejohn's Report has already been attended by practical results. The fourth of the above suggestions has been warmly taken up by Lord Provost Chambers, who has suggested a plan for carrying it out. This plan, at least in its general features, has, after considerable opposition, been approved by the Town Council, and we trust that next session the sanction of Parliament may be obtained to a comprehensive scheme. The opposition which the Lord Provost's plan has met with is by no means creditable to our councillors. However disguised, the real ground of opposition is the disinclination to lay on an assessment. Nothing could be more short-sighted. No town is more dependent than Edinburgh upon salubrity and amenity, and no one can doubt that both would be largely added to, if many buildings which at present disgrace it were removed; if light, air, and publicity, were let into dark courts and closes; and if decent dwellings were erected for the artisan and labouring classes. These improvements are not merely urged upon us by a sense of duty, they would, we believe, be found pecuniarily remunerative. A diminution of death, disease, and probably crime, would be followed by a decrease in the poor-rates; while increased salubrity would doubtless be followed by an increase of strangers, on whose residence amongst us so much of the prosperity of Edinburgh depends. The whole inhabitants of Edinburgh are deeply indebted to Dr Littlejohn for laying before them the present condition of the city, and for having awakened the authorities to the necessity of taking prompt measures for the diminution of existing evils.

Atlas of Surgical and Topographical Anatomy. By B. J. BÉRAUD, Surgeon and Professor to the Maternity Hospital of Paris; Vice-President of the Biological Society, etc. Illustrated by 100 Plates, drawn from nature by M. BION. Translated by ROBERT THOMAS HULME, M.R.C.S. Eng. London: H. Baillière: 1866.

THIS is the first part of a work intended to illustrate the regional and surgical anatomy of the human body. It is to be completed in ten monthly parts, containing one hundred plates. The first part, now before us, embraces some of the external parts of the head, the cranium, the brain, and the region of the ear. The first plate shows the frontal and parietal regions, illustrating the relations of the nerves, arteries, and veins in these situations. In the second plate are drawings of the superficial and deep layers of the temporal region; in the third, of the mastoid and occipital. The fourth plate contains two drawings, the first, of the superior, the second, of the inferior surface of the base of the cranium. The first of these shows remarkably well the arrangement of the cerebral sinuses at the base of the skull, and the points of exit of the cranial nerves; while the second illustrates more particularly the somewhat complicated osseous and muscular arrangements in the inferior surface of the same region. In the fifth plate, are drawings of an antero-posterior and of a transverse section of the roof of the skull; both show the coverings of the brain, while the first illustrates especially the arrangement of the longitudinal, the straight, and the lateral sinuses; the second is intended to show how the effects of blows on the roof of the skull are transmitted to its base. The sixth plate shows the superior and inferior surface of the brain, as covered by its membranes. The remaining four plates are devoted to the region of the ear, illustrating the external and internal surface of the auricle; the region of the external auditory canal; the region of the cavity of the tympanum; the entire auditory apparatus; the middle ear and the internal ear. These four plates are particularly good; the drawings represent the parts the size of nature, while those in the other plates are reduced about one-half.

Judging from the first part, we can cordially recommend this work. The dissections are well planned; the drawing and engraving are well executed; and the colouring is excellent. Opposite to each plate is a page of letter-press, containing a full explanation of the drawings, as well as a few sentences expressing tersely the application of the anatomical details to pathology and operative surgery. Considering the amount of matter, and the style of execution, the price of the work is very moderate; the price of each part being five shillings plain, and ten shillings coloured.

Part Third.

PERISCOPE.

SURGERY.

LOCAL ANÆSTHESIA IN A CASE OF CÆSARIAN SECTION.

BY BENJ. W. RICHARDSON.

ON Wednesday, March 28th last, Dr Greenhalgh asked me if on the following morning I would produce local anæsthesia in the case of a woman whom it was absolutely necessary to deliver by the Cæsarian operation. Dr Greenhalgh had come to the determination of asking me to make the attempt, after much consideration, and for many reasons based on an able and thoughtful study of the operation. He argued that if the superficial long incision could be made quite painless, and if the closing of the incision with sutures could also be made painless, the patient would, in fact, feel nothing more than is experienced in an ordinary labour of very brief duration. He argued that the local anæsthetic process would prevent hæmorrhage. He argued that the effect of the cold would produce rapid uterine contraction. Lastly, he felt assured that by the local process the occurrence of vomiting, so frequent after the administration of chloroform, and so troublesome and dangerous, would be avoided. At the same time, the patient herself wished *not* to have chloroform, and expressed that she was in no dread of any degree of pain that did not exceed the pain felt in an ordinary labour. Appreciating the force of Dr Greenhalgh's arguments, I undertook, therefore, to endeavour to meet his views. I constructed hastily for the occasion a large double spray producer, using a double-necked eight-ounce bottle for holding the ether. I provided a perfect ether, and I employed two hand-bellows to work the spray. With the kind assistance of my friend Dr Sedgwick, I tested the capacity of the instrument thoroughly. I found that in thirty seconds I could render the whole of the front part of my own arm insensible from the elbow to the hand, and that the same effect could be produced in even a little shorter time on Dr Sedgwick. We provided for the introduction of the sutures a separate small spray.

The operation was performed on Thursday morning (29th) at 9-30, in the presence of a large number of obstetricians and surgeons. On visiting the patient previous to the operation, I found her to be a rather short, stout woman, and apparently about thirty years old. She had had an indifferent night, and her stomach was irritable. She feared, she said, that she should vomit. Her tongue was white and her countenance pale, but her pulse was 74, firm, and steady. Her bowels had been very freely relieved.

She was placed on a short, broad table, and covered with an oilskin sheet or garment, which had an oval opening in it sufficient to expose the front part of the abdomen freely, but no more. This covering was not passed over the face, but was folded neatly over the chest. At her own request a pocket-handkerchief was passed lightly over her eyes as a bandage. The lower limbs were separated, and were allowed to bend over the edge of the table, and were supported by two assistants, who were seated. Dr Greenhalgh stood in the centre, his assistant in the operation on the left side, and I and Dr Sedgwick, who assisted me, on the right. The body of the patient was laid in a semi-recumbent position, the head firmly supported with pillows.

Dr Greenhalgh having indicated to me the line of his proposed incision from the umbilicus towards the pubis, I commenced to narcotise locally in the centre of the space, making the ether tubes bend a little towards each other, and directing the brush of spray about one inch and a half from the skin. In a few seconds I found that the ether was telling on a space full four inches long

and two broad; I, therefore, widened the spray by bringing the spray tubes parallel, and I made a gentle movement upwards and downwards, treating the spray as though it were a brush. Whitening of the skin was perceptible at the twenty-fifth stroke of the bellows, but Dr Greenhalgh wished to widen the line of insensibility a little to the right side, so I lost a few seconds in doing this. At forty-five seconds the insensibility was perfect, and Dr Greenhalgh made his incision down upon the uterus, dividing the skin and a deep layer of fat with one sweep. I moved the spray in company with his knife, and the uterus was laid bare. During the time the patient reclined absolutely unconscious of the incision; she neither winced nor spoke, her countenance was perfectly placid, and her pulse, which was being observed by Dr Wilson, of Glasgow, underwent no variation. A breath of ether spray was directed upon the uterus after it was exposed, but, according to our preconceived plan, the ether was now withdrawn from the open wound, and was directed on the skin surface that had not been divided, on the right side below the level of the umbilicus. The uterus was thus prompted to contract readily.

Having made, also painlessly, his incision into the uterus, Dr Greenhalgh began to introduce his hand, but was somewhat impeded by the uterine contraction. At this point the patient first showed signs of uneasiness, and she asked of me "What is he doing?" Soon afterwards Dr Greenhalgh had hold of the feet of the child, and delivered very rapidly and easily. As the child was being expelled—for, in fact, there was an act of uterine expulsion—the membranes gave way, and the liquor amnii escaped freely with some noise. The proceeding caused the patient to start, and she said, in the same voice as women usually speak in labour, and in the same phraseology, "I am sure I shall die." A word from me that she was quite safe, and that the child was born, reassured her; but after the child was removed, and at the moment of the extraction of the placenta, she again complained; she also complained somewhat of sudden pain, evidently from the uterine contraction. The uterus contracted at once so firmly that Dr Greenhalgh, supported by the opinion of Dr M'Clintock, of Dublin, who was present, determined to put no sutures into the uterus. For my part, I do not think the uterine wound could have been found, for the uterus had disappeared below the lower edge of the skin wound, and its muscular fibres were closely knit from contraction.

We waited twenty minutes, or nearly, for any hæmorrhage that might come on in the course of reaction. During this time the patient talked to me as calmly as possible on various subjects, told me of her symptoms, of her previous confinements, of her two children, of the part of Essex she came from, of the distance of her home by rail, of the time it took to make the journey, and the like. Once or twice when the sponge in the wound was pressed a little downwards she said "it was sore," and she thought, as it was all over, she was kept a long time. This expression was rather that of fear that something was wrong than of uneasiness or pain. In inserting the sutures I narcotised for Dr Greenhalgh with a small single jet, point by point for each needle, and she felt no pain. After the sutures were brought together, however, I having left her, a single fine suture was passed through a part that slightly projected, without narcotism. This suture she felt, and she called out as the needle passed through the skin.

The infant, which seemed to be a little over the seventh month, died about an hour after its birth; but the mother from the time of the operation until now has progressed towards recovery without one untoward symptom. Dr Greenhalgh's report to me to-day (Wednesday, April 4) is as follows:—"My dear Richardson, You will be pleased to hear that our patient is progressing most satisfactorily. She eats, drinks, and sleeps well, is free from pain, is very cheerful, and has had her bowels relieved naturally on more than one occasion. She passes water freely and healthily. The wound has healed by the first intention, and I have this afternoon removed four of the sutures. It would be impossible for any case to do better.—Believe me, sincerely yours, R. GREENHALGH."

Notes.—In this case local anæsthesia by the ether spray process offered everything that could be desired, and I think was in every sense preferable to general anæsthesia from chloroform or ether. Indeed, Dr Greenhalgh's foresight in respect to these advantages is not less conspicuous than the skill and precision with which he performed the operation. I say this, not in any way to disparage chloroform in cases where it is really demanded, but as a matter of fact and experience. The advantages of the local method were these:—

1. The operation was painless: the pain that was felt was the pain of labour, and that in the lightest and shortest degree.

2. The patient, prior to operation, was disposed to vomit. Under chloroform she would almost certainly have vomited during the operation; the intestines would thus have been brought into the wound, and the operation would have been prolonged and made more serious. There might also have been after-vomiting. The tendency of the local anæsthesia was to check vomiting.

3. The action of the cold checked hæmorrhage. I do not think three ounces of blood were lost.

4. The action of cold in producing uterine contraction was in every sense beneficial.

5. The patient was not subjected to shock. I have often, even in deep sleep from chloroform, seen symptoms of shock as the knife entered the flesh, and have felt the heart stop as sharply as though a blow had been inflicted on it. By the local anæsthesia the patient, in her full natural power, was subjected to no kind of cardiac embarrassment.

6. The consciousness of the patient was an advantage to the operator. She never was restless, she never moved her body for a moment, and when she was once asked not to bear down with the diaphragm, she obeyed immediately.

7. During the operation there was not the remotest anxiety that the patient would die from anæsthesia.—*Medical Times and Gazette.*

ON THE TREATMENT OF FISTULOUS DISCHARGES DUE TO CARIES, ETC., BY THE
"LIQUEUR DE VILLATE." BY M. NOTTA.

In a former communication, in 1863, M. Notta drew attention to the value of the *Liquor de Villate* in the treatment of certain forms of obstinate fistulous discharges. Transferred from veterinary practice, in which it has been most advantageously used for more than twenty years, this substance has proved of great service, according to the testimony of MM. Notta, Nélaton, and other distinguished French surgeons. In the present paper M. Notta relates several additional cases, in order the better to distinguish the circumstances under which it can be resorted to with advantage. Those in which it has proved pre-eminently useful have been examples of caries of bone, accompanied by fistulous tracks, which have resisted various other modes of treatment. Still, it must not be indiscriminately employed here as it will succeed only in cases in which the caries is soft and vascular, and of limited extent, being of no avail when this is very extensive or sequestered. Another class of cases which can be successfully treated are fistulæ consecutive to chronic abscess, often when iodine has been tried in vain. In several cases of tubercular fistula of the testis, M. Nélaton has effected a rapid cure by five or six injections. Even when the abscess has been of an acute character, but has eventually given rise to great detachments and obstinate fistulæ, the *Liquor* is found of equal value, several examples of which are given by M. Notta. But any attempts that have hitherto been made to extend its employment to the seemingly analogous case of fistula in ano have so decidedly failed that there is no encouragement to repeat them.

The composition of the *Liquor de Villate* is as follows:—Liquid subacetate of lead 30; sulphate of copper and sulphate of zinc aa 15, and white vinegar 200 parts by weight. It is essential to remember that it is suitable only when the affections reach their chronic stage; and that if employed prior to this period, in place of a salutary, modificatory inflammatory action, it may give rise to a phlegmon of a dangerous character. In suitable cases, injection of

the fistulous tracks may be employed every second, third, fourth, or fifth day, according to the amount of inflammation produced, the patient then being allowed to repose awhile. In very obstinate cases, the injections may be made, as in veterinary practice, daily, and continued for months, if required. In all cases, however, they must be suspended awhile if too great inflammation be excited; and, in some cases, when the wound is not deep, or filled with fungosities, or when the caries is not easily accessible, some lint, soaked in the *Liqueur*, may be applied instead of using the injections. Generally the injections give rise to pretty severe pain, which may last for several hours, or even all day; but usually, though not always, after some days it becomes easily supportable. The first injections give rise to sharp inflammation in the fistulous tracks; but the suppuration, which is temporarily increased, soon diminishes. Small osseous fragments are often detached after injection; and M. Nélaton has several times observed membranous shreds, analogous to the false membranes in the horse, coming away.—*L'Union Médicale* and *B. & F. Med.-Chir. Review*.

HOW TO MAKE GUTTA-PERCHA SPLINTS. BY EDWIN TRUMAN.

No material presents so many advantages for surgical purposes, with so few drawbacks, as gutta-percha; and a very little experience will enable all surgeons to use it perfectly. In a periodical of this year this passage occurs: "After a good deal of hard work I succeeded in thinning down the substance to the required thickness." The writer is describing the production of a gutta-percha splint for putting up a fractured jaw. It seems strange to hear so kindly a substance so spoken of—dough is not more easily moulded and flattened into sheets in the hand of the cook than gutta-percha might be—and by the same means a paste-board and rolling-pin which would have despatched this "hard work" in a few seconds, and produced a sheet of any thickness or size, perfectly even and ready for the splint. No matter in what form the gutta-percha is, nor in how many small threads or large balls,—place it in a vessel of water, and boil it till it is perfectly softened throughout; now collect it into one ball; dip it in cold water to enable it to be handled without burning, and knead it into a uniform mass with the hands. To do this, if it is very good and tough, will require some strength and also warming two or three times; but eventually it will form a uniform plastic mass not much harder than stiff putty. When in this state, place it on any flat surface which is wet, and with a roller—a common glass rolling-pin dipped in water is the best—roll it into a sheet of the desired thickness, let the roller slide upon it in all directions, and continue this action till the surface is uniform; it will now be fit for cutting into shape. This can readily be done either when hot or cold with a pair of scissors, and no force is required to mould it into the form of splint required, the only precaution requisite being to hold it steadily in that shape till it is cold; after which it will not alter unless again made hot. It may sometimes be requisite to join two pieces of gutta-percha together. This is readily done by warming the two surfaces to be united by dry heat, with a hot iron, not burning, but merely softening the surface. Two such hot, dry, soft, clean surfaces will unite most perfectly when brought into contact with each other, if held together till they are cold.

It may sometimes also be useful to know how two pieces of this material may be moulded on to each other without sticking. If one be cold, and the other hot, they will not unite, especially if the cold surface be anointed with soap-suds.

It is, I believe, the custom in some of our London hospitals to look upon an old gutta-percha splint or one which has aborted, so to say, in the making, as useless and only fit to be sold as old material, thus necessitating a considerable loss. If the above-described process were employed, no small saving might be effected, as the same piece of gutta-percha might be used over and over again.—*Archives of Dentistry*.

STATISTICS OF HERNIA.

THIS is an account by Dr Scholz of the cases of hernia which have come under the care of Professor Lorinser during the last twenty years at the Vienna General Hospital. They are not very numerous; but having been minutely analyzed, they yield some deductions worthy of record.

The entire number of cases admitted during 1844-1863 was 422, viz., 220 males and 202 females; 268 (215 males, 53 females) being examples of inguinal; 142 (4 males, 138 females) of femoral; and 11 (1 male and 10 females) of umbilical hernia. There was also 1 case of hernia of the foramen ovale in a female. The right side was affected in 246 cases (163 inguinal, 83 femoral), and the left in 156 (97 inguinal, 59 femoral). With respect to age, it was found that the number of cases was twice as large during the decennium 20 to 30 as during that of 10 to 20. During the three decennia 20 to 50 it continued nearly the same; reached its maximum during that of 50 to 60, and then gradually diminished. Before the age of 40, the males were in largest proportion (113 to 46); but after that age the proportions were reversed, there being 156 females to 107 males. Of the 422 cases of hernia, 247 (109 males, 138 females) were treated for strangulation; 192 (77·4 per cent.) recovering, and 55 (22·6 per cent.) dying. In 12 of the cases the treatment was by position (*lagerung*), in 119 by taxis, and in 116 (60 recoveries, 56 deaths) by operation—39 cases having been also brought in in too advanced a state to admit of the operation being performed. In 104 cases of inguinal hernia in males there were 37 operations, in 32 cases in females 10 operations; and in 4 cases of femoral hernia in males there was 1 operation, in 95 cases in females 65 operations. The cases were fatal after operation at the rate of 40 per cent. for femoral hernia in females, 51 per cent. for inguinal hernia in males, and 70 per cent. for inguinal hernia in females. The duration of the strangulation averaged for the 247 cases 2·1 days per case, *i.e.* 1·88 day for males, 2·27 days for females. In inguinal, the duration was 1·79 day (1·87 males, 1·5 females), and in femoral, 2·50 days (2·0 males, 2·5 females). Comparing the successful with the fatal cases, in general it was found that the duration in the former was 1·7 day, in the latter, 3·1 days. In those who were not operated upon, the duration was 1·3 day compared to that of 2·8 days in those who were operated upon. In recoveries after operation, the strangulation had lasted 2·5 days, in fatal cases 3·1 days. Of the 116 cases operated upon, the contents of the sac was intestine alone in 90 (49 recoveries, 41 deaths), omentum alone in 2 cases, both recovering, and omentum with intestine in 24 (9 recoveries, 15 deaths). In males, intestine alone was found in 26 cases, or 29 per cent.; in females, in 64 cases, or 61 per cent. In inguinal hernia intestine alone occurred twice as frequently in males as omentum and intestine, and exclusively in females; while in femoral hernia in females it occurred 4 times as frequently as omentum and intestine. In cases of intestine alone in inguinal hernia, 42 per cent. of the males, and 70 per cent. of the females died; and in femoral hernia, 45 per cent. of the females; while, when omentum and intestine were present, the deaths of the males were 66 per cent. in inguinal hernia, and of the females, 50 per cent. in femoral hernia. As to the issue of the 116 operations, there were 56 recoveries, and 60 deaths. Of the 37 males affected with inguinal hernia, 18 recovered, and 19 died; and of the 10 females, 3 recovered, and 7 died. Of the 66 females, the subjects of femoral hernia, 39 recovered, and 26 died, the only male dying. Thus, 19 per cent. more females than males operated upon for inguinal hernia died, and about 11 per cent. more male subjects of inguinal hernia died than did female subjects of femoral hernia. As to the ages of the persons operated upon, 29 were under forty (14 recovering and 15 dying), 53 between forty and sixty (32 recovering, 21 dying), and 34 above sixty (14 recovering and 20 dying). The immediate cause of death was peritonitis in 26 cases, and intestinal gangrene in 17.—*Wien Medicinische Wochenschrift* and *B. & F. Med.-Chir. Review*.

MIDWIFERY.

CASE OF EMBOLISM OF THE PULMONARY ARTERY AFTER OVARIOTOMY.

BY E. PARSON, M.D. LOND.

THE specimen I now show will illustrate fibrinous deposition in both cavities of the right side of the heart. The deposit is seen to be firmly adherent to the greater part of the wall of the right auricle, and particularly so in the auricular appendage, where the deposit seems to have begun; for here the clot almost fills that portion of the auricle, and is accurately moulded to the little muscular eminences. The clot is seen to be continued through the tricuspid valve into the right ventricle, and where it passes through this opening almost filling it; in the right ventricle it is found to be intricately interlaced with the chordæ tendinæ, it being impossible to separate their mechanical adhesions to the columnæ carnæ. As the clot extended along the pulmonary artery to the lungs, it was found to be *tubular*, and adherent to one of the semilunar valves of the pulmonary artery; I therefore concluded that the clot was *ante-mortem*, and materially assisted in shortening the life of the patient.

The following is a short history of the case:—

A young woman, æt. about 20, underwent ovariectomy in the Samaritan Hospital, by Mr Spencer Wells, on December 14th, 1864. She had been moderately healthy before, and the operation was quickly and easily performed, the cyst being almost unilocular, the wound in the parietes being the usual short one, such as Mr Wells usually practises in cases of simple cyst. The pedicle was long and thin, and easily secured, as usual, by an ordinary clamp. She recovered very well from the chloroform, and remained comparatively well during the first twenty hours after the operation. About this time the pulse rose to 120, the tongue became unusually loaded, and she became also very thirsty—much more so than ovariectomy cases generally are.

On the second day the pulse rose to 140; she complained of scarcely any pain, vomited, and there was a peculiar look of oppression in the countenance.

On the third day this oppression greatly increased; the pulse 140 to 150; the urine albuminous and scanty; heart sounds indistinct and muffled, and a peculiar dusky hue about the face; the abdomen a little tympanitic; and it seemed evident that death would result, and that fibrine was clotting in the heart (which in a former case was not relieved by any treatment, including ammonia); and as it is often found that oppression is much relieved by the appearance of the menses or bleeding from the arm, Mr Wells determined to bleed to five ounces from the arm, and to give dilute hydrochloric acid every two hours. The blood was very dark, and rather thicker than usual, but was not markedly buffed afterwards.

On the fourth day she became still more oppressed, and very restless; vomiting continued; a great difficulty in keeping her warm, the surface of the body becoming white and cold; and she died at 11 A.M. on the 18th December 1864.

On *post-mortem* examination the abdominal wound was found not to have united, and looked sloughy on its peritoneal aspect. There was diffuse inflammation of the peritoneum, of the low form, attended with a few ounces of sero-purulent fluid in its cavity; but little lymph found anywhere. The pedicle found to have been well secured. The heart was normal, except the fibrinous deposits and some blackish blood in the right side; the left side empty.

I beg to suggest the following as the probable train of events in the case. First, the low form of peritonitis setting in after the operation, which generated some poison, which got absorbed into the general circulation, and that this caused, on the first and second days, the intense thirst and loading of the tongue. That on the second day the peculiar oppression, faltering pulse, and feeble heart sounds, indicated fibrinous deposit commencing in the heart, which was better shown on the third day. I beg to remark how utterly hope-

less are those cases, and how painful it is to have to look on whilst the separation of fibrine from the blood is killing the patient by filling the right heart; and further remark that she died so slowly because that portion of the clot in the pulmonary artery was hollow, and allowed the blood, for a time to get into the lungs; and which contrasted with a former case in the Samaritan Hospital, where the patient died more rapidly, and became blue, and almost livid, before death; that this case now presented might have been diagnosed to have been a *tubular* clot, from the absence of blueness and the slowness of death; and, lastly, if now those who have helped us to the pathology and diagnosis of those cases could inform the Fellows of this Society how to *prevent* those deposits, or how to remove them when deposited, they would confer a lasting boon on us as obstetricians and those who are called upon to treat the diseases of women.

Mr Spencer Wells said he believed the peritonitis in this case to have been septic—due to the hospital atmosphere, which at this time was injuriously affected by two other patients suffering from erysipelas and pyæmic fever. His chief object in the bleeding was to give mechanical relief to the heart, and lessen congestion of the kidneys. Relief to the breathing was afforded, and the urine became more abundant after the bleeding, and was less loaded with albumen.

Dr Richardson said he regretted that *Dr Parson*, in his able report of the case, had used the word “embolism;” he regretted this because the fibrous generation in the heart that was before the Society was, in truth, not an embolus, according to the strict use of that term; and he regretted it also because the term ought never to be used at all. “Embolism” and “thrombosis” were, in fact, absurd words, and they were doing great harm, for they led the mind away from the pathological fact to mere terms that had no meaning at the best, and that were constantly misconstrued. At the same time he (*Dr Richardson*) was very happy to see the subject of fibrous deposition receiving at last the attention it deserved. When, fifteen years ago, he revived this question (for, in truth, the question is only a revival of the labours of *Chisholm*, *Gould*, and other writers of the two preceding centuries), he was considered as putting forth false notions in pathology, and it was boldly averred that these concretions were always found *after* death. Now a more correct knowledge prevailed. *Dr Richardson* then spoke in reference to the concretion itself, which he said was one of the finest specimens he had seen of a tubular concretion; such a concretion would give rise to a very slow death, extending over two or three days. He next described the symptoms of concretion in the right side of the heart, and stated the diagnosis to be so plain that the precise position of the clot in the cavity could be determined before death. Regarding the cause of deposition, the view he had formerly held, that these concretions were not produced by the introduction of septic poisons, but were of inflammatory origin, must be modified. He had learnt by experiment that fibrine might separate in the blood, not only in consequence of a direct increase of it, but by introducing with the mass of blood decomposing substances. Great obscurity, however, still surrounded the inquiry, for the same poison that in one experiment would render the blood of an animal universally fluid would in another experiment lead to deposition of fibrine. He could attribute this fact but to one circumstance, viz., that the condition of the air, the meteorological condition at the time of infection, was the determining cause of the course of degeneration of blood that would ensue. At present, in cases of ovariectomy where there was absorption of morbid matter the patients died, either from ammoniacal degeneration of the blood or from separation of fibrine; but why this distinction should exist remains to be shown. Treatment, when signs of concretion were presented, is most difficult; we might try to dissolve the concretion by alkalies; but unfortunately all the alkalies dissolved also the blood corpuscles, and in a clear case of concretion where he had pushed the alkalies to the extent of affecting the concretion, this event had happened. Some years ago he had put the question forward, whether a concretion could be *mechan-*

ically removed from the heart by the veins. In one case, where a concretion had been artificially induced in a cat, he had actually removed it from the right heart by a crotchet needle passed down the innominate vein, as he had thought that *in extremis* something like this might be done in the human subject, or that the concretion, if not removed, might be broken up; but even if these objects could be effected there would be danger that the concretion would re-form, as the conditions that gave rise to the deposit would remain. The remedy really to be looked for was a substance that would rapidly dissolve fibrine, and would leave the blood-corpuscles intact; such a remedy injected into the veins might probably prove of service, but its discovery almost implied a paradox. One point of practice, however, was open to those who performed such operations as ovariectomy, and he (Dr Richardson) would very respectfully point it out for the first time. It was, that the surgeon should begin to inquire whether the external influences of heat, humidity, barometric pressure, and ozone, did or did not affect the results of operations. He would recommend that the exact meteorological conditions shortly before and shortly after days of operation should be kept, and it should be noted what periods were the most favourable to recovery and what most unfavourable. As these were cases that could wait for the proper period, it was possible the fatality might be lessened by a knowledge thus obtained by the operator when to perform and when not to perform the operation.—*London Obstetrical Society's Transactions.*

LABOUR IN PRIMIPAROUS WOMEN LATE IN THE REPRODUCTIVE PERIOD OF LIFE.

BY GEORGE ROPER, ESQ., M.R.C.S.

In common, I believe, with most obstetricians, I held the opinion that labours late in life were almost necessarily severe and difficult. The experience of several such labours, which were not only free from severity and difficulty, but which were positively easy, has led me to the conclusion that difficulty is the exception in these cases as a class. I take the age of forty-five. There is in subjects at this period of life, contrary to what is assumed, an absence of that rigidity and tenacity of structure which we meet with in those prior to the decline of the reproductive function. The soft parts about the outlet of the pelvis are thin, flabby, and elastic, from want of tone, and offer but a feeble resistance to the passage of the child; they afford a strong contrast to the firm, plump, tonic, and unyielding tissues of middle life.

In the primipara at the age of forty-five there is generally a peculiar lean and thin, and almost wasted, condition of the soft parts which line and close the outlet of the pelvis, in consequence of which the second stage of labour is rendered easy. Assuming the cervical portion of the uterus to be healthy, the first stage of labour is also free from difficulty. It is a clinical fact observed in these cases that the uterus acts with great vigour; its powerful contraction is remarkable as compared with the uterine inertia of the multipara at this period of life; the power of the one is good and has never been used, that of the other is worn out.

On looking over Dr Robert Lee's "Report of Eighty Forceps Cases of Difficult Labour," I find that 39 were primiparous cases; in 13 the age is not stated; of the 26 in which the age is recorded, 3 were in those under twenty-five years of age, and two over the age of forty; 21 cases occurred between the ages of twenty-five and forty. As by far the largest number of primiparous labours occur before the age of twenty-five, it is inferred from this experience that difficulties are rare under this age. Only 2 cases being recorded over forty years of age, it is concluded that difficulties at this period are not common, considerable allowance being made for the infrequency of first labours at this time of life; yet if difficulty were so essentially connected with labour at and above the age of forty-five, a larger number of cases might have been expected. As between the ages of twenty-five and forty we find 21 cases, this bears out my own experience, that cases of difficulty or severe labour in primiparous women most commonly occur at the mid period of the reproductive function, say at an age between twenty-eight and thirty-eight. The object of

these remarks is not to give an authoritative opinion, nor is it desired to elaborate from statistics what might be conclusive on this point, but rather for the purpose of eliciting the opinions of the Fellows of the Society on a question in which it is felt there is some error in regarding primiparous labours at an advanced period of life as almost necessarily difficult or severe.

The *President* observed that the paper was instructive in correcting a common belief that first labours in women at an advanced age were always severe and dangerous. He had seen many instances confirmatory of the author's proposition that these were often really easy. He had recently read a memoir by Dr Matthews Duncan, in which it was shown that the maximum weight of the fœtus was attained by women pregnant at the age of twenty-five to twenty-nine, and that the weight afterwards fell. If this were so, we had here a circumstance that would help to explain the more easy labours of women who had passed the age of forty.—*Ibid.*

ON THE COMBINATION OF CHOREA WITH PREGNANCY.

BY DR BATHURST WOODMAN.

THE concurrence of chorea with the pregnant condition is, I imagine, a not very frequent event. Certainly very little mention is made of it by obstetric authors, and as I have myself only met with two cases, I have been induced by our President, Dr Barnes, to lay before you the main facts of one case, in order to elicit further opinions and experience, with a view to both prognosis and treatment. The first case which attracted my attention to the subject, was an out-patient of Dr Down's, at the London Hospital, a primipara, in whom chorea supervened on a six weeks' pregnancy, from fright at seeing her husband as she thought killed by an accident. She soon recovered from the convulsive movements, under the use of arsenic and nourishing diet, but labour came on at eight months, and the child had a cleft palate, hare-lip, and webbed fingers. As Dr Down intends to publish full details of this case, I pass on to the more recent one, which is the subject of this paper, and which in some particulars affords a considerable contrast.

Caroline S., æt. 18, an engineer's wife, residing at Bow, was admitted an in-patient, under Dr Fraser's care, on the 3d January 1865, for acute chorea. She was then about six and a half months pregnant with her first child, and had had involuntary jerking movements, chiefly of the head and upper extremities, for nearly three months. Her mother states that she suffered only from measles and whooping-cough up to the age of seven, when she was first attacked by chorea, "she thinks from fright." From this she was never free for more than a month or two, till a little after she was thirteen, or in point of fact, till her first menstruation. At eight years old she had something like rheumatic fever, and soon after that, what her mother calls "typhus," but which was evidently "typhoid" fever. This was not till a full year after the first attack of chorea. She never had scarlet fever. From the age of puberty till her present seizure she was entirely free from choraic movements—an interval of five years. No reason is known for their recurrence. She is a thin, delicate person, small in figure, of light brown hair, and eyes of nearly the same colour; was anæmic, and much impressed with dread of impending danger, as she was told by some of her friends to expect convulsions in labour. She had a distinct systolic bruit, heard at the apex of the heart. Her bowels were regular, tongue clean, and appetite fair. The urine was of specific gravity 1020, and contained no albumen, but about two grains of sugar to the fluid ounce (a condition which, in common with Dr Goolden, I hold to be not unusual in chorea); in this instance, several independent tests were relied on for the detection of the sugar. The fœtal tic-tac was only heard once or twice, owing to the unusual loudness of the placental bruit, which was heard all over the abdomen anteriorly, especially below the navel. (It was afterwards found that the placenta, which was very small, was attached to the uterus anteriorly, within the "dangerous zone," but not amounting to placenta previa.) She was first treated by steel, and afterwards (by Dr Barnes) with the diphosphate of

zinc and iron, but, in conformity to the too severe rules of the hospital, was made an out-patient at a fortnight's end, at which time Fowler's solution was given to her.

About the end of February the chorea quite left her. On March 22d, her mother came to me in great alarm, stating that the patient had been in labour twenty-one hours, and that the midwife thought there was "something wrong." I accordingly attended, and found the child's head, which was rather more than usually developed as regards bone, tightly wedged in the inlet of the pelvis, and presenting in the first position (of Naegelé). The os uteri was well dilated, but in spite of strong pains, no progress was made, and I therefore pushed up the head a little, introduced Dr Barnes' long forceps, and in twenty-five minutes delivered her of a full-sized, perfectly formed, and living female child, whose only imperfection, if it be one, was a little "tongue-tie."

She flooded a little the first two days, but had no choraic movements till four days after labour, when there were slight twitchings of the right hand, which she states "is weaker than the other." These movements only lasted three days, and she has now apparently recovered well, in spite of sore nipples, which threatened mammary abscess, but succumbed to tannic acid. The child has had no chorea yet. The mother has none now, but the mitral bruit persists, and she is still rather anæmic. The points of interest in the case are, I think, the following. First, the priority of the chorea to the rheumatism; next, its disappearance when puberty became established; its reappearance before she "quickened;" its recession at the end of the eighth month of pregnancy, and the slight renewal of the movements after the flooding consequent on labour.

I confess, that for myself, seeing as I do about 200 cases of chorea amongst in- and out-patients at the London Hospital in the course of the year, and finding the majority badly nourished, and anæmic, I am inclined to believe that these movements are chiefly reflex, and connected with the sympathetic nervous system. And notwithstanding the favourable issue of this case, so far, I think, the complication of chorea with pregnancy is an event of itself calculated to cause anxiety, and to call for prompt measures to remove all sources of irritation, and to improve, as far as lies in our power, the general nutrition of the patient.

I have since heard of another case of four to five months in a primipara, which ceased, under tonics, a month before delivery. She had no unusual symptoms. (Dr Lever published five cases in "Guy's Hospital Reports," vol. v.)

The case detailed at length in my paper has since had a subacute abscess in each breast, but no return of the chorea. She and the child are both doing well.—*Ibid.*

USE OF THE ACTUAL CAUTERY IN OVARIOTOMY. BY MR BAKER BROWN.

THE author observed that, on Feb. 1, 1865, he placed before the Society his first case of completed ovariectomy in which the pedicle was divided by the actual cautery. Since then he had published ten or eleven more in *The Lancet*; and now he wished to relate his last eleven cases, and make some remarks on the use of the actual cautery. In the eleven cases the operation was completed, and all recovered. Mr Baker Brown found that, in a few of the cases, the cautery was not sufficient alone, he having to ligature several vessels in the adhesions; and in Case 6, the artery, which was bleeding, was from a very fat mesentery, and the superabundance of fat prevented the seared edge from puckering. He considered it highly necessary to have a properly made clamp; also, it was advisable that the iron should not be too hot, a simple red heat being best, so as not to hurry the process of separation, but to bruise the pedicle by cutting it off slowly, and afterwards care must be taken not to disturb the stump. On one occasion, the author being anxious to see if the vessels were safe (after the cautery), gently rubbed the edges with a towel, when the crust was broken, and a small vessel bled. Of twenty-three cases of completed ovariectomy, the author had lost but two. He thought it

must appear evident that this success had arisen from the use of the actual cautery.

Mr Brown then presented a cyst and pieces of omentum from a patient operated on the day before, which showed that the whole omentum was so fat that the cautery would not act safely on the adhesions. In such cases it was better not to attempt its use. The pedicle itself was safely removed by the cautery clamp.—*Lond. Obstet. Soc.'s Proceedings, in Lancet.*

A CASE OF FIBRO-CYSTIC DISEASE OF THE UTERUS WHICH WAS MISTAKEN FOR OVARIAN DISEASE. BY DR ROUTH.

It occurred in a woman aged twenty. There were two large abdominal fluctuating tumours united by a large solid mass. These were diagnosed as multilocular ovarian disease by Dr Savage, Dr Greenhalgh, Sir W. Fergusson, and Dr Routh. All advised operation except Dr Greenhalgh, who recommended delay. She was operated upon on 16th November 1865. There were no adhesions of any consequence. The tumour was freely movable on the right side. On the left, it was bound down by the usual ligaments, and had become very dense and unyielding. Extirpation was thus impossible. Portions of the omentum were removed by actual cautery. The larger right cyst was punctured, and emptied of a quantity of pus, and the abdominal wound brought together. The patient scarcely rallied, was constantly sick, and died, after evidence of internal bleeding, in thirty-four hours.

The post-mortem examination revealed that all the cysts were full of blood, vessels having given way in them, probably through the vomiting. The actually cauterized surfaces were quite shrivelled, and free from even an appearance of blood. The whole fundus of the uterus was fibro-cystic.

The importance of this case was brought out in the error of diagnosis. All the usual symptoms of fibroid disease were absent,—namely, early fibroid hardness of tumour; absence of flooding; increased length of uterine cavity; absence of tubular soufflé: and previous exploratory puncture would have only given exit to pus. Secondly: Owing to numerous vascular trabeculae in the tumour, death might at any time have resulted from simple puncture with the trocar. Thirdly: It proved the great efficacy of the actual cautery, even in a case of constant sickness following operation.

Mr *Spencer Wells* said he fully admitted that in some rare cases it was very difficult, perhaps impossible, to arrive at an exact or positive diagnosis as to the nature of an abdominal tumour; but he contended that in the great majority of cases—in at least nine out of ten—it was quite possible to arrive as near the truth in this as in any other department of surgery. He admitted most fully that men of great experience, after carefully examining a case, would sometimes find it impossible, even with the help of an exploratory incision, to make a positive and complete diagnosis. In such a case as that narrated by Dr Routh, the exact nature of a tumour might be doubtful even after its removal. But such cases were few and far between, and by no means opposed to the rule that in a large majority of cases of ovarian and uterine disease a very accurate diagnosis might be made. With regard to the use of the cautery in ovariectomy, the cases brought forward certainly proved that we had been taught an additional and successful mode of dealing with the pedicle. But they also convinced him that it should be an exceptional and not a general method. The number of cases in which ligatures had been also required, the accounts of the melting and flaming fat, the care required as to the precise temperature of the irons, the slowness of the searing, the effects of disturbing the eschar, and the very frequent suppuration in the abdominal wall, were very unsatisfactory. And the argument that because out of twenty-three cases only two had died, therefore this success was due to the use of the cautery, was clearly untenable; for it was a curious fact that of the last twenty-two cases of ovariectomy in which he (Mr Wells) had been able to secure the pedicle by a clamp, only two had died, and one of these lived twelve days and died of cancer of the peritoneum, which was not detected before the operation.—*Ibid.*

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLV.—MEETING VII.

4th April 1866.—Dr MOIR, *President of the Society*, in the Chair.

I. GORDON'S SPLINTS FOR FRACTURE OF RADIUS.

Dr Watson showed Gordon's splints for the treatment of fracture of the lower end of the radius in the situation commonly called Colles' fracture. He stated, his experience in the treatment of this form of fracture by the various ingenious expedients which from time to time had been recommended, had made him very sceptical of any plan of treatment affording a completely satisfactory result in cases where there was well-marked displacement and deformity to begin with. The best results it had seemed to him had been obtained by the use of a pasteboard splint softened in hot water, and applied along the flexor aspect of the forearm and palm, together with a wooden or Gooch splint applied along the extensor aspect of the forearm, carpal, and metacarpal regions. The method of treatment by means of two rigid splints applied on the flexor and extensor aspects of the hand, wrist, and forearm had appeared to him to afford the very worst possible results. How this came to pass might easily be understood, if a straight splint were placed upon the flexor aspect of the forearm and carpal region of an uninjured arm, when it would at once be seen that the whole pressure of the splint acted on two points, viz., the upper part of the forearm and the carpal region. In the application of splints to the forearm and hand, so long as the radius remained entire this was a very immaterial matter, which might be overcome by a little padding judiciously applied between the splint and the lower third of the radius; but when the lower portion of the radius was detached from the shaft, and tended to be displaced towards the extensor aspect of the forearm, while the lower end of the shaft was carried into the position of pronation and bulged forwards the soft parts on the flexor aspect of the forearm at the site of the injury, the action of such a straight and nearly rigid splint must be to force the lower end of the radius which remained attached to the carpus backwards, and in proportion to the firmness with which the bandages were applied to displace the shaft of the bone forwards. The ill effects of this plan of treatment were manifested in cases where the bone was broken, but where at the time of the injury no displacement existed. Such splints having been applied, at the end of six weeks a well-marked deformity was found to exist,—a deformity produced, Dr Watson thought, by the apparatus rather than the original accident. The employment of the pistol splint of Nélaton, or of the angular silver splint of Dupuytren and Sir A. Cooper, or of the various ingenious modifications of these forms of apparatus, failed to procure any better result. This general condemnation was not, however, so justly applicable in theory to the method of treatment recommended by Professor Faugér, of Copenhagen, when the forearm and hand are laid over a double inclined plane, and the hands thus brought into a position of forced flexion; nor to the splints recommended by Dr Hamilton, of Buffalo, in his work on Fractures and Dislocations. These methods have not the positive advantage which the apparatus of Dr Gordon of Belfast, undoubtedly presents to the practical surgeon.

These splints consist of a flexor splint for each arm, and a single extensor splint suited to be employed with either. The latter is a straight, narrow, rigid splint, which should not be broader than the bones at the wrist, and extend from the flexure of the forearm to the metacarpal base. The other splint should correspond generally to the width of the flexor aspect of the forearm both above and below, and it is recognised as the one suited for the right or left arm respectively by the *wooden ridge* or pad, which is screwed to and projects from the radial margin of the splint. In adapting these splints to the limb, the flexor one is first applied, and the rounded end of the wooden pad is adjusted to the hollow of the lower third of the flexor aspect radius, having been previously covered with padding. The dorsal splint similarly padded is then applied, and the two are kept in position by two short leather buckling straps. The forearm is thus kept in the attitude of three-quarters pronation, and as the splint upon the flexor aspect stops short upon the level of the base of the styloid process of the radius, the hand hangs over its extremity in the posture of passive flexion and adduction.

The forearm may either be carried in a sling which leaves the hand unsupported, or when the patient is at rest it may be laid upon a table of appropriate height, the hand depending over the margin.

The only serious objection which can be alleged against this method of treatment is, that it requires a special apparatus. This difficulty can, however, be more readily overcome than might at first sight appear possible.

A short Gooch splint extending in length from the bend of the forearm to the base of the styloid process of the radius, and in width a little more than the breadth of the forearm at the wrist, will, when padded with layers of flannel upon its radial border, serve the office of the flexor splint; while a Gooch splint the same width as the forearm at the wrist, extending from the flexure of the elbow to the base of the metacarpus, will supply the place of the extensor splint in Gordon's apparatus.

Dr Watson stated, by the use of these methods of treatment he had succeeded so well in preventing all deformity, that he had no hesitation in saying, by their means the ordinary painful and vexatious results of fracture of the lower end of the radius might easily be avoided. Thus, after union was completed and the splints removed, the play of the fingers, hand, and wrist, would be found uninterrupted, adduction easily effected, while there was no projection of the lower end of the radius and carpus backwards, or of the flexor muscles above the site of the fracture forward. Neither did the projection of the ulna show itself to the inner or flexor aspect of the forearm,—deformities with which every practical surgeon must be acquainted, as more or less seriously marring the results of the plans of treatment at present in vogue.

II. HYDATID CYST OF LIVER.

Dr Grainger Stewart exhibited an hydatid cyst of the liver, of the size of a walnut, which contained a quantity of cheesy-looking matter, composed of fatty granules, and in which numerous heads of echinococci were embedded. It was found in the body of a woman who had died of syphilitic affections. It appeared to be in process of retrograde metamorphosis, and had led to no symptom during life. A small vascular nevus was shown, derived from the liver of the same subject.

III. SYPHILITIC DEPOSIT IN THE LIVER.

Dr Stewart also showed a series of specimens illustrating the changes undergone by syphilitic masses in the liver. In the first of the series there were masses of fibrous tissue of a slightly translucent appearance. In the second, the masses were of a more cheesy untransparent character. The third was characterized by an almost cretaceous appearance, with wasting of the tissues in the neighbourhood, and the formation of cicatrices. This form of syphilitic mass was quite different (*Dr G. Stewart* remarked) from the waxy syphilitic masses he had previously described and exhibited to the Society. The specimens were derived from a man who died from syphilitic disease, under *Dr Stewart's* care, in the Royal

Infirmity. A specimen of cirrhosis of the liver, associated with syphilitic masses, was also shown.

IV. STRICTURE OF THE DUODENUM.

Dr Stewart also showed a peculiar stricture of the duodenum, which had resulted from cicatrization of an ulcer. The patient had been under *Dr Sanders'* care. He was a sailor, 54 years of age; had, from boyhood, been subject to vomiting; had had various dyspeptic symptoms, which increased in severity for some years before his death. In October 1865, he had become so weak as to be obliged, for the most part, to keep his bed. He was admitted into the hospital in January of the present year. He had loss of appetite, and complained of pyrosis and uneasiness, but not pain after eating. These symptoms were followed, in about an hour, by vomiting. The death was due to exhaustion. On post-mortem examination, the only lesions found were in the duodenum and upper part of jejunum: here there was a series of strictures dependent upon cicatrization of ulcers. Several other simple ulcers were found in that neighbourhood. The narrowest stricture was of such a size as to permit the passage of a crow-quill.

V. PECULIAR CANCER OF THE STOMACH AND INTESTINE.

Dr Stewart also showed a peculiar form of cancer of the stomach and small intestine, associated with miner's lung, from a patient who had also been under the care of *Dr Sanders*. He had been healthy until the end of December 1865, when he became dyspeptic; had pyrosis and vomiting. His bowels became very loose. The evacuations were watery, tar-coloured, and offensive. He had also great dropsy, but no albuminuria. He died exhausted after gangrene, which followed upon incisions made to relieve the dropsy. The stomach was of natural size. At several points there was thickening of the sub-mucous cellular tissue. In the duodenum, and throughout a great part of the small intestines, there were numerous similar thickenings, mostly at the margins of the valvulae conniventes. Some of these had ulcerated, and at other points cicatrization, with deposit of pigment, had taken place. The glands were swollen and infiltrated, but neither in them nor in any of the thickenings were distinct cancer-cells to be found. There were very numerous large nuclei. *Dr G. Stewart* remarked that it was difficult to say positively what this lesion was. It certainly most closely resembled cancerous affections, differing from them only in its tendency to cicatrization.

VI. THE BRAIN IN CASES OF APHASIA.

Dr Take exhibited two specimens of brains bearing upon the question of aphasia.

First, The brain of an insane patient, presenting atrophy of the *right* external frontal convolution, without any history of aphasia. The patient was paralyzed on the left side for some years previous to her death, which resulted from apoplexy with paralysis of the left side. She was rather garrulous than otherwise. The following were the pathological appearances:—A. B., *æt.* 54. Post-mortem examination sixty-two hours after death. Scalp very thin. Calvarium half-an-inch thick; thicker on right side than left. Dura mater slightly adherent to calvarium over right frontal lobe. Arachnoid slightly adherent over course of sulci. On the left of the mesial line a thin diffused cyst was observed under the arachnoid. The *right external convolution* and a large portion of the orbital lobule atrophied, the space being occupied by serum contained in a cyst. Arteries, especially carotids, very atheromatous. The right middle cerebral artery more tortuous and atheromatous than left or any other vessel. Left lobe of cerebellum much atrophied, to the extent of one-third of its substance. Vessels of pia mater covered with molecular matter. 2 oz. sub-arachnoid fluid—2 oz. serum oozed from brain-substance.

Second, A brain in which the white substance of the anterior half of the frontal lobe had been destroyed by an apoplectic clot, which, having become absorbed, had left a cyst full of clear serum. On section of the grey matter,

it was found to be utterly disorganized,—a mere mass of molecular matter,—no nerve-cells. The grey matter of the left external frontal convolution was found in the same state. There was no aphasia in this case.

Dr Tuke also showed two specimens of atrophy of the cerebellum. In one case the cerebellum, with the exception of a small portion of the left lobe, was completely atrophied, the arbor vitæ was obliterated, and the organ was reduced to a semi-cartilaginous mass. On section, it was seen to be a mass of molecular matter. Nothing particular was observed during life. No. 2 was a curious specimen of a limited atrophy, in all probability congenital,—a small portion of each lobe inferiorly being absent.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXV.—MEETING VIII.

14th March 1866.—*Dr BURN*, *Vice-President*, in the Chair.

I. NOTES OF A CASE OF PUERPERAL CONVULSIONS. BY *DR CUTHBERT*.

The patient in the following case was confined with her first child at 7:30 P.M., on Thursday, 5th October 1865. There was nothing unusual in the labour itself to call forth any special remarks,—it was an ordinary primiparous labour; but in seven hours after delivery she was seized with a convulsion, which was followed by others at intervals of half an hour. I was sent for the following morning at 2-30, and arrived just as the third fit had begun. I found the patient suffering under a strong convulsion; her face was much contorted, swollen, and livid; her breathing was stertorous and puffing, with a good deal of froth issuing from between her clenched teeth; the skin was bedewed with a profuse perspiration, and the pulse was strong and rapid. I opened a vein in the arm; but in consequence of her violent struggles, I could only obtain a small quantity of blood. I now put her completely under the influence of chloroform; a dozen leeches were applied to the temples; her hair was cut out, and cold applied to the head. In about half an hour she awoke quietly, and perfectly conscious; knew her friends; talked about her own condition; and complained of a severe headache and pain in the lower part of the belly. As the leeches withdrew the blood she said her head felt better. By the time the last leech fell off the pulse had fallen to 100, and soft. The urine was tested and found a little albuminous.

At 5 o'clock, that is, two hours after the third fit, she took another one; but it was much less severe than the others. More chloroform was administered, which completely soothed her.

11 o'clock A.M.—There have been several fits since my last visit, and they have been quite controlled by chloroform. The patient has had two and a-half hours of sleep, and has also partaken of a little tea. Pulse 120, and pretty strong. To get a turpentine enema.

2-30 P.M.—There have been several convulsions since the forenoon visit. Head to be shaved, and ice to be applied to the scalp. Mustard poultices are also to be applied along the spine. To take 20 grains of acetate of potash dissolved in a little water every two hours. The injection has acted well.

8 P.M.—Patient sleeping. Has had several severe convulsions since three o'clock. A little chloroform which I left in charge of the husband was administered at each attack with good effect. To get a little essence of beef.

Oct. 7th, 11 A.M.—Has passed a pretty good night; two convulsions only have occurred since last visit, and they were not severe. When roused, the patient becomes quite sensible. Takes the essence of beef freely. Pulse 86.

8 o'clock P.M.—No convulsions since eleven o'clock. Lies quite composed and sensible. No headache nor abdominal pain; tongue clean; urine slightly albuminous, and contains an abundant deposit of the triple phosphates. Pulse 70.

9th.—Patient going on well; albumen almost gone; the crystals entirely

disappeared. Taking food with a relish, and suckling the baby. Bowels freely opened with castor oil. To take the medicine at longer intervals.

16th.—Convalescent.

The remarks which the case calls forth are the following:—There is the fact of the convulsions occurring so many hours after labour; more generally, they take place either before or during labour. The patient had, comparatively, an easy, and not a very long labour, and she went on quite well up to the time of the attack. I may also mention the unusual deposit of the triple phosphates, and their sudden disappearance.

Dr Ramsbotham is of opinion that convulsions occurring *after* labour are not so manageable as those taking place during any other period of the parturient condition; but I think the general opinion of the profession is the reverse. The worst cases being those where the convulsions occur *during* labour, and continue afterwards.

Then, as regards the treatment, the value of chloroform in puerperal convulsions is very well brought out here. The patient could always be controlled by it, and further attacks either warded off, or very much mitigated when they did occur. I think, also, great good was obtained from the acetate of potash. The bleeding from the arm can scarcely be said to have done any good; but, undoubtedly, the leeching gave the patient much relief; she expressed herself greatly benefited by it, and I think we are warranted in cases of this kind to bleed either from the arm, or, by means of leeches, from the temples.

Mr Pridie mentioned that he had seen three cases of puerperal convulsions in 1864. In two cases they occurred before delivery, and in one case after delivery, but all were first children; in each case the convulsions varied in their duration. The treatment followed in each case was bleeding from the arm, as much as thirty or forty ounces was taken in three operations from one of the patients. Acetate of potash (ʒi. to ʒviii. of water) and acetic acid were also prescribed. In one case, chloroform was administered. The patients all recovered, and have each since been again pregnant, and had living children.

Dr Pattison said that he had always observed a peculiar tallow complexion in patients suffering from puerperal convulsions.

Dr Keiller said that the case reported by Dr Cuthbert, and also those mentioned by Mr Pridie, were in several respects similar,—they occurred in first labours, and they exhibited the common features of uræmic origin. It was a matter of statistical observation that eclampsia or true puerperal convulsions were generally primiparae, by far the greater majority of cases—about 70 per cent.—occurring in first labours. The four cases referred to were those of married women who were not quite so liable as unmarried females are to be attacked by convulsions during parturition,—the probable explanation of this greater susceptibility in the latter being their comparatively mental inquietude. The expression of countenance referred to was that usually observed in albuminuria, and it ought to be not only recognised but treated so as to get rid of the puffiness or œdema of the face, etc., so characteristic of the pre-existing uræmia. He had seen and treated a variety of puerperal convulsion cases, and believed that the treatment ought to vary; some do quite well without the old “sheet anchor” of general bleeding; and many recover without recourse being had even to the local abstraction of blood. It required some discretion, and no small presence of mind, to meet each case aright, and *one* mode of treatment will be found unequal to all cases. Bloodletting was now too unpopular, but there can be no doubt of its necessity in many cases, especially of *congestive* eclampsia. He (Dr Keiller) cited his experience of the treatment of puerperal convulsions by the free use of the lancet, and mentioned the views he entertained regarding the influence of chloroform, which he first saw administered in a case long ago under his care in the Maternity Hospital, and which he had since frequently exhibited with the effect of diminishing the violence and frequency of the attack. The stage in which convulsions most frequently occurred varied; they occasionally began before labour set in, but most generally commenced during the dilatation of the os uteri or perineum, and comparatively seldom after the expulsion of the child, although they sometimes, as in the cases mentioned,

take place even some time subsequent to the completion of parturition. The child was very often lost in convulsion cases, and the necessity for hurrying the delivery was, therefore, often apparent.

Sir James Y. Simpson said that the blood in cases of puerperal convulsions was poisoned, and while bleeding did good by lessening cerebral congestion, as an effect of the fits, and perhaps by occasionally preventing a bloodvessel rupturing in the brain, it merely lessened the quantity, but did not remedy the poisoned quality of the blood. The late Dr Wilson, of Glasgow, was the first, he believed, to use chloroform in puerperal convulsions. He (*Sir James*) believed that in such cases with albuminuria the state of blood poisoning produces a supersensibility or superpolarity of the nervous system, and that chloroform undoubtedly reduces that condition. If we partially poison a rabbit with strychnine or brucine, and administer chloroform, it will be kept alive by it, when other animals of the same size which have received a similar dose perish by convulsions. A deep state of chloroform anæsthesia almost invariably kept at bay the attacks of puerperal convulsions.

II. CASE OF SPINA BIFIDA, AND REMARKS ON THE SURGICAL TREATMENT OF THE DISEASE. BY SIR JAMES Y. SIMPSON, BART.

Sir James Y. Simpson exhibited a large tumour covered with skin and containing a spina bifida, which he had removed on post-mortem examination from an infant six months old. For the last month or so, the tumour had been increasing so rapidly that it became necessary to make an attempt to check its growth, or leave the child to die. When he first saw this case he was in some doubt whether the tumour might not be in part the remains of a second fœtus, instead of a spina bifida; in other words, whether it was not an example, such as has now been described by many authors, of an undeveloped fetal body attached to the lower part of the spine of a fully-developed child. The medical gentleman who had originally watched the case in Ireland, and seen the tumour when it was yet very small, had not been able distinctly to feel that it had any connexion with the canal of the spinal column. Its base was nearly as broad as the whole width of the loins. When *Sir J. Y. Simpson* first saw it, its surface was covered over with thick cutaneous tissue; and hard points were felt here and there on the surface of the tumour. In all of these respects it resembled a case of secondary double fœtus, which he had seen many years ago, attached to the same part of the body, in the practice of Dr Paterson of Leith. In this case the tumour betimes partially shrunk; and the patient survived, and was now a healthy young man. Dr Paterson had seen the child whose case was at present detailed to the Society, and recognised the similarity of the tumour in external appearance to that which had been under his care. One symptom, however, seemed to *Sir J. Y. S.* to indicate the probability of its being a form of spina bifida, namely, the impulse which appeared to be given to the fluid within it when the child cried or struggled. To make the diagnosis more certain, by having an opportunity of examining the relations of the tumour when it was more collapsed, it was tapped at one or two points, according to *Sir Astley Cooper's* plan of treatment, by a slender exploring needle. Not much fluid escaped in the course of the next twenty hours. At that time some slight fits supervened, and it unexpectedly died in the course of some three or four hours subsequently. The child's head was of an unusual form, such as at first to suggest the idea that there might be chronic hydrocephalus, but, in answer, the mother pointed out the circumstance that her skull was of the same unusual type. On opening the body a quantity of fluid above what was normal was found in the ventricles. No lesions of any kind were found in the abdomen. The chest was not examined. The tumour was removed for more careful examination.

Dr Handyside kindly dissected the preparation, and had given the following description of it:—

“The tumour in this case consists of an elastic and fluctuating mass which protrudes from the lower lumbar region. It is of an oblate-spheroidal form; and its respective diameters are about three and a-half inches, by two and a-half. On being laid open, the walls of this compound fluid swelling are seen

to consist of very vascular integument, next of dense areolar and adipose tissues, and then of a subjacent capsule of white fibrous tissue lined with serous membrane; the last being continuous with the *dura mater* and *arachnoids* of the spinal canal, through the medium of a tubular process of these membranes one inch in length and of the diameter externally of a large goose-quill.

"More particularly,—the lining of this fluid tumour or sac is derived from the general serous cavity of the cord along the tubular passage referred to, and is evidently a prolongation of the parietal inflection of spinal *arachnoid*. This surface presents, at one or two points near the spine, small angular and abraded patches of a reddish-orange tint.

"Into this serous sac of the *arachnoid* there projects—stretching along the interior of its tubular prolongation—the neck of a small serous *ampulla*, which, within the sac itself, becomes dilated into a Florence-flask outline, and in size amounts to about one and a-half inches in length, and half an inch in breadth. This *ampulla*—a sac within a sac—quite insulated by serous fluid, and adherent only at its neck or origin, consists of thin firm serous texture, diaphanous, and covered by a layer of *arachnoid* derived from the visceral or medullary wall of that membrane; as is readily seen on blowing air and passing a large probe along its connecting tube or isthmus of communication. On the structure of this *ampulla* being similarly examined, its interior is found to communicate with the *sub-arachnoid* space of the spinal cord through an aperture that readily admits a small probe of one line in diameter.

"The walls of this *ampulla* consist accordingly of two layers of serous membrane—the *arachnoid* and the *sub-arachnoid*—connected *dos à dos* by intervening areolar tissue.¹

"On submitting to microscopic examination various portions of the tumour, serous and sub-serous, no traces of any abnormal element can be found. The reddish-orange spots, above noticed, are clearly due to increased vascularity and apparent softening. The adjacent tissues consist chiefly of white fibrous tissue with some yellow elastic fibres and large nucleated fat-cells.

"This dissection—revealing two separate and distinct serous sacs, an outer or *arachnoid*, and an inner or *sub-arachnoid*—affords countenance to the view, advanced I believe by Professor Sharpey of London, and which I have been since in the habit of teaching, *that the sub-arachnoid space is a distinct serous sac.*"

Many different ways of treating spina bifida had been attempted. Sir J. Y. Simpson had seen cases treated by pressure, by repeated tapping, by injections, by graduated compression applied to their base when pediculated; but none of the various cases which he had seen treated in these ways had been followed by successful results. The whole subject, however, was one which seemed to demand from modern surgery more attention than was given to it. The two most promising plans known at this moment were, *first*, iodine injections, especially in cases where the canal leading to the spinal canal was so small as to be capable of being obstructed; and, *secondly*, the division and partial removal of the tumour in proper cases by the knife, the peeling off, when necessary, of the opposite sides of the lining membrane, and bringing together the sides of the wound with metallic sutures, so as to produce a closed wound of soft and ultimately cicatricial tissue over the defective portion of the spine. He stated two cases where this plan of treatment had been followed in the country without apparently a correct diagnosis being made; and both the patients survived for many years. The late Sir William Newbigging had successfully used this form of operation—in one instance in the practice of Dr Hamilton. Sir J. Y. S. had tried the iodine injections repeatedly in two favourable instances, but ultimately both children took symptoms of inflammation of the nervous centres and died. He regretted that in the present instance he had not at once, instead of tapping the tumour, removed it, and closed the wound with metallic sutures, as he felt convinced that in all probability the child would have had a better chance of life than by adopting the practice of tapping as an intermediate and diagnostic measure.

Dr Henderson, of Leith, communicated the following notes of a case success-

¹ An illustrative woodcut will appear in next number of the Journal.

fully treated by him, but which he had never had an opportunity of bringing under the notice of the Society.—Seven or eight years ago, one of the members read a paper on the subject, and showed a specimen or two, at one of the meetings of the Society. A good many remarks were made upon its treatment, the different methods that had been recommended were passed in review, and a general regret was expressed that no very satisfactory treatment had been proposed. Sir J. Simpson thought that a plan he had to suggest would be more successful than any yet proposed, and that was to excise a large portion of the tumour, dissecting out the lining membrane of the portion left, down to the level of the spinal canal, and leaving only so much of the skin as would properly cover the opening into the canal, then closing the incision by the interrupted suture. A day or two afterwards a poor woman called upon me with a fine child, seven or eight months old, suffering from one of those tumours in the lumbo-sacral region, about the size of a small orange, to ascertain if anything could be done for it. I told her what Professor Simpson had said, and as the sac was beginning to get inflamed, I urged her to allow the operation to be performed immediately. About eleven P.M. she consented, and accompanied by Dr Struthers, I went to her house and operated as recommended, removing an elliptical portion of the tumour, then dissecting off the lining membrane down to the opening into the spinal canal, removing it on both sides, then introducing seven or eight needles, and with a silk thread closing the incision as in harelip. The cord was observed running down the canal, and was not touched during the operation. A good deal of blood was lost, to which I attributed a slight convulsion the child had at the termination of the operation, but it rallied very quickly after it. On the third and fourth day the needles were removed, and the incision was found to be healed completely by first intention. I saw the child occasionally for some months afterwards, during which time it continued to grow well, and was as lively and active in its movements as a child could be; it was the subject of slight talipes varus on both sides. The parents changed their residence and I lost sight of it. When it was about three years old, the house-surgeon of our hospital was called to attend it in an attack of scarlatina; and he informed me that it was keeping quite well, and could go round the room, holding on by the chairs and tables, or anything within its reach.

Dr Keiller had seen and treated a case not unlike that mentioned by Sir James Simpson. The locality of the tumour, however, was different, it being high up in the cervical region. The swelling was very large, and so broad at its base that it was difficult to make out, by external manipulation, whether or not it was really a true spina bifida. He opened it, and on passing his finger into the wound to ascertain its internal nature and connexion, a second tumour or sac was felt, which was evidently like the inner sac noticed in the case reported by Sir J. Simpson,—a spina bifida cyst, communicating with the spinal canal. The child died in convulsions, and an examination of the tumour proved its special nature. *Dr K.* had treated other cases by puncture, but had not witnessed any degree of ultimate success. One case he saw in a girl who had survived the condition to an unusual age, she being about ten or eleven years old when last seen. He (*Dr K.*) repeatedly punctured the tumour in this case, but beyond evacuating a quantity of serous fluid, and for the time being diminishing the bulk of the swelling, little advantage (but at the same time no apparent disadvantage) was observed. This case was also not unlike Sir J. Simpson's case, it being covered by thick cutaneous texture, and not, as is most frequently the case, merely covered by thin transparent tissue.

Dr Burn mentioned a case, where the sac ruptured before birth, and the child, which was small, lived only three days.

III. REMARKS ON LOCAL ANÆSTHESIA. BY SIR JAMES Y. SIMPSON.

This paper will appear in next number of this Journal.

Dr Pattison spoke of solid mercury as a local anæsthetic agent.

Dr Wilson was afraid of the local application causing sloughing.

Dr James Young said, in reference to two cases referred to by Sir James

Simpson, that the local anæsthesia had succeeded admirably in the patient from whose head they had removed two atheromatous tumours, as far as the prevention of pain was concerned during the operation; one of the wounds healed by first intention, but the other had become painful, and the skin blistered, probably from the anæsthetic having been too long continued. In reference to the case of the excision of the mammary tumour, the local anæsthetic did not succeed, probably from some fault in the ether used. Let me say, that the patient had gone out, and probably got cold, which might partly account for the local irritation of the head. Since the above was written, some other cases have been operated on, with complete success.

SESSION XXV.—MEETING IX.

March 28, 1866.—Dr CHARLES BELL, *Vice-President*, in the Chair.

I. RUPTURE OF THE UTERUS. BY DR GORDON, JUNIPER GREEN.

The case which I have the honour of bringing before the Society this evening, refers to the somewhat rare and very formidable accident, rupture of the uterus. The notes briefly are:—On the 25th of January last, about 11 o'clock at night, I was summoned to attend this poor woman in her confinement of her eighth child. The patient was a middle-sized, plump, healthy woman, thirty-three years of age. On my arrival at her residence, about 12 o'clock, I found she had been in labour three or four hours previously. Her pains were not frequent, recurring at intervals of about fifteen minutes, not by any means powerful, and had very little influence on the os uteri; she evidently, as she termed it herself, "was not bad enough yet." On making an examination, I found the head presenting, but high up above the brim of the pelvis, and the os dilated between a half-crown and five-shilling piece. Being some distance from home, I made up my mind to remain with my patient and watch the progress of the case, hoping that the uterus would soon begin to contract more powerfully and speedily terminate the case favourably. In this I was disappointed, as during the next two hours her pains increased but slightly either in force or frequency; the os had dilated a little, but the head descended none—seeming to rest somewhat against the pubic bone. By 3 o'clock in the morning, matters changed a little for the better, her pains became more frequent as well as stronger, and the occiput got relieved from its fixed position, took the axis of the pelvis, and began to descend. I hoped, now soon, to see a happy and favourable termination of the case. Lest, however, my hopes should again be disappointed, I immediately despatched for my instruments, expecting by the time the messenger could have returned that the head would be in a favourable position to enable me to apply the forceps if need be, and deliver the woman; but before the messenger returned, matters became worse, her pains became weaker and less frequent, and now for the first time she began to say that her pains were changed, that she had them more in her "belly than her back," but that they were not constant, "just the same kind of pain she had in her back." I examined with my hand over the abdomen; she complained of no tenderness whatever, and there was nothing unusual to be felt at the time. She continued having pains, but slight, and still in front; and to my annoyance the head began to recede instead of advance. I again laid my hand over the abdomen, and now found the uterus contracting with some distention of the abdomen, principally flatulence. Feeling convinced that something had gone wrong, and suspecting rupture of the uterus had taken place, I again and more carefully if possible examined the abdomen. There was now no longer room left to doubt as to the nature of the case, for the uterus was still contracting, and the extremities of the child were to be felt moving about in the cavity of the abdomen; and on making a vaginal examination, there was a slit to be felt in the posterior lip of the os; yet there was no unusual discharge per vaginam. Feeling myself at my wits' end, and being near Edinburgh, I at once rode into town, called on Dr Keiller, and stated my case to him. He, to whom I am much indebted, at once kindly offered to visit my patient with

me. On our arriving at her house, Dr Keiller carefully examined the patient, and satisfied himself of the nature and extent of the accident. He recommended to deliver by version, which operation he easily performed, except that there was a good deal of traction required in bringing the head over the brim of the pelvis. The child was still-born, and the mother survived the accident only thirty-six hours. There was a considerable prominence of the os pubis, as afterwards ascertained; but considering the child-bearing history of the patient, together with the nature of the labour, slow and powerless, I do not consider there was any urgent demand for more active measures being used in the earlier part of the labour. She had her first child when twenty years of age, a girl, still-born, no instrument used; the second, a boy, also dead, forceps used; the next three, girls, easy labours,—living children, and no instruments; the sixth, a boy, living child, forceps used; the seventh, a boy, still-born; and the eighth and last as already given. Considering this history of the case, I am inclined to think that the cause of the rupture was not so much the protuberance already referred to, but softening of the walls of the uterus consequent on repeated child-bearing.

Dr Keiller stated that when he was called to see the patient he found her in a state approaching that of collapse. On examination the contracted uterine tumour was lying forward, while the child had passed into the abdominal cavity through an opening in the posterior wall of the cervical portion of the uterus. The opening was still gaping, and on passing the hand through it he felt the head close to the rupture. The cord was not pulsating. The intestines of the mother were in contact with the limbs of the child. He easily delivered the woman by turning, but considerable traction was necessary to extract the head. There was no extension of the laceration during the somewhat forcible extraction. Dr K. thought that gastrotomy was a better procedure in some cases than turning, and more likely to diminish the mortality. Out of 60 reported cases of rupture of the uterus 57 were fatal, 53 being delivered by turning, forceps, or craniotomy, the other 7 were delivered naturally. Gastrotomy cases have been more successful. M. Dufeillay narrates 12 cases of gastrotomy, and only 1 fatal. Dr Winckel relates a case where the uterus was twice ruptured, and where gastrotomy was performed successfully on both occasions. When the child is turned and dragged back through the laceration, there is a risk to the mother from the retention of the discharges, as well as from hæmorrhage. As a general rule he would recommend turning when the rupture was at the cervix, and gastrotomy when in the body of the uterus. Dr K. mentioned one case which he had seen with Dr Moir, where the patient was delivered by gastrotomy, and rallied for some time. Dr Keiller read the following letter which he had received from a gentleman who had heard him refer to Dr Gordon's case immediately after its occurrence. The note appeared worthy of being read on the present occasion.

"In your lecture yesterday you spoke of a case of ruptured uterus,—the treatment pursued, the results of the treatment, and the plan of procedure you would adopt if another such case should occur. I thought you would not think it improper if I should narrate the following case which happened in the hands of Dr Vail, a practitioner in my native country, New Brunswick, and one of my neighbours. I may mention that Dr Vail is quite reliable in these matters, having had a very long experience, and having enjoyed a first-rate reputation in this particular branch of the profession. A woman, thirty-three years old, short stature, very fleshy, fell into labour at the termination of her third pregnancy. He examined, and found the head presenting, and proceeding regularly through the pelvic cavity. The pains were regular, of full but not dangerous force; there was not, he thought, the slightest reason to suspect any danger arising from the mere force of the pains. On introducing his finger to ascertain the condition of matters, he was surprised to be unable to find the head of the child. On carefully thinking over the matter he suspected the cause, and on introducing his hand and arm he felt two scroll-like bodies, smooth, which he suspected to be the walls of the ruptured uterus rolled up on themselves. He found the child lying among the mother's intestines, seized

a foot, and by the exercise of great force succeeded in drawing the child through the natural passages. The woman was of course very much exhausted, but by the use of gentle restoratives she rallied, and by the careful employment of opium, with good but easily digested diet, she finally came around. Of course, perfect rest and the other necessary adjuvants were particularly enjoined and observed. He informed me that no unfavourable symptom occurred until the eighth day, when she suddenly complained of pain and tenderness in the abdomen, but this was promptly controlled by hot fomentations and opium. She was able to walk about in five weeks from the occurrence of the accident."

Dr Henderson read an account of a similar case of rupture of the uterus. On the 13th January 1866, at 10 P.M., was called to attend Mrs B., thirty-four years of age, in her fourth confinement. Her previous health was very good. Her former labours had been tedious, but terminated naturally, except the last, in which, after forty-eight hours' suffering, she required the assistance of the forceps. The pains had commenced about 6 P.M., and had been increasing in frequency and force from that time; and, at the period of my arrival, were recurring every four or five minutes. I found the os uteri almost completely dilated, the head entering the brim of the pelvis, the vertex to the right front, the membranes entire. The pains continued steadily for about an hour, when the os being fully dilated the membranes ruptured. Little progress was made by the head; a slightly projecting sacral promontory apparently interfering with its advance. The pains continuing regular and strong, I left her at midnight to procure chloroform and forceps, intending to assist her, if necessary. On my return, about half-an-hour afterwards, I found her lying on her right side asleep, and learned that the pains had been gradually leaving her since my departure, and that they were now both weak and irregular. Waited about an hour with her in expectation of a change taking place, and was then called away to another case; on returning from which I found that she had been sleeping frequently between the pains, which were still feeble and at considerable intervals, and that she was fully persuaded that she would settle for the night. Thinking it probable that such would be the case, having given directions to be called upon the return of the expulsive pains, and considering that I might safely leave her under the care of the midwife, who had always attended her previously, but who had agreed on this occasion to act only as nurse, I was about to leave, when it occurred to me that it would be well to ascertain whether or not any change had taken place in the position of the head, and any advance been effected by the slight pains that had been going on between two and three hours. On making an examination after she had turned again on her left side, I was astonished to find the os uteri half closed, and the head quite out of reach. On passing the hand over the abdomen every part of the child could be distinctly felt. The pulse was 120, she was very pale, and expressed herself as very fatigued and sleepy. As there could be no doubt that rupture of the uterus had taken place, I immediately sent for my friend *Dr Paterson* to assist me, and on his arrival proceeded to turn. I had no difficulty in passing the hand into the uterus, and found that a rupture of its anterior wall had taken place, extending from a little above the os on the right side, nearly to the fundus on the left, the placenta being partially detached. The child was completely outside the uterus. I managed, without much difficulty, to get hold of the feet, and to extract a dead female child,—removing afterwards the placenta with the hand, and ascertaining that no portion of bowel remained in the opening. After being bandaged she expressed herself as quite comfortable. She was immediately put under the influence of opium. Everything went on well for three days, the pulse fell to 85, the bowels acted regularly. The lochia appeared rather scanty, and there was evidently a collection of fluid in the peritoneum. On the evening of the third day there was slight pain in the left side of the abdomen, and considerable tympanitis, and she began to vomit occasionally whatever she took. On the fourth these symptoms were a good deal relieved by blistering, chlorodyne, and tinct. mur. ferri, but her pulse began to rise, and to become weaker. On the fifth the vomiting

returned, her skin became jaundiced, and she sank in the evening. No post-mortem examination was obtained. This case shows that a most serious accident may happen without exhibiting any symptoms of its occurrence, or at least that the symptoms attending it may easily be overlooked; and it also warns us of the necessity of carefully watching every case, as I fear if I had left her she would have died of hæmorrhage undelivered. I am not at all satisfied that in this and similar cases turning is the best method of treatment. I have a strong impression, that if I had performed gastrotomy, sponging out the cavity of the abdomen, treating it as a case of ovariectomy, the patient would have been more likely to recover.

In reference to the case of Dr H.'s, *Dr Moir* saw no adequate cause to account for the rupture, unless it was weakness of the uterine walls.

Dr Pattison said that patients who had suffered from rupture of uterus, generally cried out from great pain in the belly, which was followed by collapse.

Dr Moir said, that among the many causes which had been suggested for rupture of the uterus, one was inordinate action, accompanied by excruciating and continuous pain in some particular spot of the uterine walls.

Dr Charles Bell said, that in cases of rupture of the uterus, delivery was generally effected through the rent; but he thought gastrotomy a desirable operation, in some instances, as it enabled the abdomen to be cleared out properly, and the wound nicely adjusted. *Dr Hamilton* used to think that one cause of rupture was the sharpness of the linea ilio-pectinea.

Dr Gordon, Edinburgh, asked if the children were ever saved by gastrotomy?

Dr McCowan and *Dr Moir* instanced cases where the children had been saved.

Dr Young stated that this accident is fortunately rare, not occurring probably oftener than once in eight or ten hundred cases. In the few instances he had seen, there existed considerable contraction of the brim of the pelvis, and the pains were of more than usual severity; or turning had been necessary to effect delivery, owing to hæmorrhage or some other cause; or much difficulty had been experienced in bringing the head through the contracted brim. There is no doubt it rarely occurs where the uterus is in a perfectly healthy state,—generally, he believed, a portion of the organ had undergone softening, or been thinned by the pressure of the child's head against the brim during the uterine contractions. In those cases which had come under his own observation, the age of the patient seemed to have had little or no influence. He had seen no case where it occurred by the natural expulsive efforts, however severe, where the head was low in the pelvis.

II. CASES OF PYÆMIA AFTER VACCINATION. BY DR WILSON.

I have thought it might not be uninteresting to the members of the Society to lay before them a short account of this very painful case, showing the possibility of a fatal result following the operation of vaccination, which most of us, I believe, are in the habit of considering a very trifling one, at least quite unattended with danger, and also with the view of ascertaining whether any of the members have ever met with any cases of a similar character.

The subjects of this case were twin brothers, aged five months when vaccination was performed. The children were apparently quite healthy previously, though one of them had suffered from a chest affection some time before, so severely as to appear to threaten life at the time. The other had suffered in a less degree. In consequence, they had been confined to the house since that time. They were both nursed by a healthy young woman, and had in addition two or three meals of Liebig's food, or corn-flour, etc., daily.

The first was vaccinated twenty-two days before death, from lymph considered to be quite healthy, taken from a tube. The other was vaccinated from his brother, and died on the seventeenth day after. The operation was performed by myself in both cases in the usual way. The inflammation attending the vaccination was described as considerable, but not greater than had been seen in the cases of some of the other children of the family. In one of them no lymph was taken off, as the vesicles had burst before being seen by me.

I was asked to visit them a week before their death. They were described as being dull and listless, disinclined to suck as much as usual, having slight coughs, feverish, and evidently pained on movement. At this time it was supposed they had taken cold during the irritation following the vaccination.

I did not see them again for three days. I found the symptoms had not abated. The feverishness and soreness of body still continued, and, in addition, the feet and ankles of the one last vaccinated were swollen, there was an erythematous spot the size of a shilling on the left ankle, and the hand of the non-vaccinated arm was also swollen. A rash was said to have shown itself the day before, but had speedily disappeared. There was also slight diarrhoea. Some of the urine was collected, and found to be uncoagulable by heat or nitric acid. I may state that the symptoms were not only more severe, but much more marked in the child last vaccinated, which was also the feeblest, and the disease in him ran a much shorter course, terminating fatally five days sooner than in the other, viz., on the seventeenth day after vaccination. There was a slight remission of the symptoms the day before death, but on the evening preceding it a severe attack of pain, apparently abdominal, came on, and an extensive eruption of petechiæ over the body made its appearance, and he gradually sunk, notwithstanding the free administration of brandy, which, indeed, had been given from the time I first saw them.

In the first child, as I have stated, the symptoms, though upon the whole of a similar character, were by no means so marked, and there was no eruption of petechiæ. At one time he was thought to be getting better, but the night before death symptoms of pneumonia appeared. A short time before death epistaxis took place, and also slight hæmorrhage from the vaccine crusts. I may also mention that the discharge from the vesicles in one, was described as having been offensive in smell.

The occurrence of such unusual symptoms under such circumstances naturally made me anxious to have a post-mortem examination, which was readily granted, and in this I had the able assistance of Dr Grainger Stewart, to whom I am indebted for the following notes of the dissections.

No. 1.—Petechial spots over the whole body. Two crusts on the left arm. Intestines somewhat distended. In the walls of the stomach and of the intestines similar small petechiæ. Pericardium contained considerable blood-stained serum. Heart natural. Blood well coagulated. Similar spots on endocardium. Left lung adherent; right not adherent. Liver natural. Spleen natural, slightly pulpy, and in some parts congested. Kidneys lobular; some extravasations. Urine taken from the bladder, coagulated by heat.

No. 2.—Rigor mortis considerable. A few petechiæ. Pericardium contained a little clear serum. Heart natural. Left lung, some petechiæ, not confined to the surface, which appeared to represent the first stage of secondary abscess. Right lung, similar hæmorrhagic masses, but none in the condition of abscess. Liver natural. Spleen large, not pulpy, natural. Left kidney natural, excepting a few hæmorrhagic masses. Right kidney similar to left. Intestines almost free from petechiæ.

That the fatal result in these two cases was referable to the vaccination is an impression I cannot divest myself of. For although I have seen pyæmia occur in two infants a few days after birth, in whom there was no operation beyond the simple cutting of the umbilical cord, still the fact that the children were apparently in good health previous to the vaccination, and the commencement of the symptoms occurring so immediately after the operation, point strongly in that direction. As to the precise mode in which the vaccination may have operated there is more room for doubt. That it was not referable to the mode of performing the operation I am quite clear, for this differed in no respect from the way in which I usually do so, and that it was not owing to undue interference afterwards is equally certain, for in one of them there was no attempt to take lymph. We are therefore driven to the conclusion that it was owing to some peculiarity in the lymph used, or in the subjects themselves. With regard to the lymph I may state that it was taken from a supply collected within the previous two months from children believed to be healthy, and all of whom, so

far as I am aware, are alive and well at the present time. Indeed, the only suspicious circumstance that I can recall is that there were a few blood-globules mixed with the lymph, which was quite transparent; but so little importance did I attach to this circumstance that I would not have hesitated to have vaccinated a member of my own family therewith. But it is quite possible that the action of the blood may have altered the constitution of the lymph, and if this was unfortunately the source of the mischief, it shows the possibility of introducing through the process of vaccination a poisonous fluid, and inducing a fatal disease, whilst the subject from whom that lymph may have been taken remains perfectly healthy. I need not say that the possibility of such a dreadful result will lead me to be more than ever careful in the selection of the lymph used. Of course, it may have been owing to some peculiarity in the children's constitution. To this circumstance, or to the state of health of children at the time or subsequent to the performance of the operation, I believe we are justified in attributing many of the unpleasant sequelæ, *e.g.*, eruptions, etc. As in a case we have occasionally seen, and which I believe is not very uncommon, where three children are vaccinated from the same arm and at the same time, in two of them the vaccination may be perfectly normal, but in the third it may present some anomaly. Had one of these cases stood by itself, I should have been inclined to ascribe it to that cause; but the one following the other leads me to fear that I may have been the unconscious instrument of introducing some poisonous agent, and so leading to this distressing result. But I shall be glad to have the opinion of the members of the Society on this point, and especially shall I be glad to learn whether any of them have met with any case of a similar character. Till their occurrence I acknowledge I should have been sceptical of the possibility almost of such a result taking place, never having met with anything of the kind before in a very extensive experience in vaccinating. The only fatal case I had previously met with being one of diffuse inflammation in a child vaccinated by a midwife. Though on reflection, perhaps, the wonder is, not that such an occurrence may happen, but that it does not do so oftener. I trust I need make no apology for having brought this subject before the Society.

Dr Ritchie gave the following particulars of a case of erysipelas following vaccination, which lately came under his observation at the Children's Hospital. He was obliged to *Dr Moffat*, the assistant to the extra physicians, for drawing up the case. *J. E.*, aged five months, a healthy, well-developed child, was vaccinated at the hospital dispensary on 10th February. The usual phenomena of successful vaccination presented themselves, and when brought back for inspection on the eighth day there was a well-formed vesicle, surrounded by an inflammatory ring, bright red, hard, and somewhat elevated. On the following day (the ninth) the redness and swelling had extended down the arm and forearm to the wrist, and the hand was swollen and oedematous. A day or two after the front of the chest became similarly affected, and from these the redness and swelling gradually extended to the right shoulder, and down the corresponding arm. The back was next affected, and then the lower limbs, and lastly each side of the face and head became involved. About three weeks elapsed from the time of the first appearance of the erysipelas on the vaccinated arm till its invasion of the face and scalp, and as the inflammation crept from one part of the surface to another, desquamation of the cuticle followed in its wake. No abscesses formed, but about the end of the second week attention was directed to a slightly elevated fluctuating swelling in the lower dorsal region, about the size of a crown piece. Throughout the whole course the constitutional symptoms were those of great prostration; the child refused the breast, was restless and irritable. Pulse weak and rapid; bowels constipated, and motions offensive; tongue dry and brown, and countenance of a pale dirty colour. Treatment consisted in dusting the affected parts with flour and enveloping in cotton wadding. Quinine, strong beef-tea, and wine were freely administered, but, notwithstanding, it sank and died on the 11th March. The child lived in the Cowgate, first flat of a stair, house consisting of one room, occupied by four persons in all, large and free from damp. As to the source of the vaccine virus

employed, nothing further is known than that it was obtained from some of the children attending the hospital dispensary, and it is very likely that others were vaccinated with matter from the same source, without any bad effects following. Dr Ritchie observed, there was no reason to believe that the lance used was the cause of the erysipelas. He considered it was due to constitutional causes.

Dr Stephenson had met with three cases similar to the one related by Dr Ritchie. The first was a child two months old, living in the Causewayside, which had been vaccinated at the Royal Dispensary two weeks previously. The vesicles were said to have been perfectly natural, and when seen, the areola had quite disappeared. The erysipelas first appeared on the arm vaccinated, below the elbow, the disease extending to the hand. The other arm was next affected, and the erysipelatous appearance had no sooner disappeared from it, than it showed itself on the one leg, then on the other,—passing afterwards up the back. The child survived the disappearance of the rash and swelling, but afterwards sunk from the debility resulting from the altered condition of the blood. The vaccination in this case, though no doubt a predisposing cause, cannot be blamed as the exciting one. In the other two cases the disease appeared as a concomitant of the vaccination. One of the children recovered under treatment at the Sick Children's Hospital. The other, from the neighbourhood of Leith, was too ill to be brought out, and the mother was recommended to call in a medical man, so that the result was unknown. Although there was no erysipelatous condition of the skin in the two cases related by Dr Wilson, the symptoms in them resembled very much the condition known in adults as pyæmia; and remembering the close relationship which exists between that disease and some bad forms of erysipelas, the occurrence of these cases, Dr Stephenson thought, pointed more to some atmospheric or anti-hygienic condition under which the children had been placed, than to any fault in the vaccine virus.

Dr Linton stated a case of the vaccination of a child, with two pustules, from which he re-vaccinated other three children—the day after a scarlatina rash manifested itself on the child—two took scarlet fever, and one had an inflammatory attack, with abscesses, but all recovered.

Dr Keiller remembered having vaccinated three young ladies (sisters) from the same vaccine matter, and all had erysipelas of the arm thereafter.

Dr Henderson said he had had several cases of erysipelas after vaccination; he used always the same lancet.

Dr Watson, of Mid-Caldor, said that in thirty cases of vaccination, he had seen four cases of erysipelas. Papular, pustular, and eczematous eruptions sometimes followed vaccination, and the lymph was frequently blamed.

Dr Charles Bell thought that the main cause of such results was atmospheric, arising from the locality, and not depending on the state of the matter. There was a great prejudice in England against vaccination, in consequence of its being frequently followed by bad results.

Dr Keiller said that deaths were so very rare after vaccination that the lymph had probably less to do with the deaths than epidemic influence.

III. CASE OF ANENCEPHALOUS FŒTUS. BY DR JAMES YOUNG.

I was summoned to a patient on the 17th of March last. When I saw her she was complaining of slight pains, although she told me that she did not expect her confinement for four or five weeks. On examination, I discovered the uterus reaching much higher than usual at eight months, while the os uteri could not be felt with the finger. Mrs S. informed me that a large quantity of water (liquor amnii) had come away that morning. In reply to my query, she also said that she had felt no fetal movements for seven or eight days. I recommended her to do nothing, but keep quiet, as she might, in the circumstances, expect her confinement within a day or two. I was, accordingly, sent for two days subsequently, about 9 A.M. On examination, I discovered that the first stage of labour was almost over, and that the presentation was somewhat unnatural. Something about the size of an orange was felt; certainly

it was not the breech. The quantity of liquor amnii was excessive, despite what was expelled two days previously. In half an hour the child was born, and presented the appearance as is seen in the preparation. Let me here state that I fortunately obtained the fœtus for preservation, that I might more carefully examine it at home. I have called it an anencephalous fœtus, which means a fœtus having no brain substance; but Dr Keiller says that every such fœtus possesses a remnant of brain matter. I might justly use the term anencephalotrophia in reference to this case, as that word means an atrophy, or a want of nourishment of the brain, which, although not much used, expresses better the condition of such a fœtus than anencephalous. In like manner we use the word anencephalohaemia, from *αν εγκεφαλος* and *αιμα*,—meaning, *defect of blood in the brain*. And, again, anencephaloneuria, want of nervous action in the brain,—from the same Greek words,—and *νευρον*, a nerve. The term anencephalotrophia as well as anencephalohaemia might both read ancephalotrophia and ancephalohaemia, in each case leaving out the intermediate preposition *ευ*. In this manner we would find the term ancephalotrophia reduced to a less uncouth form, and yet meaning the same as before,—namely, want of nourishment of the brain; as the other word cephalohaemia means an accumulation of blood in the brain, so ancephalohaemia would mean a want of blood in the brain, and so with many similar expressions. Let me just add that the usual accepted term anencephalous conveys a wrong meaning, and may be held as an error in phraseology; meaning as it does,—a brainless fœtus: while in all such, there is a remnant of cerebral matter. The term anencephalous, if used, might be rendered ancephalous; as the Greek word *κεφαλη* means *brain* as well as *head*.

Dr Cappie mentioned that he had met recently with a similar case. It was the patient's second confinement; the only difference being, that in his case the opening extended farther down the neck.

Dr Stephenson, in reference to the term anencephalotrophia, remarked that it was a new one to him, and certainly bore on its face a contradiction in terms. It assumed an atrophy of an organ which the first part of the word stated did not exist. It was a good specimen of the attempts which are becoming far too fashionable of coining big names for what might be expressed in plain English,—a custom against which the profession ought to protest. He thought that when medical men shut themselves up in their studies and conceived such words as anencephalotrophia, they certainly brought forth monsters.

Dr Watson, of Mid-Calder, thought the word anencephalotrophia a contradiction of terms, and should have been encephalotrophia.

Dr Keiller considered that all anencephalous fetuses had a remnant of brain substance, and that they were originally hydrocephalic which had become shrivelled after the rupture of the morbidly distended organ during its early formation.

Dr James Young said that he would not use the word anencephalotrophia as a title to the case, seeing its meaning was disputed. He stated, however, that although not familiar to many, it was, he believed, quite correct; meaning a want of nourishment of the brain matter. At the same time the expression might be altered to ancephalotrophia, leaving out the intermediate preposition, and using *κεφαλη* to mean, as it may, *brain* as well as *head*; but were it to be read as Dr Watson says,—encephalotrophia,—that would mean nourishment of the brain, just the reverse of my case.

IV. DISEASED PLACENTA.

Dr Wilson showed a specimen of morbid placenta, probably of syphilitic taint.

LUNACY ACTS (SCOTLAND) AMENDMENT BILL.

A BILL to amend the Acts relating to Lunacy in Scotland, and to make further provision for the care and treatment of lunatics, has been brought in by Government. The following are the most important provisions of the bill:—Clause IX. provides that any parochial board may direct any pauper lunatic (not having been committed as a dangerous lunatic), with whose maintenance it is

chargeable, and who is detained in an asylum, to be discharged therefrom; but that if the superintendent of the asylum makes a written representation that the lunatic is dangerous to himself or to others, it shall be lawful for the board to prohibit his discharge; and any inspector of the poor removing a pauper lunatic from an asylum or house against the written representation of the superintendent, without the sanction of the Board, shall be liable in a penalty not exceeding *ten pounds*. Clause XII. renders it compulsory in all persons in whose houses lunatics are temporarily placed for gain, to report their having such inmates within fourteen days. Clause XIV. allows the parochial board of any parish, or parishes, to assess themselves for the purpose of erecting an asylum for the care and treatment of all or any portion of the pauper lunatics of such parish or parishes. Clause XVI. is as follows:—"Every letter written by a patient in any asylum or house, and addressed to the Board or their secretary, or the Commissioners in Lunacy, or any of them, shall, unless special instructions to the contrary have been given by such commissioners, or any of them, be forwarded to its address unopened; and every letter from the Board or their secretary, or such commissioner or commissioners, to any such patient, when marked "private" on the cover, shall be delivered to him unopened; and every person who shall intercept, or detain, or shall open any such letter without the authority of the patient by whom it is written, or to whom it is addressed, shall be liable in a penalty not exceeding *ten pounds*."

The Royal College of Physicians of Edinburgh held an extraordinary meeting for the purpose of considering this bill, and agreed to the following Memorandum, a copy of which was sent to all the Scotch Members of Parliament:—

1. The College has no objection to the continuance of the Lunacy Board for Scotland.

2. The College has great satisfaction in observing the introduction of a clause for the purpose of facilitating the admission of patients presenting themselves voluntarily into asylums, the necessity for which was pressed on the attention of Parliament by the College in 1864.

3. The College objects very strongly to Clause XIV. of the bill, which gives to parochial boards the power of providing buildings and other accommodation for the insane pauper poor. The report of the Royal Commission on the state of lunatics in Scotland gave ample proof of the misery and bad management of such places. The reports of the Commissioners both in Scotland and England are full of statements opposed to them; large sums of money have been advanced for the erection of commodious district asylums, on the faith of the poor-house wards for lunatics having been abolished, and yet this bill proposes to return again to an obsolete system which has met with all but universal condemnation.

4. The College is of opinion, and has already urged on the attention of the legislature the necessity of extending the provisions of Clause IX. of the bill to all lunatics, whether pauper or not. Many of the Fellows can testify to great inconvenience and danger to the public being experienced by the discharge of patients from asylums at the request of their friends, long before they can safely be trusted at large.

5. The College objects to Clause XVI. of the bill which allows patients to correspond *privately* with the Lunacy Board. The College is of opinion that the existence of any such law would foster distrust in the minds of patients in an asylum, and would be very subversive of discipline. The College earnestly entreats the legislature to pause before it enacts such a law.

VI. The College also objects to Clause XII. of the Bill which makes incumbent on all persons in whose house lunatics may be temporarily placed, and who may derive profit from lodging them, to report their having such inmates to the Lunacy Board within fourteen days. If this becomes law it will seriously interfere with the due professional treatment of a number of cases of slight and temporary aberration of mind. In these cases change of air and scene is often of importance in hastening recovery; but these curative means would, in many cases, be objected to by the relatives, if they could not be enjoyed without notification being made to the Lunacy Board.

VII. The College is of opinion that it is incumbent on the Legislature, which requires of medical men the duty of signing certificates of lunacy, to give them some sort of protection from reckless and malicious prosecutions raised against them by lunatics, whose reason may have been only partially or not at all restored. The College has no desire to protect medical men who either through carelessness, corruption, or malice, have signed certificates not borne out by facts; but it is of opinion that no prosecution should lie against a medical man unless the prosecutor is obliged to prove that the certificate has been signed without probable cause, *i.e.*, wilfully, falsely, or maliciously.¹

If it be asked what protection the public have against the improper signing of such certificates, the College would point to Sect. XXXVIII. of the Lunacy (Scotland) Act (Vict. 20 & 21, cap. 71), which it is not proposed to repeal, and which contains most stringent provisions, which can be enforced under the penalties of fine or imprisonment, against any medical man who either carelessly or falsely signs the certificate required by the Act.

The College begs to suggest the addition to the bill of some such clause as the following:—

Proposed Clause for the Protection of Medical Men from unjust Prosecutions.

The 14th section of the Act 25 & 26 Vict., cap. 14, is hereby repealed, except in so far as it repeals the 34th sect. of the Act 20 & 21 Vict., sect. 71, and in lieu thereof, any party applying to have any lunatic detained in any asylum, lunatic ward of a poorhouse, or house duly licensed for the reception of lunatics, may present a petition to the sheriff of the county where such lunatic is resident or may be found, or where the asylum, lunatic ward, or house be situate to be subscribed by the party applying for the same, accompanied by a statement of particulars in the form of Schedule C to the Act 20 & 21 Vict., cap. 71, annexed, and setting forth the degree of relationship or other capacity in which the petitioner stands to such lunatic; and such sheriff shall thereupon remit the said petition to two medical persons to be named by him (such persons being registered practitioners under the Medical Act), to see and examine the said lunatic, and satisfy themselves as to the mental state or capacity of such lunatic: and if such medical persons shall be satisfied that the petition should be granted, they shall report their opinion and the condition of the alleged lunatic to the said sheriff, and the sheriff may thereupon grant an order for the reception of the said lunatic into or detention in the said asylum, lunatic ward, or house, or if they shall think that the petition should not be granted, they shall report the same to the sheriff, who shall refuse the said petition, or make such other order as may appear expedient, in the form of E to the said act annexed, or as near as may be to the terms of such form; provided that the two medical persons shall have no immediate or pecuniary interest in any asylum, lunatic ward, or house: And no superintendent of any such public, private, or district asylum or house, shall receive or detain any person as a lunatic therein, unless there shall be produced to and left with such superintendent, such order by the sheriff, dated within fourteen clear days prior to the reception of such lunatic; or if such order be granted by the sheriff of Orkney and Shetland, within twenty-one clear days prior thereto; provided that the superintendent of any public, private, or district asylum, may receive and detain therein, for any period not exceeding three days, and without any order from the sheriff, any person as a lunatic, whose case is duly certified to be one of emergency, by one medical person qualified as aforesaid; and no action at law shall lie against any medical person so acting, unless corrupt or malicious motives.

¹ The of law Scotland in regard to actions against medical men signing certificates of lunacy, is ruled at present by the decision of the Judges in the case of *Strang v. Strang* (Report of Cases in Court of Session, second series, vol. xi.).

In that case it was ruled, that a medical man who grants a certificate of a person's insanity, as required by law, is not a privileged defendant; and that a person who has been confined by a sheriff's warrant in consequence of such certificate, in pursuing for damages, is not obliged to prove that the certificate was granted without probable cause, *i.e.*, wilfully, falsely, or maliciously.

The sheriff shall fix the fee to be paid to such medical persons to whom the petition shall be remitted by him; and such fee shall be at once decreed for by the sheriff, and paid by the party applying.

In name and by authority,

JOHN SMITH, M.D., *President.*

CORRESPONDENCE.

(To the Editor of the Edinburgh Medical Journal.)

SIR,—I am surprised to find from the letter in the number of your Journal for the present month, that Dr Sidey seriously claims not only the invention of the uterine support, a description of which I published a few months ago, but even asserts that I assent to his pretension. I beg leave, however, most distinctly to deny this assertion, and to say that I am not aware of ever having given him the slightest reason to suppose that I recognised his claim. It is true that Dr Sidey made a statement, about a fortnight ago, before some professional friends, to the effect that he had at one time devoted his attention to the improvement of pessaries, and that one had occurred to him similar to mine; but he gave no evidence that he had ever brought it to such a state of perfection as to be used with comfort or safety by a patient; and when he said he intended publishing his statement, I considered he was in jest, and said, "By all means do so, and be sure you publish the whole;" but I never supposed he would do so, as I had told him that I had incontrovertible proof that I had first suggested the instrument, of which I hope fully to satisfy you before I finish.

But, in the first place, allow me to say, that I never heard of Dr Sidey's pessary until mine had long been in use; and if he had invented so valuable an instrument as the one I have had the satisfaction of bringing before the profession, it is very remarkable that he should have allowed so many years to pass without publishing it; and that he should not now be able to show a perfect specimen of his invention. On the contrary, the only examples he can give of his instrument are what he calls "*his failures.*"

Let me further state that I had not the least suspicion of any one having proposed such an instrument as the one I gave a model of to Mr Young, who made it for a patient of mine, entirely from my instructions, and so successfully that the lady was enabled from the first to wear it with the greatest comfort and benefit. In proof of the facts I now state, I beg to subjoin a letter from Mr Young, which, in my opinion, is perfectly satisfactory.

I cannot conclude this letter without referring to the fact that this is not the first time the merit of having suggested this instrument has been claimed, which is a very convincing proof of its value. I hope, however, that these groundless pretensions will now cease, and that the discussion they have given rise to may have the effect of bringing the instrument still more into notice, as it not only affords the greatest comfort, but renders unnecessary the dangerous operation which was attempted, and has been boldly advocated by Dr Marion Sims in his recent work, on the Surgical Diseases of Women.—I am, Sir, yours, etc.,

CHARLES BELL, M.D.

Copy of Letter from Mr Young, Cutler.

"Dr Chas. Bell.

Edinburgh, 28 North Bridge, Jan. 23, 1866.

"SIR,—In reply to your inquiry regarding the date at which the first pelvic uterine support was made, I beg to supply you with the following extracts from my ledger:—

'1856, March 5.—A Prolapsus Bandage, with various alterations, £2.'

"This was the first uterine support I ever made with a steel pelvic spring or frame; it was made and completed entirely from the directions and instructions you gave me.

"The next instrument of the same kind was made for Dr James Sidey, as the following extract from his account shows:—

'1856, July 7.—A Pelvic Uterine Support, £1, 12s.'

"I am, Sir, yours most respectfully.

(Signed) ARCHIBALD YOUNG."

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*The Harveian Discourse : On the Life and Character of Dr Alison.* By DR A. HALLIDAY DOUGLAS, President of the Society : April 1866.

GENTLEMEN,—The subject on which I propose to address you must be regarded as an estimate of the character rather than a sketch of the life of Dr Alison. Nothing that concerns him can be thought unsuited to this occasion ; and I invite your attention to the circumstances of his personal history, even more than to his professional life and works. It is not on account of personal knowledge of Dr Alison that I ask you to listen to me :—I cannot hide from myself that to many of you he was better known, and I have hesitated not a little in my attempt to commemorate the name of one so great and good,—one of the truest men to be found even in the highest place of our profession. The depth of his character, the multitude of his works, and the profound philosophy of his intellectual speculations will excuse my hesitation ; but though I can say little of these in such an address as this, I cannot but endeavour to convey to you something of the pleasant and improving fellowship I have enjoyed in the renewed study of his works, and the contemplation of his character.

If I can inculcate any lesson from the example of his life, I shall be satisfied. To illustrate the doctrines he taught would be to rewrite his books, and would be altogether beyond the compass of an address suited to the occasion of the HARVEIAN FESTIVAL.

Though Edinburgh claims the honour of being the birthplace of Alison, the first openings of his mind are to be traced in the retired parish of Kenley, whither his father had proceeded in the discharge of his holy ministry.

Whatever share the parish schoolmaster may have had in communicating the rudiments of knowledge to the juvenile WILLIAM ALISON, we cannot overlook the influence of the companionship and the scholarly instruction of his father—the accomplished author of the philosophic *Essays on Taste*.

In the year 1800, at the age of ten years, we find Alison

returning to Edinburgh with his father, the Rev. Archibald Alison. And now, let it be observed, that, with all the advantages offered for the school education of his son, Mr Alison adhered to the system of home education.

I must not detain you by speculating on the influence thus early exerted in framing a disposition, naturally tender and thoughtful, into habits of life in which modesty, self-forgetfulness, and self-reliance were singularly conjoined. Let me quote Sir Benjamin Brodie's estimate of his home education :—"Much of my success in my worldly career is," says Sir Benjamin in his Autobiography, "I am convinced, to be attributed to this discipline of early years." And again, after recording the drawbacks of his home life, he adds :—"Yet, I am inclined to think, that what we gained in some respects, was fully equal to what we lost in others. In my solitary walks, I early acquired the habit of reflection, and of thinking and judging for myself; and the consequence has been that, through the whole of the after part of my life, I have never been inclined to adopt opinions on the authority of others, nor until I had looked at both sides of the question. I also learned to be independent of others for occupation and amusement."

I should like to have had Alison's written estimate of the home and paternal influence on his life.

If we may reason from Mr Alison's writings on the subject, we learn how important he regarded the period and the impressibleness of youth; and of what consequence he thought it to cultivate in the young the instinctive tastes for the beauty and sublimity of nature :—He says, "It is to provide them amid all the agitations and trials of society with one gentle and unrepublishing friend, whose voice is ever in alliance with goodness and virtue, and which, when once understood, is able both to soothe misfortune and to reclaim from folly. It is to identify them with the happiness of that nature to which they belong; to give them an interest in every species of being which surrounds them; to lay the foundation of an early and of a manly piety; and to make them look upon the universe which they inhabit, not as the abode of human cares, or human joys, but as the temple of the living God in which praise is due, and where service is to be performed :"—Methinks we shall search in vain for a better illustration of these truthful descriptions of the father, than the life and character of the son offers. Where shall we see better exemplified the pure enjoyment of daily life; the interest in every species of being; the manly piety and the intelligent view of the universe, not the abode of human care, and human pleasure only, but a place of service and of praise to its Almighty Creator.

It was neither an accident nor a miracle that the father's views were thus reflected in the life of his son; there was both an inherited resemblance and a communicated impression; and to the same influence we may ascribe much of the habit of his after-life,

and the singular freedom of his mind from everything like a narrow professional bias.

Those of us who knew Dr Alison only during the last thirty years of his life probably do not associate with him the idea of great physical activity; but I am well informed that a preference for active life, and great power of endurance, were manifested from early life. "From 1809 to 1813, he and his brother, Sir Archibald Alison, made pedestrian tours yearly in the Highlands of *Scotland*, in *Wales*, and in *Cumberland*, with knapsacks, etc., walking 300 or 400 miles each time.

"In 1819, he attempted the ascent of *Mont Blanc*. The party slept at the *Grands Mulets*; and next morning, when within two hours' walk of the summit, a tornado of wind and snow, which lasted two days, obliged them to return to Chamouni 're infecta,' which he always lamented."

Alison's preference for military life, and his interest in military affairs, existed from his earliest years. Sir Archibald Alison describes, as one of his earliest recollections of him, that "during the Irish rebellion in 1798, he was caught belabouring a bed of onions, and said, 'Oh, papa, we are only quelling the rebellion among the onions.'" But at that early age he showed more than a playful interest in the military events of the day. His brother mentions that "in the year 1799, William, then eight years old, and I were with our father, on a visit at an old place near Kenley, our home; it was at the time of Suwarrow's campaign in Lombardy. The company were looking at a fine map to find the locality of one of his engagements. No one could find it." . . . "William, said his father, do you know where it is? and he at once put his finger upon it."

After his return to Edinburgh, he is described standing for hours together looking at the soldiers at drill on Bruntsfield Links; and as soon as he could shoulder a musket, "he entered the ranks himself;" and "the moment peace was concluded in 1814, he hurried over to Paris with his brother and Mr Patrick Fraser Tytler, and feasted his eyes for two months with the sight of the French and allied armies."

With Alison all this was not the mere whim of youth. It was no fancied heroism of military life. With him to enter the volunteer ranks, and master the details of military drills, would be more than a pastime. His brother's earliest recollection of him is his "warm interest in public, especially military affairs;" and, doubtless, with a public spirited intelligence peculiar to himself, he early apprehended the national importance of military events on which were suspended greater than even national interests.

Many of you can recall his interested and clear descriptions of military events and battle scenes. Sir James Simpson has favoured me with the following account of a curious occasion:—"Shortly after I became a professor," writes Sir James, "Dr Alison joined

our graduation examiners one day after a country excursion, full of the enthusiasm of the soldier rather than of the physician. He had just visited the site of Birrenswork, in Dumfriesshire, where he had traced out two square Roman camps at the base of the hill, and a rounded British camp on its summit. Dr Alison believed that he had discovered the strategy by which the British stronghold had been successfully besieged and won. He described and drew the site of the well, half-way up the hill, which had supplied the British forces, and he showed us that the Romans had gradually extended outwards and upwards a screen wall, stretching from the side of one of their camps, till they were enabled to cut off by it the native forces from access to water. I remember," adds Sir James, "how earnestly he descanted on the advantage of this special mode of siege, and how he rejoiced in the idea that he had detected the exact mode of the military operations though seventeen or eighteen centuries had elapsed since they had been enacted."

At the age of thirteen he commenced his course of study in the University of Edinburgh. He has not left any record of what departments of study chiefly engaged him, but there are not wanting evidences of earnestness and success. At this period he was surrounded in his father's house by a singularly select circle of the most accomplished men of letters.

In addition to these influences, it is known that he was early and deeply impressed by the lectures of Dugald Stewart,—“his intimacy with, and his admiration for whom,” as I am informed, “were great; and he visited him regularly for weeks together for several years at this time; and this very much determined the direction of his thoughts and studies.” I learn, also, from his distinguished brother, that he “was a good mathematician,” that he inherited all his natural talent in this respect; and, Sir Archibald adds, “he was my only master in Euclid till I went to the mathematical class of Mr Leslie.”

In the choice of his profession, it is well known that he sacrificed his predilection for military life in deference to his father's and his mother's wish; and that “his inclination early led him to the study of philosophy—especially metaphysics, of which he was very fond.” We have also seen how ardently, in the friendly intimacy of Mr Stewart, Alison afterwards prosecuted the study of philosophy; and you well know how closely he carried the spirit and the principles of that philosophy into his habits and his writings as a scientific physician.

With all this in view, I have failed to discover any conclusive reason why he selected Medicine as the business of his life. It could not be unknown to himself, that his attainments in philosophy had gained for him the high approval of Mr Stewart, who did not conceal the desire that he should succeed him in the Chair of Moral Philosophy. Alison, however, does not appear to have entertained this view at any time, as I am informed it was “never understood that he thought of making the study of philosophy the

work of his life; his mind led him to prefer an active life as his duty." This, and the affectionate admiration he entertained for his relative Dr Gregory, and the field which, mayhap, he even then foresaw the practice of our profession would offer for the life-long exercise of his practical benevolence, no doubt determined him in the choice of Medicine as his profession.

Dr Alison graduated in the year 1811, but we find no record of his professional pursuits till 1815. From this time he devoted himself to his multiplied labours with an earnestness and self-denial that were distinguished, even in a profession in which such qualities are not rarely met with.

As a scientific physician and a physiologist, Alison was less an observer than a thinker; and he has not added to the store of our knowledge by discovery. The great power in his character arose from his fearlessness and logical consistency, and his perseverance and incessant labour in the pursuit of any object he had set before him; and he was upheld in these by an extraordinary self-renunciation.

The magnitude of his intellectual views, his power of reflection, and his perception of great principles, taught him to rely upon results still distant and future. A vivid and a vigorous imagination was one of the attributes of his mind. It was this that to himself vivified and enhanced his pursuits, and gave such force and forethought to his excogitations, that he became a *discoverer* by his inductions, if not by his observations.

In his tastes and recreations may be seen the action of an imagination both clear and active. "His taste for poetry was very refined, and he had a great many of the finest passages of Milton, Shakspeare, Campbell, Burns, Scott, and others, by heart, which he was fond of repeating in his pedestrian tours." He is described, too, as taking a simple and fond interest, during his early life, as well as in the quiet retirement of his declining years at Woodville, in watching the habits and recognising the familiar note of every little bird, and he specially delighted in the bright beauty of every flower that adorned the garden or the hedge-row. The private life and the public works of Dr Alison prove him to have been a man of eminently social disposition; but that he was not expressly of a convivial spirit is shown by the circumstance that he never attended any of the festival meetings of this Society until he occupied the chair in the year 1832, when he delivered a discourse on Medical Education. The strength and the reverential character of his associations is illustrated in the following account, by an eyewitness, of a scene which must have been as impressive to his class, as it seemed moving to Dr Alison himself:—"In the course of his lectures on physic, in the session 1843, the class was moved to the Moral Philosophy class-room. Dr Alison, finding himself about to occupy the chair from which Dugald Stewart had lectured, re-

mained standing, and spoke to the effect,—that ‘it was with a feeling amounting almost to awe, that he found himself about to address his class from the same chair as had been occupied by his great preceptor Dugald Stewart, to whose metaphysical training he owed more than he could convey in any words.’ He went on to characterize Mr Stewart as a philosopher who had been one of the very brightest ornaments of the University, and added, that while he would to the utmost of his power endeavour to do honour to his memory by discharging his duties in something of the same spirit as had animated him, he earnestly hoped we would give the same diligent heed to such instructions as would be offered to us, as those students had been wont to do, who had enjoyed the great privilege of listening to his extraordinary prelections.”

Although we have no record of the first years of his professional life, it is abundantly evident that Dr Alison had at the outset applied himself to the work of preparing for the duties of a physician and a teacher. His laborious exertions as physician to the New Town Dispensary in the year 1815, and subsequently, were prosecuted with the same earnestness and perseverance as were the whole pursuits of his professional life in after years. The reports, founded upon the practice of the Dispensary, testify how arduous Dr Alison made the duty to himself; and, by his elaborate arguments on various practical questions, he made it evident that he had entered upon a field of observation which he would turn to good account. In these reports is to be found the basis of some of his most instructive contributions to the literature of practical medicine, particularly on fever and on varioloid diseases. *There*, too, may be traced his early impressions of the relations of destitution and disease, which, in after years, grew into his writings on fever and on the management of the poor. At this stage of his professional life, he, doubtless, owed much to the mutual regard and confidence of Dr Gregory and himself. So early as this, Dr Gregory employed him in writing his letters of consultation, as well as in assisting him with his lectures on the practice of physic; and, not long after, Alison came to be recognised in many quarters as a consulting physician; but still, and for years subsequently, he devoted himself to the observation of disease amongst the poor, to an extent almost unequalled in the history of medical men.

Perhaps it is true, that if his reputation had rested on his attainments as a practical physician, we could not have claimed for his memory the extraordinary honour which his name commands. It is doubtful if at the bedside he brought to bear the acuteness of observation, and the taste for details which, in the case of most men, are essential to habits of prompt and accurate diagnosis. Nor does he appear to have had the versatility of mind and the fertility in resource which give power to the physician in the management of the sick and the treatment of disease. His universal repute as a physician rested on his vast intellectual

power, his unpretending common sense, and the entire absence of self-regard: these silently secured the confidence of all, and a foremost place in his profession was accorded to him, though never asserted for himself.

In so far as he did not derive the full benefit of the high position he so soon attained, it was simply the result of his engrossment with inquiries into the condition of the poor, and his childlike unconcern about his own material interests. I cannot attempt to illustrate the phases of these habits which adhered to him to the end; so that, when, by universal consent, he held the first place in Scotland as a consulting physician, his income is believed to have been short of what it might have been; and, moreover, what now he did not give *in time* to the destitute, he made up by *his gifts*; so that those who knew him best, say of him,—“He made a fortune by his talents, and spent it by his benevolence.” There is no reason to believe that he was indifferent to the honours of distinction, or to the good opinion of his neighbours; we have seen, indeed, how little he valued mere worldly prosperity for its own sake, but he never undervalued, he never neglected the opportunity of using his influence for the good of our profession, and, doubtless, it was such disinterested motives that prompted the last public efforts of his professional life, when enjoying the much needed rest of his latest years, he came forward at the Medico-Chirurgical Society, and to preside at the British Medical Association, to impress anew on our profession the principles and those views of practical questions which his own experience and the advances of knowledge had only rendered more and more important to his own mind.

Dr Alison was connected with the University for above forty years as a teacher. For the last three years of his life as *Emeritus* Professor, and for a few years previous to 1821, assisting his uncle, Dr Gregory, and as Professor of Medical Jurisprudence.

I hesitate to speak of Dr Alison as a Lecturer, and yet a general estimate of what he was cannot well be formed without something being said on this subject. It is undeniable that he possessed rare qualifications for the duties of teaching; though he did not excel in all respects in this capacity. His extraordinary intellectual power and the moral dignity of his character secured for him in the class-room an affectionate and universal deference; and to this I attribute what is mentioned to me by a correspondent, who writes,—“Dr Alison’s was the only class in which I never heard a student reproved for inattention.” His comprehensive views of principle and the simplicity of his aim in the prevention and cure of disease, gave weight to his teaching; though the involved style of his composition sometimes made it difficult for his younger and less educated pupils to follow his meaning in the lecture-room.

The grand defect, perhaps the only one, in his lectures on Physic consisted in the hurry and compression of the lectures in the later

part of the session, owing to want of plan and method in the early part of his course.

The variety of Dr Alison's written works makes it impossible to convey any general idea of their character and value; and the analysis of them individually is beyond the compass of the Harveian Address; nor shall I tax your patience by giving a mere catalogue of his writings. It is indeed scarcely possible to give a just estimate of his character and work without a detailed notice of his great works,—particularly on "Vital Affinities," "Inflammation," "Fever," and the "Management of the Poor." These, however, have been referred to by other writers; particularly in the notice of his life by Professor Gairdner, in which we have a singularly complete view of his works and a concise statement of his opinions. Permit me rather to direct your attention to some of his earlier works. I think the arguments maintained in these have not been so justly recognised as they deserve; and there is a peculiar interest attaching to the earlier writings of one whose works have exerted so important an influence on medical opinion.

In passing, let me refer to his *Outlines of Pathology*, one of the most important and useful of his works; and which, in the words of one of his reviewers, "bears all the impress of the author's character,—calm, thoughtful, and reflective." This work, I have reason to fear, has not the place on our bookshelves nor in our thoughts which it well deserves. I am quite sensible of the unattractiveness to many minds of the close and compressed reasonings which it contains. It is not as a text-book for students I would urge its use. It is not constructed for easy reference; nor is it clothed in a style of language so attractive as to gratify us in its mere perusal. For the use of those, however, who desire to cultivate and to maintain a comprehensive view of the relations of diseased actions, based upon sound physiological principles, the first and second parts, on the general doctrines of disease, on therapeutics and on inflammation, are invaluable,—embodying a system of true medical philosophy; and in its practical reasonings, eminently suggestive to all those whose experience may enable them to illustrate for themselves the principles unfolded.

The extraordinary extent and the variety of Dr Alison's attainments, the honesty and broad sense of his reasonings, and the cautiousness of his own conclusions, are well illustrated in his papers on the "History of Medicine during the early Part of this Century," and on "Instinct." In the latter, he displays an acquaintance with natural history which did not appear in his ordinary pursuits.

Much interest attaches to his paper on the "Physiological Principle of Sympathy." It illustrates well the difficulties which occurred to a thoughtful mind in regard to the discoveries of Sir Charles Bell; though the advances of knowledge have removed

much of the difficulty then felt. By a forcible argument, Dr Alison confirmed the view, "not that the respiratory nerves excite *only* respiratory movement, but that these are the only nerves which excite such movements." In the course of this paper, Dr Alison asserts, on behalf of older authors, "the merit of exposing the futility of explanations of the respiratory actions, derived from the connexions among the nerves in their course," which he thought "Sir Charles Bell claimed as his own." "No one," says he, "can read the works of Haller, Whytt, and Monro on this subject, and suppose that it was reserved for any physiologist of the present day to prove that the sympathetic actions of the respiratory muscles cannot be explained by the connexion among their nerves." The paper is characterized throughout by Dr Alison's unfailing objection to merely mechanical explanations of conditions which demand a more truly physiological interpretation.

The earliest of Dr Alison's professional writings were his Dispensary Reports, contained in the Edinburgh Medical and Surgical Journal for the years 1817, 1818, and 1819. In these reports he laid the foundation for some of the most important discussions that engaged him in his after-life:—for example, the relations of destitution and disease, and the distinction of chickenpox from smallpox. The direction in which the practical habit of Dr Alison's mind turned, and the sagacity with which he laid the foundation of his reasonings, are well illustrated in many parts of these Reports. With reference to the causes of fever, its sources, the influence of extraneous effluvia, and the non-production of the specific disease from these, its contagious communication, and the importance of isolation, and other preventive measures, it is profoundly interesting to read the forcible and conclusive exposition he gives of the views which in point of fact remain, after the lapse of fifty years, very much as he propounded them. It is inconceivable that they should have forced themselves on public attention so slowly, and that even now we have learned only in part the practical application of his principles. Were it possible, I would ask you to listen to some of the passages in which he records the result of his observation with a fulness and a force that gives them great practical value at the present day.

About the same period, a question arose as to the identity of chickenpox and modified smallpox. The observations and inquiries on the subject by Dr John Thomson, as recorded in his very remarkable Letters on Varioloid Diseases, went far to satisfy many that they are not distinct diseases. The grounds of his reasonings are fairly indicated in the following sentence:—"I have had many opportunities of perceiving the uncertainty of the grounds upon which, in particular instances, a case was declared to be chickenpox or smallpox,"—"a reference was usually made to an ideal standard, indefinite and varying, and influenced by moral as well as medical considerations."

The subject became a matter of diligent inquiry to Dr Alison ; and his Dispensary Reports contain the records of his observations ; but he argues the whole question in a review of Dr Thomson's Letters, contained in the sixteenth volume of the Edinburgh Medical and Surgical Journal. Now, though the harmlessness of both diseases has deprived the discussion of much of its importance, still the book, as well as the review, are invested with a lasting interest as noble specimens of philosophic medical discussion, and reflecting minds both great, both practical, and differing in their views of the question at issue, as I think, in no small degree, on account of different inborn mental habits,—Dr Thomson refusing to admit any doubtful conclusion until it is demonstrated ; while Dr Alison, in this as in his other discussions, proceeds upon a broader and a safer foundation, and is disposed to rely with greater confidence upon premises which may indeed be conjectural, but which are in harmony with the great principles that lie at the basis of the natural sciences.

In reviewing the diagnosis of the two diseases, Dr Alison freely admits that they cannot be distinguished by the appearances of the rash alone ; and he lays down a principle with which now-a-days we may be quite familiar, but which is perhaps sometimes lost sight of :—“ Any nosological arrangement of febrile eruptive diseases,” says he, “ which is founded solely upon the appearances of the skin, cannot be justified without losing sight of the useful practical purposes which render it important that diseases should be classified at all, or studied in connexion.” Without tracing Dr Alison's argument, I quote the following statement of facts on which he rests his conclusion against the identity of the two diseases, and in support of the distinct individuality of each :—

1. The disease will always produce an eruption similar to the original one.

2. It will affect indiscriminately and with equal mildness persons who have had smallpox or cowpox, or who have had neither.

3. It will not furnish any protection against smallpox.

“ Cowpox,” he says, “ appears to run its course after chickenpox frequently.”

In closing the argument, Dr Alison admits that “ Dr Thomson, by the ingenious explanation he has attempted of all the phenomena in question, has in some degree neutralized the evidence of the facts adduced,” which, I presume, amounts to nothing more than an admission that the question does not admit of a final and conclusive proof on one side or other.

The review also contains important information regarding the character of the prevailing varioloid epidemic with reference to statements in the Report of the National Vaccine Establishment. It enters into the question of the proper mode of conducting the vaccine process, and of the nature of the protection afforded. I quote one only of his conclusions, to the effect that his observation

of the recent epidemic tended "to limit our confidence in the preventive power of cowpox, but at the same time to extend and confirm our knowledge of its modifying effect."

The well-known observations on "the Pathology of Scrofulous Diseases," and on the "Origin of Tubercle," first read to the Medico-Chirurgical Society in the year 1822, are pre-eminently distinguished by the philosophic character of Alison's practical writings, and of his bent towards public or social medicine. In themselves, the observations have an intrinsic value on account of the very complete exposition he gives of the predisposing circumstances and exciting causes of tuberculous diseases. Not professing to write what was new on the subject as regards the nature of the external causes, he illustrates with a truly thoughtful discrimination their relative importance, founding his observations, as he says, "on facts occurring on so large a scale as to be freed from sources of fallacy which vitiate many medical reasonings." Those of you who have studied his observations will remember the copiousness of the evidence he adduces on the influence of cold and wet, of town and country life, and of poverty, with its concomitants,—dwelling not so much, as preceding writers had done, on the supposed influence of each or several of these circumstances, but illustrating the effects of such adverse influences acting upon previously debilitated—that is, scrofulous constitutions.

Many passages might be quoted to show that Alison, at that early period, fully appreciated the importance of the constitutional cachexia in the tubercular diseases: let one suffice. With reference to the phthisis of young persons, he says "it is more generally the effect of the deposition of the matter of the white tubercle, and depends on circumstances of predisposition." Thus pointedly distinguishing, though not in explicit terms, between such cases of constitutional tuberculosis and those usually met with in more advanced years in the form of the tubercular infiltration of Laennec, in which Alison insists on the existence of inflammation, and which recent observers agree in regarding as an inflammatory product.

As to the theory of the inflammatory origin of tubercles, of which Alison has been considered the advocate, and so far held responsible for fancied notions in the antiphlogistic treatment of tubercular diseases, I regard his view to be, not that tubercle is in any sense a product of inflammation, but rather that inflammations are apt to arise in connexion with tubercular deposit, in some cases as its exciting cause, in other cases as its effect.

It must be admitted that such a proposition as the following, taken just as it stands, has the appearance of a very absolute assertion to the contrary:—"The deposition of tubercle may be, and often is, the result of an action to which it would be absurd to give any other name than inflammation." But I submit that the assertion here is in no sense absolute, and its meaning, in all fairness, is to be deter-

mined in the light of such of his statements as the following:—"The distinctions between tubercles and the usual effects of inflammation are such as to show that tubercles can in no case be ascribed to inflammation as their sole cause." Again, he says, "But, in certain constitutions, or in certain circumstances, an inflammatory attack, if not very speedily remedied, is very likely to act as the immediate cause of a deposition of tubercles." Such views as are expressed in these sentences do not necessarily involve any direct connexion between the inflammation and the tubercularization; and, moreover, it must be borne in mind that the tubercular infiltration (of Laennec) had not been distinguished from the miliary tubercle; and so long as the former had not been recognised as a product of inflammation, having no direct relation to tubercle, and both forms of disease were regarded equally as tuberculous in their nature, it is not surprising that the inflammatory process, known in many cases to originate the infiltrated form, should be supposed to have some influence in determining the other form or miliary deposit; but we have seen that Dr Alison never loses sight of the all-powerful influence of the tubercular diathesis in the deposit of the miliary (or white) tubercle.

In accordance with these views, the general rules laid down by Dr Alison so long ago as in the year 1822, are precisely those on which not a few modern physicians have plumed themselves as though they had introduced a new era in the treatment of tubercular diseases. "Our practical objects are," says he, "*First*, To improve the morbidly defective state of the vitality of the blood. *Secondly*, To avoid causes of congestion of blood, or inflammation in such subjects. *Thirdly*, When inflammation occurs, to restrain it by early, but moderate antiphlogistic treatment." I humbly submit that these general rules have not been greatly modified by all we have learned since the time they were laid down, nearly fifty years ago.

The last of Dr Alison's writings to which I refer are amongst his minor and latest works.

His address to the British Medical Association, delivered about a year before his death, may be regarded as his last testimony to our profession:—All it contains is eminently characteristic of himself; it seems as if he had sought to compress in it and to illustrate the principles and the religiousness of his own professional life. He refers to the connexion and reconciliation of science and religion as the great question of the day; and he urges upon our profession the responsible consideration that it is by the natural sciences, and especially by those departments which constitute the basis of medical knowledge that that great question is to be illustrated. He asserts, "The fixed system and design in creation,"—"No creature being a fortuitous concourse of atoms."—"We hold," says he, "the science of physiology to have been truly regarded and justly vene-

rated from the earliest period of scientific inquiry as that portion of general science in which the *adaptation of means to the end* is most obvious;—"The argument founded upon which," he adds, "is more convincing now than it was centuries ago." He sums up his elaborate argument by re-affirming the syllogism of Reid, which he says, "hardly admits of simplification, but it receives immense additions in the minor proposition by the recent discoveries of comparative anatomists:—That the adaptation of means to the end implies design; that many such adaptations are seen on examination of nature, and, therefore, the works of nature indicate design." In the course of his address he insists upon and illustrates what theologians would style the doctrine of the *sovereignty* of God; but which he styles in language more suited to the occasion, "The arbitrary will of the Supreme Being." "The indications of arbitrary will," says he, "as distinguished from necessity, are of peculiar importance in this argument;" and he goes on to illustrate "how the facts of natural science afford the clearest indication that the universe is under the guidance not only of laws, but of a power superior to these laws, and capable of changing them at pleasure;" urging still further, that "the adaptation of means to ends is often most distinct in cases where order, so far as we see, is broken." I leave these quotations to speak for themselves. On the subject of religion, more than any other, I desire that Alison should be his own witness. It is written of him, by Dr W. T. Gairdner, that "he did not talk about religion, he only acted it." Most true, indeed; and he tells us how unwilling he was to undertake the duty of an instructor in religion. Yet he has written what enables us not only to judge of his creed, but also to determine the Christian motives of his life.

In his Essay prefatory to the Lectures on Medical Missions, he gives a summary of Christian doctrine, and he enforces, from the point of view of a philosophic thinker, these doctrines as necessary conclusions, though they are not urged with the fervour of a preacher. He illustrates the peculiar fitness of a medical training for the reception of the doctrines of the divine grace; and maintains that "it is not assigning too much importance to medical science, or to the medical art to assert that, of the temporal benefits which must precede and attend the extension of Christianity over the world, *their* influence on social happiness must be one of the surest and most powerful;" and that "the only security on which reason or experience authorizes us to place any reliance for the continuance of this influence over the rest of mankind is the union of that power over nature, which we have acquired by science and art, with that sense of obligation to use that power for the glory of God and for the good of mankind, which is imposed by Christianity, and by no other power that has ever yet influenced masses of men." And we are not left in doubt of what Alison really meant by Christianity, its principles and obligations. He asserts expressly "the inefficiency

of human works for the salvation of the human soul"—that there is an "enemy in the world"—that "the mind of man is so constituted as to be open to the influence of a higher spirit"—and that "the only atonement which can avail has been granted as a free gift from heaven." Gentlemen,—I cannot, I need not detain you by descending on these affirmations of one who did not hastily obtrude his views on these subjects.

Perhaps it is not generally known that, during his last years spent in the retreat of country life, under the always increasing interruptions of his dreadful malady, and though memory was in part impaired, his activity and his interest in many public matters, especially medical affairs, was uninterrupted; and his attention to all the different literature of the day was unceasing. Above all, his enjoyment of nature was the source of great happiness to the last days of his life. The placid and daily renewed cheerfulness of his life, and his continual surprise at the pleasant flight of time has left a deep impression on those who surrounded him during those years; and, viewed in the light of his life, and of his own written records of divine truth, we contemplate the peace he enjoyed as that which passes understanding, just as the same records enable us to trace the motives of his devoted life to the consciousness that seems to have grown with him from his earliest years, that he "was not his own, but bought with a price, to glorify God in body and spirit, which are God's."

"Who is the honest man?

He that doth still and strongly good pursue;

To God, his neighbour, and himself most true:

Whom neither force nor fawning can

Unpin, or wrench from giving all their due."—HERBERT.

ARTICLE II.—*On Paracentesis Thoracis in the Treatment of Pleural Effusions, Acute and Chronic.* By J. WARBURTON BEGGIE, M.D., Fellow of the Royal College of Physicians, Edinburgh.

(Read to the Medico-Chirurgical Society, 2d May 1866.)

THE perforation of the thoracic walls, in order to give exit to purulent and other fluids, is an operation dating from the most remote antiquity. In the Hippocratic treatise, ΠΕΡΙ ΝΟΥΣΩΝ ΤΟ ΔΕΥΤΕΡΟΝ—*De Morbis, Liber Secundus*,—a work which there is good reason for believing, although not composed by the illustrious Father of Medicine himself, was written either by one or more of his contemporaries, or by some among his immediate descendants in the school of Cos,—there occur two most interesting and instructive passages, the one having reference to the method by which the existence of pus in the pleural cavity, or empyema, as the latter term is understood by the moderns, is to be recognised,

while the other describes the operation by which that condition is to be removed. The order of procedure, both as regards the diagnosis and the treatment, is in these passages laid down with great exactness. Under the former head, it is directed that after the patient has been carefully washed with warm water, he is to be placed in a firm seat, and his hands held by an assistant, the physician meantime taking him by the shoulders shakes him, and attentively listens in order to determine on which side of the chest a sound is occasioned. Again, in treating of the means of cure, it is directed that recourse to the operation is not to be had before the fifteenth day from the commencement of the effusion; where pain is chiefly felt, and swelling is most conspicuous, the opening is to be made, while a preliminary incision through the integuments precedes the penetration of the pleura, effected by a sharper and more pointed instrument than that which is required for the earlier incision, the second instrument being protected by a piece of cloth. In some instances, the perforation of the thoracic parietes was made not through an intercostal space but through a rib. When a sufficient quantity of pus has been permitted to flow, the wound is to be closed by means of a portion of linen-cloth attached to a thread. Daily a similar quantity of pus is to be evacuated. On the tenth day, when the whole of the collection has been allowed to escape, a mixture of tepid wine and oil is to be injected through the opening for the purpose of cleansing the lung. This part of the operation is to be practised twice daily: the injection of the morning being withdrawn and replaced by a fresh quantity in the evening, and so on. At length, when the purulent fluid has become clear and thin, a metallic sound is to be introduced, the size of which is to be gradually lessened as the fluid itself diminishes; thus the wound is permitted to cicatrize.

In connexion with the brief description which I have now epitomized, it is very interesting to note the expression by the author of the treatise, of some shrewd observations relating to prognosis; for example, an empyema on the left side, he remarks, is less dangerous than on the right. In modern times, as I shall have occasion again to notice, the operation of thoracentesis has been more successful on the left side than on the right. Again, the author mentions, that according to his observation, when the pus was clear and studded more or less with small sanguinolent threads, that appearance indicated the probability of a satisfactory recovery; but, on the other hand, if on the first day of its removal the fluid possessed a colour like the yolk of egg, while on the succeeding day it was thick, having a pale green hue, and emitting a fetid odour, it was likely that the sufferer would not recover, but shortly die. In Hippocratic times, the operation of thoracentesis was not invariably made with the knife, that is by division of the integuments and muscles, in an intercostal space, and the subsequent penetration by means of a sharp-pointed instrument

through the costal pleura; it was sometimes effected by means of the actual cautery.

The succussion of the chest, to which as a means of diagnosis reference is made in the treatise from which I have been quoting, occupies, as is well-known, to this day an important place in our recognition of hydro-pneumo-thorax, or, as it may be called also, of pneumo-thorax with effusion; and very appropriately it bears in our time the designation of *Hippocratic* succussion. The production of the fluctuation or splashing sound on succussion enables us to recognise the existence of that morbid condition, and as I believe of that condition alone. It cannot be said to diminish the interest of the statement in the Hippocratic writings concerning this sound, that the author supposed its production to depend on the existence of pus in the pleural cavity, in contradistinction to water,—he regarded it as a ready means of distinguishing an empyema from a hydro-thorax. Thus, he had overlooked, or rather had been unaware of the necessity there exists for the co-existence of air and fluid in the pleural cavity, in order that any sound should be produced. The writer had noticed and has placed that observation likewise on record, that when much sound is heard there is less pus in the cavity than when the sound produced on succussion is feeble. The explanation of this accurate observation is now readily afforded by us, although misinterpreted by the Hippocratic writer; but surely the misconception respecting the primary cause of the auscultatory phenomenon, which he has so faithfully described, need not lessen in any measure the tribute of respect and admiration for his amazing powers of observation, which a perusal of almost any portion of his writings is calculated to draw forth.

Other references there are in several of the Hippocratic works to the relief which an operation is capable of affording to the sufferers from empyema and aqueous collections,—for example, in the Book of Aphorisms—one of these, because expressing an opinion which I shall have to consider in another part of this paper, may be quoted. It is the twenty-seventh aphorism of the sixth book:—"The sufferers from empyema and dropsies treated by incision or by the cautery certainly perish if the pus or water is suddenly evacuated."

In the writings of Galen there is to be found little in addition to the directions for the treatment of empyema which exist in the pages of Hippocrates. It is not unlikely that the operation of thoracentesis was performed by him at Pergamos, if not in Rome; but that this means of cure was not held in any high estimation by the professors of the healing art during the earlier centuries of the Christian era, is rendered probable from the little that has been written regarding it by Galen himself, and by the nearly complete silence on the subject of Cælius Aurelianus and Celsus.¹ I need

¹ As regards the last-mentioned renowned writer, it is no doubt true that in book fourth, when treating of affections of the liver, he remarks,—“Si vero

not particularly refer to the opinions of the Arabian physicians concerning thoracentesis, some of whom, Rhazes for example, undoubtedly practised it, much as the Greek physicians had done, but none have added anything to our store of knowledge regarding it. Nor do I propose to trace at any length the history of thoracentesis during the seventeenth and eighteenth centuries,—it had been again employed, although very rarely, in the sixteenth ;—such a task is quite uncalled for, seeing that M. Trousseau in the first volume of the “*Clinique Médicale de l’Hotel-Dieu*,” has already accomplished it in a most interesting and satisfactory manner. Moreover, as the student of medical history is well aware, the subject to which I have thus briefly alluded, is discussed at great length by the erudite Kurt Sprengel in his “*Versuch einer pragmatischen Geschichte der Arzneikunde*.” It is worthy of note, however, that in the seventeenth century Thomas Willis and Richard Lower, two names famous in connexion with the progress of anatomy and physiology in England, had performed it. About the middle of the eighteenth century, when the employment of the cautery in thoracentesis had become entirely superseded by the knife, the latter method became in its turn supplanted by the trocar. M. Trousseau informs us that the use of the trocar was recommended in 1765 by Lurde, but that nearly a century before that time its employment had been proposed by Drouin. That the strong recommendation of its advantages by Lurde and others did not obtain for the trocar an immediate or even speedy adoption is evident from the circumstance that surgeons of the eighteenth century so distinguished as Desault and Chopart objected to its employment, on the ground that the wound it inflicted was necessarily violent (*pénétrer brutalement*), and that there existed no small risk of injuring the intercostal artery or the lung.

As was to be expected, the great discovery of Laennec gave a decided impulse to the performance of thoracentesis, from the aid which auscultation afforded in the diagnosis of pleural effusions. The whole subject is discussed with that author’s usual sagacity as well as brilliancy, in his work on auscultation; but, strangely enough, he withheld his recommendation of the operation, except in cases which were in their nature exceptional. He remarks, “There are two cases of pleurisy in which the operation should be performed. The first of

jecur vomica laborat, eadem facienda sunt, quæ in ceteris interioribus suppuracionibus. Quidam etiam contra id scalpello aperiunt, et ipsam vomicam adurunt.” And again, in the eighth book, and there having distinct reference to the chest, because writing “*De Costis fractis*”—“*Si suppuratio vicerit, neque per quæ supra scripta sunt, discuti potuerit; omnis mora vitanda erit, ne os infra vitietur; sed, qua parte maxime tumebit, demittendum erit candens ferramentum, donec ad pus perveniat; idque effundendum;*” but notwithstanding these and a few other references to the treatment of empyema,—that is, of internal abscesses, abdominal and pectoral,—I repeat that Celsus is nearly silent on the subject of thoracentesis, which, on the other hand, as we have already seen, Hippocrates so fully considers.

these is when in acute pleurisy the effusion, very abundant from the beginning, increases with such rapidity that in the course of a few days it causes a general or local œdema, and threatens suffocation." To this Laennec gives the name of acute empyema (*empyème aigu*); and, on the other hand, he assigns the appellation of chronic (*empyèmes chroniques*) to those instances in which the effusion has from the outset been chronic, as well as to those which, in the first instance sufficiently acute, have afterwards become chronic. Now, regarding the employment of thoracentesis in the latter cases, he remarks,—“It is to be had recourse to as an extreme remedy, when there is œdema of the affected side, when the lengthened existence of the malady, the gradually advancing emaciation and feebleness of the patient, and the failure of all other remedies to effect absorption of the effused fluid, seem to hold out little hope of any cure.”

Laennec proceeds to state that the operation is rarely followed with success; and this he ascribes to causes which have not been sufficiently estimated. The first of these is the unhealthy condition of the lung, which is too frequently the seat of tubercles; but the chief cause which, in the opinion of Laennec, opposed the successful result in cases of empyema treated by perforation was the compression, amounting to flattening (*aplatissement*), of the lung against the mediastinum and vertebral column, and the dense nature of the false membrane coating the lung. “The lung,” he observes, “subjected to pressure (*refoulé*) for a long time has lost its elasticity and expansive force; it allows with great difficulty the air to penetrate from the trachea, and only very slowly regains a size sufficient to fill something like the space which it occupied before the occurrence of the malady.” The objection to the performance of thoracentesis from the entrance of air into the pleural cavity, which, as he remarks, has always attracted the attention of surgeons, Laennec does not overlook; but it is evident that the importance assigned to it has in his opinion been exaggerated, while the precise action of air upon the contained organs has also been misinterpreted. Notwithstanding the objections to thoracentesis thus alluded to by Laennec, and there are others, a reference to which will be made as I proceed, it is very satisfactory to find this great observer, near the conclusion of the article on the treatment of pleurisy, thus expressing himself:—“I am persuaded that the operation for empyema will become more common and more extensively useful in proportion to the increasing employment of mediate auscultation.”

Becker, a physician of Berlin, is quoted by Trousseau as having, in 1834, published five cases of chronic pleurisy treated by puncture of the chest, the operation having at his request been performed by the distinguished surgeon, Diffenbach. In the following year, Dr Thomas Davies, in the London Medical Gazette, endeavoured to establish the value of the operation as a

remedy in hydrothorax and empyema, while he objected to its employment in pneumothorax as useless. There are two particulars worthy of note in the observations of Davies,—the first is, the very successful results of paracentesis thoracis in cases of children. On this head I shall have something to say in another part of this paper; for my own experience of the operation in children, although by no means extensive, leads me to agree in the expression of opinion given by Dr West, “that the impression left on my mind by each year’s additional experience is more and more in favour of its comparatively early performance.” The second particular in the lectures of Dr Davies has reference to the exact method he pursued in operating. He used a small trocar, and adopted no special means for preventing the entrance of air; but,—and this procedure has undoubtedly something to recommend it,—he introduced a grooved exploring needle as a preliminary measure, seeking thereby to determine the nature of the effusion, its consistence, and if false membrane likely to interrupt the removal of the fluid existed. In short, as a means of establishing the diagnosis, Dr Davies used the exploring instrument, and as giving confidence to the physician, I am prepared still to recommend its use; indeed, in practice I very generally employ it.

In 1841, there appeared a very valuable contribution to the subject of paracentesis thoracis in the treatment of pleurisy, by a well-known surgeon of Vienna, recently deceased, Schuh, and a still more distinguished physician of the same city, Skoda. This monograph was succeeded in 1844 by the important memoir read to the Academy of Medicine in Paris, by M. Trousseau; while in the same year there were published in our own country two papers well worthy of notice, both of which have had an important influence in extending the benefits which the operation of thoracentesis is capable of exerting. These papers are, first, “On Paracentesis Thoracis as a Curative Measure in Empyema and Inflammatory Hydrothorax,” by Dr Hamilton Roe;¹ and, second, “On Paracentesis Thoracis, with Cases,” by Dr Hughes and Mr Cock of Guy’s Hospital.² Since the period of publication of these memoirs, the attention of the profession has been called to the subject by the appearance from time to time of many successful instances of thoracentesis in the different journals. In America, of late years, Dr Brady, of New York, and still more recently, Dr Bowditch, of Boston, have done much to gain for the operation an established place in professional confidence. The latter physician employing a particular apparatus, for which we are indebted to Dr Wyman, has had his method of operating, and his remarkable success in the treatment of pleurisy by that means, brought under the notice of the profession in this country, more especially through the writings of Dr Budd, and Dr W. T. Gairdner. In his work on Clinical

¹ Medico-Chirurgical Transactions. Second Series, vol. ix. 1844.

² Guy’s Hospital Reports. Second Series, vol. ii. 1844.

Medicine, Dr Gairdner has at some length discussed the propriety of operative interference in cases of pleurisy; and to some of his observations I shall have occasion in the sequel to refer.

So far as I am able to judge, it appears to me that there is a growing conviction in the minds of professional men, that, notwithstanding the reliance which is justly placed in the ordinary, and happily still more available remedies which we possess, the cases of pleural effusion, acute and chronic, are not few in which recourse to the operation of thoracentesis should be had; and in the instance of those who have employed it, a growing confidence in the remedy itself. Pleurisy, whether acute or chronic, is to be regarded as a serious, it is not unfrequently, however skilfully treated, a fatal disease. From time to time there appears to be an unwonted or remarkable prevalence of pleurisy, and it is specially at such times, as I have myself observed, that there are apt to be marked peculiarities in the nature of the cases which present themselves to our notice. Some are characterized by the violence of their onset, the very acute lateral pain, and the high pyrexia, which seem to call for speedy and very active treatment; others are unaccompanied by any marked degree of either—indeed the pain, wanting altogether the true characteristics of the typical pain of pleurisy, may be absent, and the constitutional disturbance for a time at least trivial. The latter cases are oftentimes, moreover, most insidious in their progress; and it is not of unfrequent occurrence to find a sufferer from pleurisy presenting himself at the hospital, or in some other way seeking professional advice, when, and not till then, one side—it is usually the left—is distended by effusion. There is, probably, much truth in the statement of M. Pidoux, that large or excessive effusions are apt to occur in one particular form of pleurisy very different from the ordinary disease.

Such experience is, I am persuaded, far from being unusual; and I am equally satisfied that, in the opinion of all experienced physicians, the disease in its ACUTE FORM—for of that I wish now *in the first place* to treat,—is one which tends in some cases, if not at times in many, to the unfavourable termination. M. Trousseau has very specially considered this question, and that in all its bearings. “To justify the employment of paracentesis of the chest in pleurisy with large effusion,” he remarks, “it is necessary as a first step to establish, contrary to the opinion expressed by M. Louis, that pleurisy is sometimes fatal.” Here let me briefly record not the earliest fatal case of pleurisy which I have witnessed, but one among the earliest. There were circumstances in connexion with this case which necessarily led to its making a deep impression on my mind; to these I need not now refer, except to express the conviction which, with an increasing experience of the treatment of pleurisy, and, latterly, an assured confidence in the value of paracentesis, I do with confidence, that the use of that remedy *might* have availed to save life.

CASE I.—A lady, æt. 45, the mother of several children, had enjoyed uninterruptedly good health, till the occurrence of her fatal illness, now about twelve years since. That took place as follows :—After exposure to cold she was seized with severe pain in the left side, accompanied by short cough, and difficulty of breathing. A few leeches were applied over the seat of pain within a few hours from its commencement, and when the existence of distinct pleuritic friction sound had been discovered on auscultation, repeated small doses of Dover's powder, and a mixture containing acetate of potash, were also prescribed. On the following day, the lateral stitch was found greatly relieved—in fact nearly removed—but extended dulness on percussion and considerable embarrassment of breathing were present. The pulse, previously sharp, was then very frequent and soft. The same treatment was continued, and shortly after a large blister was, in addition, applied to the side. In the course of a few days there was evidence of increased pleural effusion, afforded by still more extended dulness on percussion, by displacement of the heart to the right of the sternum, and by entire obliteration of vocal thrill and vesicular respiration sound over considerably more than the lower two-thirds of the left side of the chest. These indications were not in themselves alarming; but in the anxiety of countenance, and the frequent as well as somewhat feeble pulse, there *was* something to create anxiety. I requested Dr Begbie to visit this lady with me. On that occasion, Dr Begbie was so much struck by the anxious expression of her countenance, and by the peculiarity in the action of the heart and character of the pulse, consisting mainly in their feebleness, that he suggested the possibility of the pericardium being involved; that, in addition to pleural, there was possibly pericardial effusion. This suggestion appeared at the time very important; and although, from the feeble action of the heart and the extended dulness on percussion in the neighbourhood of the organ, perhaps otherwise induced, we had unusually little to depend upon in the way of physical signs, still the implication of the pericardium, or possibly of the heart itself, seemed at the time adequately and satisfactorily to account for the alarming symptoms. From that time the patient had at brief intervals a little brandy or wine. Towards evening of the day on which the last report was made, there occurred a tendency to syncope, demanding the freer employment of stimulants. During the night an attack of fatal syncope took place, death resulting before—being hurriedly summoned—I had time to reach the house. On examination of the body, we found a large collection of serous fluid, with a few flakes of soft lymph floating through it, in the left pleural cavity, the left lung much compressed in its lower lobe, but only partially so in the upper, which was adherent to a limited extent in front. The surface of the lung, where free, was coated with a tolerably thick layer of recent lymph. The heart was considerably displaced to the right, but the pericardium and the interior of the

organ were healthy; and, on careful microscopical examination, the muscular structure of the heart was found to be quite unimpaired. Neither in the aorta nor in the pulmonary artery was there found any clot, or any obstructive coagulum sufficient to account for the fatal symptoms: these indeed did not point to the likely occurrence of embolism, and certainly no embolism existed. This, then, was a death in uncomplicated acute pleurisy; in a case, moreover, not characterized by the existence of any excessive effusion. It is worthy of remark, that the mode of fatal termination was purely that of failure of the vital function of the circulation—a syncope, threatening in its recurrence again and again during a brief period, and then ultimately causing death. Neither is this experience in pleurisy—remarkable as it is—very unusual; indeed I feel persuaded, and in special view of the employment of paracentesis, would desire to urge this consideration, that in cases of pleurisy with considerable effusion we do well to regard a termination by syncope as one of the dangers which is to be if possible averted. Such a termination may occur suddenly, almost unexpectedly, or, as in the case now briefly detailed, may be threatened more or less imminently for a time, perhaps for days, and then unhappily take place. It is very specially in such circumstances that the performance of paracentesis is called for, and that the results from the operation are found to be so satisfactory. The following instance of acute pleurisy, treated by paracentesis and other remedial means, will best illustrate the plan which I have now frequently adopted, and invariably with the same gratifying success.

CASE II.—A. T., æt. 22, a servant, was admitted to the Royal Infirmary, under my care, in March 1864. On the 5th of that month, after exposure in washing and drying clothes, she was seized with severe shivering and vomiting, and soon after had pain in the left side, with stitch. Thus suffering, she was confined to bed and under medical treatment till the 15th, when, with considerable difficulty, owing to her weak condition, she was removed to the Infirmary. On admission, the embarrassment of breathing was great; the face somewhat flushed; the countenance anxious; pulse 130, and feeble; there was little cough and no expectoration; she was unable to assume the entirely recumbent posture, but was supported in bed with pillows in the semi-erect position; she stated that for several days she had been unable to lie on the right side. Passed a very small quantity of high-coloured urine, with a copious deposit of pink urates. On percussion, the left side of chest, in front, in the lateral region, and posteriorly, was found absolutely dull, with the exception of a very limited space in the vicinity of the left sterno-clavicular articulation and above the clavicle, where percussion elicited a partially tympanitic note. The vocal thrill over dull regions was absent; at the very summit of the lung, anteriorly, it was felt in an exaggerated degree. Distant bronchial breathing was audible along the left border of spine, but no vesicular murmur was to be heard anywhere. The timbre of voice posteriorly was

ægophonic in character. The lower intercostal spaces in the left lateral region were extended, but there was an absence of any marked distention. There was no parietal oedema of the chest, and no oedema of arm or neck. The heart's impulse was perceptible to the right of the sternum and at the summit of the scrobiculus cordis, it was feeble, and so were the cardiac sounds, which otherwise were unaffected. The respiratory murmur over the whole right lung was greatly intensified. At visit of the 16th, the patient having passed a wholly sleepless night, and taken no food, I tapped the chest, introducing a small-sized trocar, without any preliminary incision, between the eighth and ninth ribs posteriorly, in a line drawn directly downwards from the lower angle of the scapula. A clear straw-coloured and highly albuminous fluid escaped. Of this twenty-five ounces were allowed to flow; certainly no air entered the chest, although no special means for effecting its exclusion was at the time adopted; and then the canula being withdrawn, a small piece of lint, held in position by two short cross strips of diachylon plaster, was applied over the minute wound. A little brandy with water was administered to the patient during the operation, and after the withdrawal of the fluid she found that without any difficulty the recumbent posture could be assumed. Before leaving, directions were given for the application of a large blister over the left side of chest, and the following remedies were ordered:—*R.* Potassii iodidi $\mathfrak{z}$ i., aquæ destillatæ $\mathfrak{z}$ vi.—*Solve.* Sign., a tablespoonful thrice daily. *R.* Pulv. scillæ gr. i., massæ pilularum hydrargyri, pulveris ipecacuanhæ cum opio, āā gr. ij.—*M.* Mitte tales vi. Sign., one, night and morning. On the 17th, we found at visit that the patient had passed a good night, and that she felt much more comfortable, breathing with little difficulty. She had taken some food. The bowels had been moved, and an increased amount of urine passed. On examination of the chest, the region of tympanitic percussion note in front was found to be considerably extended, and a similar character of resonance was for the first time noticed above the scapular spine posteriorly. After this there was no further anxiety about the case; the iodide of potassium, and diuretic pills were continued for a few days, and then, without the latter having produced any physiological action of mercury, a mixture containing the infusion of digitalis and scopolarium was substituted for them. A second blister was also applied after the first blistered surface had healed. Gradually the dulness on percussion diminished, and the normal resonance became restored. Friction sound, very coarse in character, was audible in the left lateral region for a considerable period, but in time that also disappeared. The patient's convalescence was protracted, or rather delayed, by an attack of dry pleurisy, very distinct in its characters, on the right side; but having regained flesh, and with completely re-established health, and with very few, and these indistinct, signs of the malady which brought her under our notice, she left the hospital on the 15th June, after exactly three months' residence.

Now, in this case, the termination of which was so satisfactory, as,

at all events, to imply that the remedial means employed were well suited for the treatment of the disease, the question presents itself, was thoracentesis really requisite? in other words, was the condition of the patient at the time when the single tapping was performed, such as to make it at least improbable that other, and these the ordinary remedies, would have availed to combat the disease? This is a question which I have invariably put to myself before using the trocar, in all of the now many operations in acute and chronic pleurisy which I have performed. In the instance of A. T., the feebleness of the heart and pulse, taken in connexion with the very considerable amount of dyspnoea, led me to conclude that it was necessary to afford relief to the patient as soon as possible: that there was danger, imminent danger in practising delay. I cannot certainly affirm that in this case, any more than in others, to some of which I shall immediately refer, recovery without paracentesis would not have taken place; but I had then and still have a strong impression that the unfavourable termination was, of the two events, the more likely to occur.

The recommendation has been given by some physicians to allow a *certain* period of time to elapse from the occurrence of the effusion, during which the diligent employment of the more ordinary remedies available for promoting its removal is to be practised, and that when this period, fixed by some at fifteen days, by others at three weeks, and as long as six weeks, has been permitted to pass, then and then only the operation truly in such circumstances, as a "*dernier ressort*," or with the proved failure of all other means—the confessedly "*remedium anceps melius quam nullum*," may be had recourse to. This I submit, as has already been done by many experienced physicians, is not giving thoracentesis a fair or even decent trial; for if the remedy can be shown in careful hands to be itself free from any unusual hazard, clearly then we are entitled to claim for it an employment more dignified than is implied in its being looked to as a last resort, or at best a very doubtful means of cure. For myself, I am, in so far as this question of thoracentesis in acute pleurisy is concerned, entirely opposed to the plan of settling the exact occasion of its employment by any reference whatever to time or days. I agree in the criticism by Celsus, of one of the Hippocratic doctrines founded too exactly on the Pythagorean philosophy, "*quum hic quoque medicus*," says he, "*non numerare dies debeat, sed ipsas accessiones intueri; et ex his conjectare*," etc. Nor do I conceive that a satisfactory answer to the many questions which crop up, on anything like a candid consideration of this subject, can be obtained from a statistical investigation—that certainly may aid, it has already aided our inquiry, as in the extended experience of Dr Brady and Dr Bowditch,—but concur in the expression of Dr Gairdner, "This question is one not to be answered by statistics, but rather by the careful consideration of individual cases."¹

¹ Clinical Medicine, page 375.

There are, then, a few features of importance in the case now briefly detailed, to which I desire to call attention. The effusion in it was considerable, but certainly not excessive, for there was an entire absence of parietal œdema; and although the left side of chest was somewhat rounded, there existed no very marked bulging of the intercostal spaces. Moreover, the heart was not displaced to the extent I have often seen it in cases of very large effusion; and there still existed, at the time of the patient's entrance to hospital, a little, although that little was altered, resonance on percussion. But with a pleural effusion of fourteen days' existence, stopping short of being excessive, there was in this case a very feeble action of the heart, and a quick as well as feeble pulse. The dyspnœa was great; indeed, as orthopnœa existed, it may properly be styled urgent; but as I have witnessed larger pleural effusions than this case presented, accompanied, as might be inferred, by greater dyspnœa, and have not performed thoracentesis, but trusted to the use of ordinary remedies, so I desire here to impress the view, that the determination immediately to adopt the operation in this case was formed from a consideration of the impediment to breathing, coupled with the feebleness of the circulation. This consideration has guided me in all the instances of acute pleurisy in which I have used thoracentesis. I look upon it as the most important point to receive attention. There may be very considerable dyspnœa without marked depression of the circulation, and there may also be marked depression of the circulation without any very great dyspnœa; while these conditions may exist with a disproportionate amount of effusion. For I have a distinct impression, that in my own experience the cases calling most urgently for the employment of the remedy the judicious use of which I advocate, were not characterized by the excessive amount of the effusion nor by an extreme degree of dyspnœa.

That the tendency to syncope, observable in some cases, is not directly due to the amount of the pleural effusion, I infer from what I have myself witnessed of pleurisy, both fatal and otherwise. In the case of the lady already described, which belongs to the former category, it will be remembered that the amount of fluid found after death was not very large, and further that the physical signs during life—and this holds true of cases belonging to the second category—did not indicate the occupation of the pleural cavity to any inordinate degree by fluid. Again, in seeking to arrive at the true cause of this tendency to depression of the vital function of circulation, it is further worthy of notice that the displacement of the heart, which at first sight might not unreasonably be regarded as explaining the different experience in different cases, does not in or by itself prove sufficient; for, although admitting that altered and probably depressed function will result from its dislocation, it is consistent with my own observation that the cases in which the position of the central organ of the circulation has been

most altered have not been characterized by the occurrence of the syncope tendency. And, again, that tendency, in a most marked and even in a fatal degree, has been exhibited in cases in which cardiac displacement has been by no means great. I have thought that possibly some such cases as the former had escaped this danger, from the very circumstance that the heart, yielding to the accumulating pressure, had had its position more decidedly altered. This explanation, however, is inadequate. The tendency to the occurrence of syncope—the actual occurrence of fatal syncope in pleurisy—has taken place when the effusion has had its seat in the right side as well as in its more frequent situation, the left. In the former, the implication of the heart by direct pressure has been comparatively slight. I apprehend that the cause of this really important, because serious, event in pleurisy is to be sought not in any merely mechanical or purely dynamical disturbance,—it lies, in all probability, deeper. We may not be able—rarely do I think we shall be able—to indicate at an early or at any precise period of the disease the cases in which the depression referred to is to be looked for as more than ordinarily likely to occur. It is, however, intimately connected with those vital changes, more especially of innervation, which we know to exist in all abnormal action within the body, and still more immediately with inflammation, changes, too little regarded by those whose pathological alterations must needs be seen by naked eye or microscope, but which nevertheless modify the progress, and probably largely determine the ultimate event in cases not of pleurisy merely, but of all fatal diseases. Dr Walshe, who has been fortunate in very rarely meeting with a fatal termination in acute pleurisy, has stated, and the expression is borne out by my own observation, that asphyxia (pure), owing to the copiousness of effusion, he has never witnessed as a form of danger in the acute stage of the simple unilateral disease.¹ That such a termination as that now under consideration may take place in cases where the heart's structure has been previously unsound, is a proposition which it is unnecessary to consider at any length: that is self-evident; but the tendency is of much more frequent occurrence in pleurisy than in such comparatively rare instances. Besides, these are examples of complicated disease which do not call for consideration, as I am now treating of simple pleurisy. Already, as in the first case detailed in these observations, the occurrence of fatal syncope has been found in connexion with a heart perfectly sound in structure, and only slightly displaced by an effusion far from excessive. It has indeed been suggested, as by Dr Gairdner, that death in such circumstances may be considered to be due “to the severity of the diet, or to the treatment by depletion and digitalis, rather than to the disease.” This explanation cannot apply to the cases already noticed, nor to others which I am now anxious to record.

In the following case, the operation of thoracentesis was per-

¹ Diseases of the Lungs, page 290.

formed on account of an acute pleurisy occurring in a man upwards of seventy years of age, the oldest subject of the disease as well as of the remedy who has fallen under my notice :—

CASE III.—T. B. was a patient in the Royal Infirmary in October 1862. He was seen at an early period of his illness, and judiciously treated by the late Dr Andrew Pow; but owing to an increase in the severity of the symptoms, and some obstacles to his being properly cared for in his own house, on Dr Pow's recommendation he was brought to the hospital. On admission, he had the ordinary signs of a large pleuritic effusion on the left side. The only exceptional point in his case regards the heart and arteries. He had evidently considerable degeneration of the arterial tunics; and the heart, displaced by the pleural effusion, was also the seat of disease. A bruit indicating some obstruction at the aortic orifice existed, and that obstruction was regarded as in all probability due to some atheromatous affection of the semilunar valves themselves, or to some deposition of a similar nature at the very commencement of the great vessel. These changes, incident to his advanced years, did not certainly improve the prospects of his recovery. I know that my late friend Dr Pow regarded the condition of this old man as very critical, and did not hesitate to ascribe the satisfactory termination of his case to the treatment which was pursued. The patient, on admission, was extremely breathless, and quite unable to lie down in bed. His heart's action was feeble and a little irregular; and his pulse at the wrist, where the radial artery was distinctly visible as well as tortuous, possessed the same characters. Any exertion, even of the slightest description, aggravated the dyspnoea. The usual remedies had been employed before his admission: blisters, diuretics, and the external application of iodine. Three weeks had elapsed from the first occurrence of the malady. Looking upon this man's age and previous condition of health, as exerting a decidedly unfavourable influence on the therapeutic action of such remedies as might otherwise have received for a time at least a renewed trial, and believing that neither his age nor infirmities could be regarded as in any sense barriers to the operation, I introduced a small trocar between the eighth and ninth left ribs in the lateral region, and drew off nearly thirty ounces of pale, clear, albuminous serum, in which a fibrinous clot formed very speedily on cooling. The immediate result was most assuring, and the subsequent progress of the case such as to satisfy me that a right remedy had been used. As in the former case, the tapping was single; the use of the acid tartrate of potash, in the form of an electuary, with treacle, the application of a blister over the left side, with a moderate allowance of stimulants, were the only other means employed. Admitted on October 2d, the old man was discharged with an expanded left lung, and healthy breath-sounds audible over its whole surface, on the 22d November.

I need not in this case again particularly inquire, Was the simple operation necessary? That question I have endeavoured to answer, in part at least, in the observations which succeeded the details of the former case; and I will only add here, that if "the end justifies the means," the case of this old man and the treatment it received are well worthy of attention. I might speak at length of the great relief he experienced on the withdrawal of the first few ounces of fluid, and of his unwillingness that I should, by the removal of the canula, interrupt the flow. I do not do so, because present relief is one thing, certainly not unimportant, but permanent benefit and successful treatment are what I desire to establish as the results of a judicious employment of thoracentesis.

The record of this second instance of a single successful tapping in acute pleurisy leads me, however, to make one or two further observations. It will be observed, that in neither case was thoracentesis employed until the ordinary treatment had been put in execution and had received a proper trial. I am most happy to express my great reliance upon and confidence in the remedies which are generally employed for the treatment of acute pleurisy in all its stages, including moderate bloodletting, specially in the earliest stage, when lateral stitch is very severe and inflammatory fever high; likewise in febrifuge medicines, especially those exerting a sudorific or diaphoretic action. When effusion has occurred and fever still prevails, I use the salts of potash, the nitrate, acetate, or bicarbonate; while in young and vigorous constitutions I sometimes select calomel and opium, or blue pill with squill and digitalis,—remedies which I have often seen most useful; or if a rheumatic or gouty taint be present,—as is very frequently the case,—I combine with these remedies lemon-juice or colchicum, or aconite, or actæa. When the more ardent fever has subsided, I have recourse to blisters, large and frequently applied, and to the internal administration of iodine, particularly of iodide of potassium, or the acid tartrate of potash. Rarely disappointed in these remedies, there soon comes a time when, with a steadily if not rapidly diminishing effusion, a change to the use of the liquor iodi compositus, or the syrup of the iodide of iron, or one of the simple salts of iron, or of quinine or cinchona, is found to be beneficial. Placing, therefore, great reliance in the use of these remedies, I do not wish it to be supposed that the cases are otherwise than a small minority in which the withdrawal of the fluid from the pleura in part, or as nearly as possible in whole, is to be considered desirable, or, to use more correct language, is called for.

During the period when the hospital cases already detailed occurred, there were many cases of severe pleurisy successfully treated by some one or other of the remedies named,—cases which I should as soon have thought of treating by the trocar as I should now be willing to forego the use of that valuable instrument in any case resembling those of the young woman

and the old man, already detailed. But, again, in both of these cases, I wish it to be distinctly noted that a limited amount of the fluid contained in the pleura only was removed; in both instances a large quantity, probably a much larger quantity than that withdrawn, remained. Now, I think there is a very great advantage in this particular mode of procedure: for, *first*, while there are no doubt in different cases somewhat different reasons for having recourse to the operation, there is one reason common to all, namely, the desire to give immediate relief to the more urgent symptoms,—to make, if possible, the breathing easier, and the pulse, or rather heart's action, steadier. I believe that these important issues can, in the great majority of cases of acute pleural effusion, be secured by the removal of a limited—even a very limited amount of fluid; in the instances already recorded, twenty-five ounces, and about thirty ounces respectively were allowed to flow; and then the change in the condition of the patient to the better being conspicuous, the further abstraction was unnecessary; for, *secondly*, experience shows, that not only are the urgent symptoms thus relieved, but that the apparently direct consequence of the operation is a busy action of the absorbents and secreting organs, by which the remaining fluid is speedily carried off. This result is in some instances really quite astonishing. In one case, for example, in which the symptoms of pleurisy were very severe, and the effusion excessive, but where for certain reasons I had wished not to have recourse to the simple remedy a day too soon, I was at length compelled to do so, from the extremely scanty secretion of urine, and the apparently entire failure of the foremost diuretics to stimulate the kidneys. I withdrew, by Bowditch's syringe, a very large quantity of clear fluid, and within twenty-four hours the patient passed a large chamberpotful and a-half of urine. This powerful diuresis continued for some days, and the pleural effusion had in that time completely disappeared. But, *thirdly*, the operation of thoracentesis, performed in the way and for the purpose described in the two cases which I have detailed, is really a very simple and an eminently safe means of treatment. There is no risk whatever in such circumstances of the entrance of air, and therefore a simple trocar and canula are alone required; an exhausting syringe or valvular apparatus is quite unnecessary. Selecting a dependent position, and having with a little force depressed the intercostal space about its middle, you introduce the trocar boldly where dulness is well marked; and, as you remove it, the fluid passes through the canula; the limited portion you desire to remove is, properly speaking, determined by the influence the withdrawal of the serum has upon the patient's condition. You carefully watch that, and whenever the breathing is decidedly relieved, and the pulse is improved in strength,—for these events occur together,—you remove the canula. Thus operating, I have never known any air to enter.

While alluding to the possibility of the entrance of air during the performance of thoracentesis, let me say that the dangers of this occurrence have, in my opinion, been greatly exaggerated. I quite agree with an able American writer, the late Dr Swett, that "experience proves that the admission of air into the chest in these cases is attended with no serious inconvenience." In operating in some sub-acute and chronic cases, and before I had the advantage of possessing the syringe of Wyman and Bowditch, occasionally a little air entered, but I never saw this give rise to any unpleasant consequence. The air was speedily absorbed, and the signs of a very limited pneumothorax, which were at first present, soon ceased to exist. No importance, I believe, is to be attached to the notion entertained by some, that the presence of air in the pleural cavity favours the decomposition of the contained fluid. No doubt, the fluid drawn off from the chest in cases of hydro-pneumothorax is sometimes fetid; but that that condition is not caused by the mere presence of air is proved by the circumstance that, on post-mortem examination in long-standing cases of hydro-pneumothorax, and of empyema with pneumothorax, it is not of unfrequent occurrence to find no fetor either of the air or fluid such as must have otherwise existed before death. I have satisfied myself that the fluid withdrawn from the chest by thoracentesis may be kept with little change for many days. I have kept it for a fortnight freely exposed to the air, and in contact with animal matter removed along with it from the pleura, without its undergoing any decomposing change. The considerable presence of chloride of sodium in such fluid may probably tend to its lengthened preservation of both normal appearance and smell.

Let it be granted, however, that the entrance of air should be if possible prevented,—and in the treatment of acute pleurisy requiring thoracentesis that can be done by the employment of the American syringe,—or if, as I believe in the vast majority of instances is alone necessary, it be intended to remove only a limited quantity, let the trocar be introduced in a dependent position of the chest, where the signs of effusion are unequivocal. I have of late generally, as Dr Bowditch mentions is his usual practice, entered the trocar on the posterior aspect of the chest between the seventh and eighth or eighth and ninth ribs, in a line leading directly downwards from the lower angle of the scapula. In acute cases of pleurisy, adhesions interrupting the proper performance of the operation rarely—in my experience never—exist in that situation. There is truth in the axiomatic statement of Hippocrates, that the sudden withdrawal of fluid from the shut sacs is attended by danger. I have seen a patient nearly faint—and at such a time a faint is a very dangerous event—when the fluid was rushing out quickly from the pleura through a pretty large canula. We see this accident occur in paracentesis abdominis, if the patient be raised in the sitting posture, and the fluid be suddenly or very quickly withdrawn. It is prevented by keeping

the patient in the recumbent posture, and using a small trocar ; and these measures render the employment of substitute pressure by the broad sheet unnecessary. In the removal of a limited amount of fluid from the chest, I have never seen anything like faintness ensue, for generally, on the contrary, the strength of the patient immediately improves. Having given the particulars of the case of an old patient, the subject of acute pleurisy, I may briefly refer to a few other instances, and, first, to that of a young child.

CASE IV.—A girl, *æt.* 4, was tapped on an evening in the month of June 1863. I had been asked to see her through the kindness of Professor Simpson, and on account of thoracentesis being in his opinion required, owing to the severity of the dyspnoea and great feebleness of the little patient. She had been suffering from pleurisy of the right side for several days, and the effusion had somewhat suddenly undergone increase, giving rise to considerable aggravation in the character of the symptoms. In the presence of Professor Simpson and Dr Black, the trocar was introduced between the seventh and eighth ribs in the lateral region, and by means of Bowditch's syringe nearly eighteen ounces of a clear serum, in which a coagulum of fibrine speedily formed, were drawn off. I saw the little patient on the following morning in a greatly improved state, and had afterwards the satisfaction of learning from Professor Simpson and Dr Black that the recovery was complete. I have already alluded to the generally very successful results of thoracentesis in young children, and this remark applies to cases of acute pleurisy and empyema as well as of chronic effusion. I have employed this remedy in the cases of several children besides the one just recorded, particularly in a little girl of three years, and in another little girl of five, both of whom made rapid and complete recoveries. I should certainly not be deterred from the use of thoracentesis by the consideration of the mere tenderness of the child ; however young, if the case itself called for the operation, I think it should be employed. Indeed, considering the difficulty of administering ordinary remedies to very young children, it appears to me that they ought specially to be regarded as suitable subjects for the operation.

CASE V.—Of all the cases of acute pleurisy, treated by thoracentesis, which have fallen under my own care, the one in which I felt most satisfied, not only as to the urgent necessity there existed for immediate interference, but after the operation had been had recourse to, of the remedy having truly availed to save life, was that of a youth admitted to the Royal Infirmary, under my care, in October 1864. He was suffering from a very large pleural effusion on the left side, the form of which was greatly altered, being rounded with obliteration of the intercostal spaces, and great prominence. The side was uniformly dull on percussion, and the heart much displaced to the right ; there was likewise a little parietal œdema over the affected

side of the chest. Orthopnœa, and great feebleness of the pulse existed. Considering this patient to be in imminent danger, a view likewise taken by my friends Dr Wilkinson, late of Tranent, and Dr Alexander Robertson of Queenscliff, Australia, who happened to be in the ward at the time, I at once introduced the trocar, and removed about forty ounces of highly albuminous serum, which, as in other cases already detailed, only to a greater degree, formed into a jelly on cooling. There was immediate relief from the operation, the patient assumed the recumbent posture, and fell into a tranquil sleep. Blisters and diuretic remedies were subsequently employed, and the youth made an excellent recovery, leaving the hospital within six weeks of his admission.

It will be observed that in this case, as in the others, a single tapping was alone required: the practice pursued being that to the excellence of which I have already borne a strong testimony. In this instance the pleura was packed with fluid, and I believe in such a condition as to make it very unlikely that any internal remedies—such had been judiciously used—could act favourably, or indeed act therapeutically at all, until the removal of a portion of the fluid permitted them to do so. Speedily the passage of an increased amount of urine followed the withdrawal of the effused fluid from the chest. Immediately on the performance of the operation, expansion of the compressed lung occurred, ascertained by the clear percussion note over the summit of the chest, anteriorly and posteriorly, as the lad sat in bed, and by the distinct though feeble respiratory sound on auscultation. The patient was disturbed by cough for a day or two, short in character, and attended by the expectoration of a little clear phlegm. This will generally, I believe, be noticed to succeed the performance of thoracentesis in both acute and chronic pleurisy, and more particularly in those cases where the lung, previously compressed, rapidly expands. The presence of a little catarrhal affection of the lung itself, certainly not to be wondered at, is probably the determining cause of the forcible expiratory effort. One other remark is called for by what we observed in this youth's case, as well as in other instances, indeed less notably in his than in others. The lung seemed to expand steadily, for a time even rapidly; percussion resonance, normal in character, returned to the anterior and upper part of the affected side, but in the dorsal region, and still more, the axillary and lower lateral regions, dulness continued marked. I have known this to hold true in cases of pleurisy thus treated for many months, indeed for years. It need, however, scarcely be observed that the same dulness on percussion, with diminished expansive power, impaired vocal thrill, and enfeebled respiratory murmur, are found long continued, or lasting permanently, in many cases of severe pleurisy treated by ordinary remedies, and which have made a satisfactory recovery. In such instances a considerable deposit of lymph has in all probability taken place, and it is

to the existence of that inflammatory product that the physical signs, as well as the physical change in the condition of the chest just noted, are to be ascribed. It is remarkable to how great an extent that dulness and those other signs may in course of time be found yielding, or even entirely passing away.

In at least four other instances of acute pleurisy, very similar in their character to those already detailed, I have thought it necessary to have recourse to thoracentesis. In one of these four, a young gentleman of twenty-four years of age, the pleural effusion was associated with a peculiar swelling of the corresponding limb, both in the leg and thigh. This swelling was not of an oedematous character, but firm, and resembling a good deal the condition of the extremity when affected by phlegmasia dolens. In two cases of typhus, and in one of enteric fever, one of the subjects being a gentleman of about twenty-five, and the other two females in hospital, I have witnessed a precisely similar affection; but Dr Begbie informs me,—and the observation appears to be very interesting,—that in three cases of pleurisy occurring to him within a limited period of time, he has observed the swollen limb corresponding to the pleuritic side. In one of these the lymphatic disturbance in the leg—for such it would appear to be—preceded the pleurisy, in the others was consequent on it. To a few particulars in the history and treatment of one other example of acute pleurisy I shall now allude.

CASE VI.—A married woman, in very comfortable circumstances, was the subject of a very severe pleurisy. The disease occurred on the left side, and was ushered in by considerable constitutional disturbance and great pain. The distinctive signs of pleurisy were early discovered, and before any fluid effusion had occurred, the patient had been placed under appropriate treatment. This, however, did not avail to prevent the serous accumulation occurring and augmenting, until the cavity was apparently completely occupied, and the parietes were much distended. Three weeks of employment of ordinary measures were followed by no improvement whatever. The condition of the patient had indeed now become critical; there was great fever, hot skin, very frequent pulse, much breathlessness, and general derangement of system. In these circumstances thoracentesis was performed; and with Bowditch's syringe I removed upwards of sixty ounces of straw-coloured serum. The whole fluid passed into the condition of a really firm coagulum within a few hours of its removal from the chest. From the day, I may say the hour, of its performance, this patient improved, and nothing occurred to retard her recovery, which has been rapid as well as perfect.

I shall conclude these observations, on the treatment of acute pleurisy, by a few remarks specially applicable to the condition of EMPYEMA, as it occurs in an acute form. When the effusion of fluid within the pleura is known or suspected to consist of pus, there is then much less room for difference of opinion as to the

proper plan of treatment. Purulent fluid is far from being readily absorbed from the pleura, so that in such cases the advice of Dr Watson will probably be found consistent with the view generally entertained by the profession, that "whenever (no matter how we ascertain the fact) the effused fluid consists of pus, it should be let out." If thoracentesis is not performed at an early, or comparatively early period, the probability is that the pus will find a way of exit for itself, either through the chest walls—when the opening is far more likely to prove fistulous and permanent than when incision or puncture is made—or through the lung, or less likely through the diaphragm into the cavity of the abdomen (case of Carroll, in Ward 7, Royal Infirmary, 1857), or in some other manner. The cases of acute empyema which are met with, vary in their character and course not a little. Some are the result, after a time longer or shorter, of ordinary acute pleurisy; others, and these are, as a general rule, less promising, appear to have been attended by an effusion, which, from the very commencement, has been puriform or even purulent. In such there is little formation of lymph, and pleural adhesions rarely result to any extent. That the removal of pus in large amount from the pleura in cases of acute empyema may be followed by results as satisfactory as those which have already been described as succeeding the withdrawal of serous fluid, my own observation and experience have convinced me. It must, however, be held in remembrance that such cases as are from the first attended by purulent effusion, and those also, although in less degree, in which the fluid has soon become pus, are more serious in their nature, accompanied by fever of a truly hectic character capable of rapidly undermining the patient's strength, less amenable to treatment of all kinds, and are very frequently connected with disease of the lung. I am more suspicious of empyema on the right side being connected with pulmonary disease than when the effusion is seated in the more ordinary position on the left. Possibly this more frequent connexion of tubercular disease of the lung with empyema on the right side may in part if not wholly account for the fewer recoveries after the operation of thoracentesis on the right side as recorded in the statistics of Dr Brady. I will briefly relate the particulars of one remarkable instance of acute empyema treated by thoracentesis, and in doing so shall be led to offer a few observations on the special points of difference between this affection and the one we have already considered.

CASE VII.—Eight years ago, G. P., æt. 25, a painter, became my patient in the Royal Infirmary, suffering from an extensive and recent pleural effusion on the left side. There was that about the appearance of this man which induced me to believe that the nature of the fluid was purulent; he had hectic fever, dusky countenance, and anxious expression. Moreover, he had suffered previously from cough, and had other chest symptoms at the same time; he

had lost one brother from phthisis, and during his residence in the hospital another brother died of the same disease. In this case the effusion was very large, altering the form of the chest, giving rise to considerable distention, and to cedema of the thoracic parietes, as well as of the upper part of left arm. The heart was much displaced, and the left side of the chest universally dull. After fully a week's employment of ordinary remedies, there appeared a tendency to pointing in the lowest part of left lateral region, and in that situation I accordingly introduced a trocar, removing 120 ounces of healthy-looking pus, free from smell. The dyspnoea, which had been very great, was much relieved by the operation, and the condition of the chest not a little improved. In ten days the re-accumulation of pus necessitated the performance of thoracentesis for the second time, when about 80 ounces were withdrawn. So matters continued for several weeks, till I had myself, on nine different occasions, removed nearly 1000 ounces. About five months after the original perforation by the trocar, I made a pretty free incision through the parietes and into the pleura, between the eighth and ninth ribs in the lateral region: pus flowed freely into a soft sponge, and the sponge made hollow in the centre, being reapplied, the pus was thus for many days collected. The patient thereafter left the hospital and resided for some weeks in the country. He visited me from time to time, and calculated that from the period of leaving the hospital fully another thousand ounces of pus had escaped. In the course of other six months the wound healed, and the patient was able to resume his occupation, at which he has continued ever since. The condition of his chest was for a lengthened period most satisfactory; from the time the opening closed until recently, the left lung was fully expanded, and occupied the pleural cavity so completely, as to make it a difficult task—in which many educated students have failed—to say, from a simple inspection, on which side of the chest his empyema had existed.

To an interesting circumstance in connexion with this case I may here allude. An aduncated appearance of the nails, and distinct clubbing of the fingers on the affected side took place during the period the patient was under observation. This change became so marked, and was so much more conspicuous in the left hand than in the fingers of the right, that I had, through the kindness of Dr Frederick Steell, a cast of the hand made, in which the peculiarity in question was well exhibited. A reference to the occurrence of clubbing of the finger ends on the diseased side in empyema is made by Dr Walshe, and by other writers. Dr Walshe remarks that this change "is sometimes strikingly marked." This, however, is not the point to which I have specially to call attention, which remains to be noticed, it is this, that after the lung underwent expansion, and the purulent discharge diminished, and still more decidedly, when the lung had fully expanded, and all passage of pus had ceased, the clubbing of the finger ends became much less marked, and for many

months so continued. Latterly this man has had pulmonary symptoms, which have occasioned a little anxiety: he has repeatedly spat blood, although never in large amount, and the cough, which had entirely disappeared, has again returned. With these symptoms there have presented themselves signs of tubercular deposition in the summit of the right lung, and the fingers of both hands have become clubbed. Nevertheless, he has, with occasional interruptions, been able for his work as a carriage painter; and since his attack of empyema, he has married a wife and begotten one child, healthy, and in every respect well-conditioned.

I have spoken of collecting the pus in a sponge applied over the opening in the thoracic parietes. This plan answers well both in adults and in children, in whose cases it was first recommended by Dr Brotherston of Alloa, in a paper giving an interesting account of three instances of empyema occurring after scarlatina, treated by paracentesis.¹ Dr Brotherston applied the sponge while the canula was still retained; and this—following his suggestion—I have also done, and found most useful, keeping the instrument effectually in its place, which, owing to the alarm and restlessness of very young patients, is otherwise very difficult.

The difference between cases of acute pleurisy, the effusion in which is serous, and cases of acute empyema, is not such as to make the rules which should guide us in the treatment of the former, otherwise than applicable to the latter. I am prepared to recommend the employment of paracentesis in all cases, where there exists such an association of symptoms,—but very specially the tendency to failure of the circulation,—as already has been so much insisted on, and that without any reference whatever to the nature of the effusion. In the fatal cases of acute pleurisy, which I have seen, and in almost all of the instances of acute pleurisy treated successfully by thoracentesis, either by myself or others known to me, the effusion has been still serous. This experience, however, it is quite possible, might readily undergo a change, for it is only probable that in cases of purulent effusion the symptoms which seem to call for the operation are as likely to be present as in those of the more simple serous effusion. Indeed, the hectic fever and more profound constitutional disturbance which accompany empyema, must be held as likely to cause a predisposition to their occurrence; but with this preliminary statement, I do not consider it inconsistent very specially to urge the propriety of thoracentesis in cases of acute empyema. If the effusion is ascertained to be purulent,—I think the sooner it is removed the better. In effecting this, being desirous to prevent

¹ Monthly Journal of Medical Science, 1853. So interesting are these cases that M. Trousseau is found quoting one of them,—“Je vous demande la permission (he says, in the first volume of the Clinique Médicale de l'Hôtel Dieu), de vous lire un de ceux (empyèmes purulents scarlatineux), que le Docteur P. Brotherston a publiés dans le Monthly Journal.

the entrance of air, it is well to employ the American syringe. That a satisfactory recovery may take place without the employment of that instrument, the case of G. P., now detailed, and the many instances of successful treatment of empyema by the ordinary operation of paracentesis which have been recorded, sufficiently prove; but as no danger attends the use of the syringe, and as a conceivable source of accident is by its use removed, while the statements of Dr Bowditch regarding its employment are so encouraging, I now prefer in all cases of empyema to remove the fluid by that means.

I have stated that we are not to look to statistics for an answer to the question when thoracentesis is to be performed; but we may, on the other hand, with reason accept a satisfactory reply from that method of inquiry, on some points of no small interest bearing on the more general question, for example, how the quality, and how the quantity of the effusion, influence recovery after thoracentesis. The latter part of this query may be answered in a single word: when the effusion has been very large, when it has been excessive, recovery has often occurred; but the success of the operation—and this has more particularly reference to empyema—is greatest in cases not marked by the largeness, but by the comparative smallness of the effusion.

As regards the quality of the fluid, there is reason to believe that a more extended and careful investigation may lead to some important conclusions. Hippocrates, as we have already seen, attached a distinct prognostic value to the nature of the pus, and this has generally been done from his day down to the present time. The statements of authors are, however, so contradictory as to render renewed inquiry necessary. Dr Brady of New York, for example, gives the results of 132 operations collected from various journals as follows:—Pus in 52 cases, of which 37 recovered, 2 were relieved, and 13 died. Serum in 59 cases, of which 29 recovered, 12 were relieved, and 18 died. Sero-pus in 8 cases, of which 5 recovered and 3 died. In 13 the nature of the fluid was unknown, and of these 10 recovered and 3 died. Dr Brady's results may be expressed in this way,—that of cases of empyema in which thoracentesis was performed, about 25 per cent. died, or 1 in every 4; while of the cases in which the effusion was serous, about 30 per cent. died, or 1 in little more than every 3.

But turn from this picture, which I cordially agree with Dr Walshe would not justify us in regarding thoracentesis as “among the valuable gifts of surgery,” to that presented by Dr Bowditch. In 26 out of 75 instances serum was drawn, and 21 of the 26 got well. Pus was found in 24 patients. Eight got well, 7 died, 9 were *relieved* one or many times, but they had either a very tedious illness, terminating usually in phthisis, or fistulous opening, or a “doubtful result.” I am unwilling, with my present limited experience, to refer to statistics at all, but there can be no impropriety in mentioning,

that in no case in which serous fluid was found, has the result been other than a satisfactory recovery. The little girl to whose case I alluded as having been visited by Professor Simpson, is now, as I have understood, at the distance of nearly three years from the operation, sinking from disease of another nature altogether, and seated in a different part of the body. One young gentleman, on whom I operated nearly a twelvemonth ago, is still far from strong, but his want of strength—and I am happy to think he is regaining strength—is not so intimately connected with his attack of pleurisy, as to forbid me stating his recovery from that disease to have been satisfactory. In the remaining cases, and they exceed a dozen in number, in which serous fluid has been removed, there has been no untoward termination.

It is otherwise where pus has been found. In cases of acute empyema, in which there has been great reason to believe that the lung had been involved in tubercular disease, I have witnessed a fatal termination within a day or two, or a few days, after the operation. In these instances, I do not believe for a moment that the performance of thoracentesis accelerated the fatal event. On the contrary, having seen the wonderful degree of relief to most urgent symptoms experienced by the patients, and having heard their own expressions of thankfulness, I am disposed to think that in such circumstances, although a doubtful, as regards ultimate recovery, it was the best remedy to employ, and that, consequently, it was demanded. For as a palliative remedy, I am indeed prepared to recommend the operation, in the circumstances now adverted to, as well as in cases of hydrothorax, the result of organic disease. In two cases of this kind, the remedy afforded great relief. In one of these I tapped the pleura ten times, in the other twice. The former case has been published in Dr Beale's Archives of Medicine.¹ The latter was seen by Dr Begbie and Sir James Simpson, with whose concurrence thoracentesis was performed. The fluid which escaped was sanguinolent—dark red in colour. Thus, the nature of the case, which had been conjectured, was established. On three occasions I have witnessed such fluid escape, and all of the three patients have died. I must, however, add, that the presence of a little blood, which after the fluid has been for some time in repose, falls to the bottom of the containing vessel, and leaves the great body of the fluid clear, is not by any means an unfavourable index. In such case the blood has escaped from the parietal or pleural wound. Neither do I think the presence of a little blood is to be regarded as serious, where a free coagulation of the mass of fibrine entangling the blood corpuscles takes place; it is the thin watery fluid of low density, and wholly as well as uniformly coloured, that I should dread to find: for that marks the case as one of malignant disease, either of the pleura alone, or of the lung and mediastinum as well.

I have now to direct attention to the employment of thoracentesis

in cases of *chronic pleurisy*, and this I shall do by relating in the first place the history of one or two cases.

CASE VIII.—W. H., æt. 28, by occupation a tanner, became affected in March 1864 with difficulty of breathing, hard dry cough, night sweats, loss of appetite, and great loss of strength. In the course of three months he was so much reduced in strength, and so breathless, that he could scarcely walk more than a few yards without taking a rest. During the summer there was for a time a diminution in the severity of these symptoms, but as winter advanced, he felt himself getting worse than ever, had great difficulty in lying down, and could not maintain the recumbent posture except on the left side, with the head and shoulders much elevated. At this period, and up to the time of my seeing the patient, he suffered greatly from palpitation of the heart on the right side. He continued to lose flesh and strength, although taking, under medical advice, cod-liver oil, and having his chest rubbed with the same. This man came under my notice in the month of August 1865. Calling at my house, which he was able to reach only with great difficulty, and with the assistance of his brother, I was struck by his emaciated and worn-out appearance. He was very breathless, and gave me the impression, as I looked at him, that he was labouring under phthisis in an advanced stage. Upon proceeding to examine his chest, however, the cause of his symptoms was found to be a large accumulation of fluid in the left pleura, displacing the heart to the right to an extent I have never seen surpassed; depressing the left lobe of the liver; and causing great prominence of the intercostal spaces, entire immobility of the chest on the affected side, absolute dulness on percussion, and total suppression of all respiratory sounds. A slight degree of cedema of the chest on the left side existed. I learned from the patient, that up to the commencement of his present illness, he had uniformly enjoyed good health; that his immediate relatives were all healthy. Further, that during his illness he had been repeatedly blistered, and had taken from first to last a large amount of medicine. He had the impression, and this was shared by his brother who accompanied him, that the disease under which he laboured was consumption; he had been so frequently told so, that he could not doubt it; still, he thought it strange that he should have no spit, and no constant cough.

Having formed my own opinion of his case, I expressed it to him, and on the following day commenced the treatment, by drawing off, with a small trocar and canula, about twenty ounces of clear dark straw-coloured serum, having a density of 1030, highly albuminous, and containing a large amount of chlorides. This limited tapping greatly relieved the breathing, but it was far more useful than that; it convinced me, that although the effusion had been in existence from March 1864 to August 1865,—nearly eighteen months,—the compressed lung was still capable of expansion, for upon careful

examination of the chest after the removal of the fluid, I found improvement in the percussion note, near the sterno-clavicular articulation, and, on auscultation, heard a distinct though distant murmur of breathing. Accordingly, being assured, by this simple preliminary tapping, of the expansibility of the lung, I drew off, within a day or two thereafter, upwards of one hundred ounces of fluid. This was done with a common trocar, but no air was permitted to enter. The opening was made posteriorly between the eighth and ninth ribs. This operation, performed in my own house, and without assistance, gave immense relief, and for the first time the patient slept on his right side, and with the head raised in bed only to the usual extent. A blister was applied over the side, and a little iodide of potassium and acid tartrate of potash prescribed.

In five weeks there was an accumulation of fluid to such an extent as to make it desirable to repeat the thoracentesis. That was done, and about sixty ounces of fluid were removed. The patient now desired to go home to Stirlingshire, and this I permitted, after receiving his promise to return in the course of a few weeks. He did so; I again tapped him, removing about forty ounces. Again he returned home, and again came back in six weeks according to promise, having meantime begun work as a shoemaker. I tapped him for the fourth time; drawing off about thirty ounces. At intervals, varying from five to seven weeks, I have tapped him three times since; the last occasion was on the 21st April, when, with Bowditch's syringe, I could only get away eight ounces. There was no more to come; and I have every confidence that another tapping will not be required. The man has gained nearly four stones in weight, eats well, sleeps well, has walked several miles without fatigue, and his breathing is, to use his own words in a letter I lately received from him, "as good as ever it was." This is a most satisfactory case, and yet I believe it would be far from being remarkable, if physicians placed that confidence in the efficiency of thoracentesis as a remedy in chronic pleurisy, which, in my opinion, it justly deserves.

Let me ask attention specially to a few features of interest suggested by this case, as now briefly recorded.

1st, The fluid was still serous, at the distance of sixteen months from the commencement of the illness, and it continued so, changing a little in colour, becoming pale, and losing in density, till twenty-five months after the attack commenced, when its formation may be said to have ceased. I have known the fluid to remain serous for a much longer period, indeed, for several years. It did so in the case of a sailor, who was under my care in the hospital eight years ago. I tapped this man's chest on three occasions, removing between fifty and sixty ounces of serum at each time, dark in colour, and free from any smell. The first quantity removed was found to contain cholesterine in considerable amount. The man, finding himself much relieved, insisted, greatly to my

regret, on leaving the Infirmary, and so the subsequent progress of his case is unknown to me. That the right lung—his was a pleural effusion on the right side—had expanded to such an extent as to make it hopeful that the case might terminate as favourably as the one now related, I was convinced, but I am equally sure, that after a time and for a considerable period the fluid would slowly re-accumulate, rendering repeatedappings necessary.

2*d*, I have mentioned that blisters were used, and that diuretic as well as deobstruent remedies were employed after the performance of thoracentesis. Such cannot be expected to act with the same degree of energy as when used in like circumstances in the acute effusions; but, still, I believe their action to be far from wholly inert; used independently of tapping they are useless. Again and again I have seen them employed in cases of chronic pleurisy, diligently and for a lengthened period, but never can I say that any decided benefit resulted.

3*d*, The tapping earliest performed in W. H.'s case was tentative. I ask attention specially to this expression—for had the lung not given evidence of expansibility after its employment, I should either have adopted a different practice, as in other cases, or on subsequent occasions used the measure as one purely palliative. The lung did, however, expand, affording evidence that after many months of compression the return to a healthy condition is quite possible. To attempt either in the case of a chronic serous effusion, or in that of an empyema, the removal of other than a moderate quantity of the effused fluid, until the capability of the lung undergoing expansion has been ascertained, is, I think, a dangerous practice. True, it is only by strong suction power, which in such circumstances is not safe, that we are able to remove any very considerable amount; but, I repeat, a safer and better plan is to watch carefully the extent to which pulmonary expansion occurs, and repeat the operation according to circumstances. This also was the method pursued in the following instance.

CASE IX.—D. B. was a patient in the Royal Infirmary in 1863, suffering from chronic pleurisy on the right side, of several months' standing. After the repeated application of large blisters, and the use of iodine and mercury without appreciable benefit, a tentative thoracentesis was performed, and the lung having evidently undergone expansion, the operation was repeated. This plan was pursued for some months, thoracentesis being performed by means of a small trocar at varying intervals, and blisters, as well as iodide of potassium, being likewise used. The result was most satisfactory: the patient, after nine months' residence in hospital, was dismissed with a lung greatly expanded, and in a state of health permitting him to return to the performance of his duties as a warehouseman, from which he had for many months been compelled to desist.

It is not, however, in all cases of chronic pleurisy, and certainly less frequently in empyema, than in instances of simple serous

accumulation, that results so satisfactory follow. I have twice, in cases of chronic empyema, found death to ensue after repeated tapplings,—the patient gradually becoming more and more hectic, and ultimately sinking through a gradual asthenia; and in a third case, a permanently fistulous opening having been established, the same train of events occurred. This was, however, a case of empyema complicated with serious pulmonary disease on both sides of the chest.

I have had too little experience of the influence of injections into the pleura to be able to express any confident opinion regarding their value. In one case of empyema, with very offensive fetid discharge, the injection of a little chlorine water, freely diluted, had certainly a good effect in controlling the fetor. In another case of very chronic empyema, limited in extent, I injected, after the manner of the late M. Aran, a little tincture of iodine. The immediate effect seemed to be satisfactory; but the patient, a man in hospital, requested his discharge a short time after the treatment was put in execution, and the result was alike imperfect and unknown. Both Aran and Trousseau, however, as well as other physicians, have borne testimony to the efficacy of injections with iodine.

Another expedient, of which, although I have no personal experience, it appears much may be made, is the drainage tube. In the hands of Dr Goodfellow, of London, and more recently of Dr Banks, of Dublin, Dr George Kidd,¹ and other Irish physicians, the introduction of a drainage tube, by permitting the fluid in the pleura to run off as soon as formed, has, undoubtedly, in acute cases of pleural effusion—and also in chronic empyema with fistulous opening, in which the tube has favoured the freer escape of pus—rendered eminent service.²

I have not thought it necessary to discuss the employment of thoracentesis in chronic pleural effusions at the same length as that to which my observations on this means of treatment in the acute effusions into the chest have extended, believing that there is by no means the same difference of opinion in the minds of physicians regarding the former, as unquestionably exists in relation to the latter.

My belief is, that in many instances of chronic pleural effusions, it will be found an available means of cure; that in others, judiciously employed by itself, or aided by injections, or by the use of the drainage tube, it is capable of greatly alleviating the distress and suffering of not a few cases of this serious chest disease.

¹ See his interesting paper in the *Dublin Medical Journal* for 1865.

² The drainage tube will further be specially useful in those cases in which the compressed lung is incapable of expansion. By its means the fluid can be completely removed, and so a contracted side—always preferable to an abnormally distended side—will result.

ARTICLE III.—*The Convolution of the Human Cerebrum Topographically Considered.* By WILLIAM TURNER, M.B. Lond., F.R.S.E., Senior Demonstrator of Anatomy in the University of Edinburgh.

[The following description is based on a Lecture delivered at the request of the Members of the Royal Medical Society, in their Hall, March 9, 1866.]

FROM an early period of anatomical research, the convolutions of the human cerebrum have been regarded as the parts of the nervous system most intimately associated with the intellectual processes; and their structure, connexions, and mode of arrangement have at various times had bestowed on them a large share of attention by the anatomist and physiologist. Not only by the older writers, but up to the end of the last century, it was customary, in describing the convolutions, to speak of them as if they had no constant arrangement—a mere maze, a riddle, a chaotic mass—and to figure their folds like the coils of the small intestine, with which, indeed, they were not unfrequently compared. Even Vicq d'Azyr,¹ who laboured so assiduously at the elucidation of the structure of the brain, was not able altogether to free himself from this conventional mode of delineation, though in the explanation to his first plate he states that the convolutions were drawn from nature. From the beginning of the present century, a greater amount of care has been bestowed on their representation, and various attempts have been made to unravel their complexities and to discover some order and method in their arrangement. Sömmerring, Gall and Spurzheim, Burdach, Rolando, Arnold, Cruveilhier, Leuret, Valentin, Foville, and Huschke, have all contributed by their labours various important facts to this department of anatomical inquiry, and have assisted in showing that some of the convolutions possess in all brains well-defined positions and relations.

So long, however, as anatomists confined themselves to the study of the convolutions in the fully developed cerebrum, many difficulties lay in the way of obtaining an exact conception of their mode of arrangement. For, in the course of development, not only are the convolutions, which first appear, to a greater or less extent subdivided by secondary fissures, but these secondary sulci, as well as some at least of those which mark the primary subdivisions of the hemisphere, are bridged across by one or more secondary gyri, so that the boundaries of the fundamental convolutions in the different lobes are rendered indistinct. The more highly convoluted a brain is, the more complex a disposition do these secondary gyri and sulci assume.

Of late years much attention has been paid, not only to the development of the brain in the human embryo, but to the com-

¹ Œuvres de Vicq d'Azyr, vol. vi. p. 27, Pl. 1, 1805.

parative study of that organ, more particularly in the higher mammalia, with especial reference to the arrangement of the convolutions in man, and through the application of these well-known methods of anatomical research, much additional light has been thrown on the subject. The names of Tiedemann and Reichert are intimately associated with the developmental aspect of the inquiry, whilst with that of its comparative study the name of the late Professor Gratiolet, of Paris, stands out with great prominence. The last-named author¹ approached the subject by tracing the gradual appearance of the convolutions as he passed upwards from the smooth-brained marmoset monkey, to the more complex cerebrum of the orang or chimpanzee, amongst the quadrumana, and thence to the human cerebrum. In this manner he was enabled to determine which gyri, so far as regards order of appearance, are to be looked on as primary, and which as secondary. The facts obtained by this mode of research may be applied, so far as they are in accordance with those derived from the study of the mode of development of the human cerebrum, to the elucidation of the arrangement of the convolutions in the brain of man himself. M. Gratiolet has also the merit of having introduced a system of nomenclature, so much more precise and simple than any previously employed, that it has been adopted, with some slight modifications, by many subsequent writers, not only in France, but in Germany and Britain. In Germany, the late Professor Rudolf Wagner, of Göttingen, tested the observations of Gratiolet by the examination both of several quadrumanous brains, and of a series of human cerebra possessing a highly complex arrangement of the convolutions. In this country, also, the subject, with especial reference to M. Gratiolet's mode of description, has been carefully inquired into by Professors Huxley, Rolleston, and Marshall, and by Mr Flower. Through the conjoined labours of these anatomists, our knowledge not only of the form, size, and relations of the great subdivisions of the hemisphere, but of the topography of the individual convolutions has been very materially advanced; so much so, indeed, that we can now localize the different gyri, and give to each its appropriate name. In drawing up the following description, I have based my statements on the examination of a number of apparently healthy brains,—partly male, partly female,—obtained either from the dead-house or dissecting-room, and presumably from natives of the British Islands. At the same time, constant reference has been made to the descriptions and drawings of the various distinguished anatomists who have written on the subject.

In entering on the study of the arrangement of the convolutions, it will be found convenient to look, in the first place, at those which are situated on the outer surface, and the anterior (orbital) part of the under surface; and, secondly, at those which lie on the inner

¹ *Memoire sur les Plis cerebraux de l'Homme et des Primates.* Paris, 1854.

face, and the posterior (tentorial) part of the under surface of each cerebral hemisphere. On each of these surfaces certain well-defined convolutions may be distinguished, which, though they may present some variations in arrangement in different individuals, have, in all well-formed brains, such precise connexions and relations as to enable the anatomist to state that they are homologous structures. The separation of the convolutions from each other is indicated on the surface by numerous fissures, some of which are short and shallow, and not unfrequently vary so much in extent and arrangement in different brains that they must be regarded as quite of secondary importance. Others, again, are deeper, more uniform both in position and extent, and possess, therefore, for topographical purposes, a primary value. Of these more constant fissures, some not only indicate the superficial separation of the different convolutions from each other, but also the subdivision of each hemisphere into smaller lobes. In most of our anatomical text-books¹ only one of these primary fissures—viz., the fissure of Sylvius—has been described in a sufficiently precise and accurate manner, and it has been employed to separate the hemisphere into an anterior and a medio-posterior lobe, the subdivision of the latter into its median and posterior parts being indicated not by a fissure, but by an artificial line corresponding to the outer edge of the cerebellum. The recognition of other constant fissures on the surface of the hemisphere has enabled the descriptive writer to subdivide it not merely into three lobes, but into five, each of which possesses certain relations to the great subdivisions of the cranium. These lobes are distinguished by the names frontal, parietal, occipital, temporo-sphenoidal, and central.

1st, The External and Antero-Inferior (Orbital) Surfaces of the Hemisphere. Figs. 1, 2, 3.

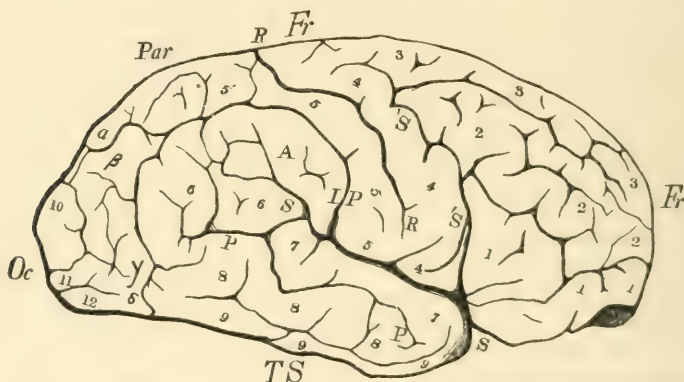
On these surfaces the following fissures, to which special names have been applied, and which, therefore, merit a separate description, may be distinguished without any difficulty:—The fissure of Sylvius (S'S), the fissure of Rolando (R), the external parieto-occipital fissure (PO, Fig. 2), and the intra-parietal (IP), and parallel (P) fissures. The fissures of Sylvius and Rolando, and the parallel fissure, are usually more distinct than the remaining two, for the latter are not unfrequently bridged across by one or more secondary gyri, which form superficial connecting bonds between the convolutions forming their proper boundaries. The fissures of Sylvius and Rolando, and the external parieto-occipital fissure are important because they mark the lines of separation of the hemisphere into its five lobes; the parallel and intra-parietal fissures,

¹ When the lecture was delivered the second part of the new edition of Quain's Anatomy, in which a short account of the mode of arrangement and method of nomenclature employed by Gratiolet is incorporated, had not been published.

again, are situated within certain of these lobes, and express the lines of separation of individual convolutions.

The fissure of Sylvius (S'S).—This well-known fissure has long been recognised by descriptive writers. It is not only the first to appear on the surface of the hemisphere in the course of development, being visible about the fourth month,¹ but it may be seen in the smooth and lowly organized brains of the lower mammalia. When studied in the human brain it is found to be not unfrequently subdivided into two parts, a horizontal and an ascending limb.

FIG. 1.



Note of Explanation to FIG. 1.—This and the succeeding outline figures are drawn from nature, from the brain of a Scotchman, aged 23 :—Fig. 1. A profile view.—Fr., Fr., frontal lobe. Par., parietal lobe. Oc., occipital lobe. T. S., temporo-sphenoidal lobe. S. S., horizontal; and S. S., ascending limb of the Sylvian fissure. R., R., fissure of Rolando. 1. P., intra-parietal; and P. P., parallel fissures. 1, 1, 1, inferior; 2, 2, 2, middle; and 3, 3, 3, superior frontal gyri. 4, 4, ascending frontal; and 5, 5, 5, ascending parietal gyri. 5', outer part of postero-parietal lobule. 6, 6, angular gyrus. 7, 7, superior; 8, 8, 8, middle; and 9, 9, 9, inferior temporo-sphenoidal gyri. 10, superior; 11, middle; and 12, inferior occipital gyri. A., supra-marginal lobule. α, β, γ, δ, first, second, third, and fourth annectent gyri.

The horizontal part (S) is that which is constant and best known. It forms a deep fossa on the outer face of the hemisphere, and passes from before backwards, and with a slight obliquity upwards. Its length varies from 2·5 to 3·5 inches. It is often somewhat undulating, and at its posterior end not unfrequently bends more or less sharply upwards. In some rare cases it may extend so far upwards as to become continuous with the intra-parietal fissure. It may terminate either simply without dividing, or it may bifurcate or trifurcate, in which cases one branch may ascend, and another may pass obliquely downwards and backwards.

The ascending limb (S) is not unfrequently continuous with the anterior end of the horizontal part (fig. 1). It ascends in the substance of the frontal lobe, anterior to the convolution afterwards to be described as the ascending frontal 4, 4, for a greater or less distance.

¹ Reichert, pl. xi., fig. 32.

Its extent varies considerably in different brains, and is determined by the mode of origin of the three tiers of frontal convolutions. When these arise superficially from the ascending frontal gyrus, the extent of this fissure is short; and if the origin of the inferior frontal gyrus should be quite superficial from the lower end of the ascending frontal, then the ascending limb of the Sylvian fissure is cut off from the horizontal. This subdivision of the Sylvian fissure corresponds to that described by Mr Huxley, in the quadrumanous brain, as the antero-parietal sulcus.¹

The fissure of Rolando (R).—This fissure, although known to Vicq d'Azyr, was first accurately described by the Italian anatomist Rolando,² after whom it was named by Leuret,³ the distinguished French physiologist. By Huschke,⁴ Reichert,⁵ and Rudolf Wagner,⁶ it has been termed the sulcus hemisphæræ centralis (Central-furche). It occurs constantly in the human brain, and is met with in the brains of the great majority of the quadrumana. It appears in the brain of the human foetus about the sixth month of intra-uterine life.⁷ It commences a little above the horizontal limb of the Sylvian fissure, and ascends obliquely backwards and upwards in a slightly undulating manner until it reaches the edge of the great longitudinal fissure of the cerebrum. It is bounded both in front and behind by a well-marked convolution, each of which ascends from the Sylvian fissure as far as the great longitudinal fissure. These convolutions will be distinguished in the following description as the ascending frontal (4, 4) and parietal (5, 5) gyri. Short secondary fissures, continuous with the fissure of Rolando, not unfrequently pass into the ascending gyri. At their lower ends the two ascending convolutions usually become continuous with each other, and cut off the fissure of Rolando from that of Sylvius; but I have now more than once seen the fissure of Rolando prolonged so far downwards as to be continuous with that of Sylvius. I have also seen, on both sides of the same brain, the fissures of Rolando separated from the longitudinal fissure by a broad superficial convolution, which connected together the upper ends of the ascending frontal and parietal gyri. In no case have I seen a connecting convolution bridge across this fissure in any part intermediate between its two extremities, and I know of but one brain, that of the clinical physician, C. H. Fuchs, figured by R. Wagner, in Pl. 1 of his 2d Abhandlung, in which this has been represented.

¹ On the Brain of *Ateles paniscus*.—Proc. Zool. Soc. of London, vol. xxix. p. 257, 1861.

² Della Struttura degli Emisferi Cerebrali.—Mem. della Reale Accademia delle Scienze di Torino, vol. xxxv. p. 103, 1831.

³ *Système Nerveux*, vol. i. p. 398, 1839.

⁴ Schaedel, *Hirn und Seele*, p. 137, 1854.

⁵ *Der Bau des menschlichen Gehirns*, 1859 and 1861.

⁶ *Vorstudien zu einer wissenschaftliche Morphologie des menschlichen Gehirns*, 1860, p. 15.

⁷ Reichert, tab. xii. figs. 47, 48.

The fissures of Rolando on the two sides reach the longitudinal fissure sometimes opposite each other, at others the right a little in front of the left, or *vice versa*, and they form the sides and apex of a V-shaped figure, the apex of which is always nearer the posterior than the anterior end of the cerebrum. The more highly developed the frontal lobe of the brain is, the more oblique is the fissure of Rolando, and the further is the apex of the V thrown backwards. In this description the fissure of Rolando will be regarded as separating the frontal from the parietal lobe.¹

FIG. 2.

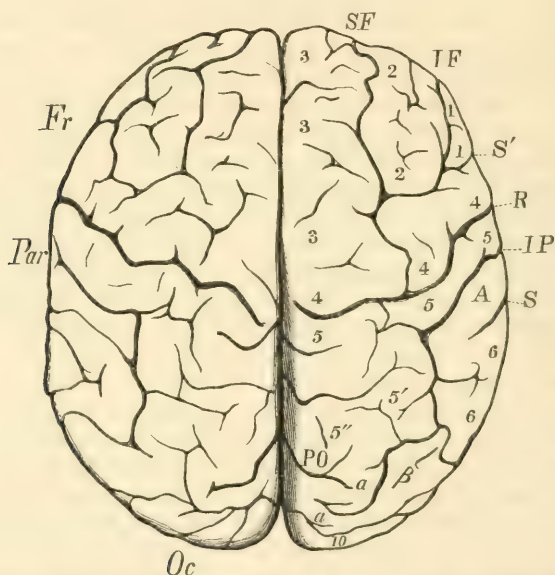


FIG. 2.—Vertex view of the Brain represented in Fig. 1. The same letters and numerals indicate the same parts. In addition, note PO, the parieto-occipital fissure, the external part of which, unshaded, is continuous with the internal part represented in shadow. SF, supero-frontal, and IF, infero-frontal fissure.

The external parieto-occipital fissure (PO, Fig. 2).—Various names have been applied at different times to this fissure. Burdach² had evidently recognised it as continuous with the upper end of his fissura posterior, and the name posterior fissure has been applied to it by Reichert and Wagner. By Gratiolet and other writers on the cerebral anatomy of the quadrumana—a group in

¹ The fissure of Rolando is supposed by many anatomists to lie directly under the coronal suture, but M. Broca (sur le Siège de la Faculté du Langage articulé, Paris, 1861), from positive observations, has shown that it is posterior in position. From the examination of eleven adult male brains, he found that, at the middle line, the fissure was some distance behind the suture, whilst at the outer end it approached much more closely to it.

² Vom Baue und Leben des Gehirns, vol. ii. p. 166, 1822.

which it is much more strongly marked than in man—it has been named the external perpendicular, or vertical, or posterior transverse fissure. The name parieto-occipital is, however, to be preferred to any of the above, because it expresses that it serves as the line of separation between the parietal and occipital lobes. This fissure appears in the development of the human brain about the sixth month.¹

It is situated at no great distance in front of the posterior end of the cerebrum. It commences at the great longitudinal fissure, where it is continuous with the internal parieto-occipital fissure, and, through its continuity with the latter, its position, even in those cases in which it is feebly marked, may be always recognised. In some brains it appears simply as an indentation in the margin of the hemisphere, but in others it extends for some distance more or less transversely outwards. I saw it in the right hemisphere of a female brain pass rather more than two inches outwards; and in another specimen, also the right hemisphere, it proceeded for four-tenths of an inch outwards, and then extended downwards as far as the lower margin of the outer surface of the hemisphere. The imperfect definition of this fissure in the majority of human brains, as compared with its remarkable distinctness in the brains of most quadrumana, is owing to the presence in the former of certain superficial well-marked secondary convolutions which bridge it over and connect the parietal with the occipital lobe. The closer the first of these bridging gyri lies to the longitudinal fissure the shorter is the external parieto-occipital fissure. Sometimes two or three secondary sulci proceed from it, which may pass either forwards or backwards into the parietal or occipital lobe. The corresponding fissures of the two hemispheres may be exactly opposite, but more frequently one lies a little in front of the other.

The intra-parietal fissure (IP).—This fissure is figured in all the more accurate drawings of the human and quadrumanous brain, though no special description has been given of it. It lies within the parietal lobe, from which circumstance I have termed it intra-parietal.² It may be recognised at the sixth month of intra-uterine life. It is situated immediately behind and ascends parallel to the ascending parietal gyrus, and then bends almost horizontally backwards, and extends for a varying distance in different brains; in some cases it may be traced between the first and second bridging convolutions, as in the right hemisphere of figs. 1 and 2. Its ascending part separates the supra-marginal gyrus (A) from the ascending parietal (5); its horizontal part separates the supra-marginal gyrus from the ascending parietal lobule (5'). Not unfrequently one or more secondary gyri bridge it across superficially; a frequent seat

¹ Reichert, pl. xii. figs. 46, 48.

² See my "Notes more especially on the Bridging Convolutions in the Brain of the Chimpanzee."—Proc. Roy. Soc., Edinburgh, Session 1865–66, p. 578.

for such a connecting convolution is at the angle of junction of its ascending and horizontal portions, excellent representations of which may be seen in the brains of the Bushwomen figured by Professors Gratiolet and Marshall.¹ The intra-parietal fissure is usually separated from the horizontal part of the fissure of Sylvius by that portion of the supra-marginal gyrus which joins the lower end of the ascending parietal convolution. In Gratiolet's Bushwoman it may be seen, however, that they are continuous with each other; and I have observed the same arrangement in more than one European brain (See fig. 1).

When the hemisphere is viewed in profile, three fissures may be traced passing for a greater or less distance upwards on its outer face. These are from before backwards, the ascending limb of the Sylvian fissure, the fissure of Rolando, and the intra-parietal fissure. They may, more especially the first-named, be continuous with the horizontal limb of the Sylvian fissure, and then seem as it were to radiate from it, but not unfrequently they commence at some little distance above it, and it is owing to their position and direction that the ascending frontal and parietal convolutions are so accurately defined.

*The parallel fissure (P), (Antero-temporal, Huxley).—*This fissure is situated in the temporo-sphenoidal lobe. It lies below and parallel to the Sylvian fissure, hence Gratiolet has termed it the parallel fissure. It may be seen in the brain of a sixth-month human foetus,² and it is well marked in the brains of the quadrumana. It runs more or less tortuously backwards and upwards. Posteriorly, it extends into the angular gyrus, and not unfrequently gives off secondary sulci; by one of these it sometimes becomes continuous with the Sylvian fissure, and occasionally it, or one of its branches, passes quite through the angular gyrus, and separates superficially the second bridging convolution from that gyrus. When the Sylvian and parallel fissures are connected by an intermediate sulcus, as in fig. 1, the first temporo-sphenoidal and angular gyri are no longer continuous superficially with each other.

Lobes of the Cerebrum.—Although the usual plan adopted by anatomists was to divide the hemisphere into two, or at the most three, lobes, yet attempts were occasionally made by descriptive writers to introduce a more complete subdivision. In a Treatise on the Structure of the Brain, published in 1822, Burdach describes the hemisphere as consisting of five lobes, an anterior, superior, posterior, inferior, and central. Arnold also, in his systematic work, speaks of four lobes, anterior, superior, posterior, inferior. But this method of description, on account probably of the boundaries of the different lobes not being indicated with sufficient exactness, did not meet with much acceptance. In 1854, Gratiolet re-introduced the system of more complete subdivision, gave to each lobe precise

¹ Philosophical Transactions. 1863.

² Leuret and Gratiolet, pl. 29, fig. 13.

limits, and distinguished them by names indicating their position in the cavity of the cranium. The nomenclature and mode of subdivision employed by Gratiolet have been adopted by most subsequent writers, and, with a slight modification in the posterior boundary of the frontal lobe, will be used in this description.

The Frontal lobe (Fr), as its name indicates, forms the anterior end of the cerebrum. It is subdivided into two lobules, a frontal and an orbital. *The frontal lobule* extends from the anterior end of the cerebrum to the fissure of Rolando. It includes the convolution which ascends in front of that fissure as far as the margin of the great longitudinal fissure, and which may be called, therefore, the *ascending frontal gyrus* (4, 4). By Gratiolet (Mémoire) this lobule is not considered to extend so far back as the fissure of Rolando, and the convolution here termed ascending frontal gyrus is included by him in the parietal lobe and named first ascending parietal convolution; under this arrangement the posterior boundary of the frontal lobe is the ascending limb of the Sylvian fissure (antero-parietal, Huxley). But it seems to me that the gyrus ascending in front of the fissure of Rolando is more appropriately placed in the frontal lobe, as it is cut off superficially from the other parietal convolutions by that fissure, and it is not unfrequent to find it cut across by secondary sulci into two or three stages, which are quite continuous with those gyri which run from behind forwards and form the three stages of the frontal lobe.

In front of the ascending frontal gyrus the convolutions run from behind forwards, and extend as far as the anterior end of the cerebrum, where they become continuous with the convolutions of the orbital lobule. They are more or less perfectly subdivided into three stages or tiers, a *superior*, *middle*, and *inferior*, by two fissures running in the antero-posterior direction, which are named the *supero-frontal* (SF) and *infero-frontal* (IF) sulci.

The superior frontal gyrus (3, 3, 3) forms the margin of the great longitudinal fissure in its anterior part; the inferior, or infero-external gyrus (pli surcilier) (1, 1, 1), bounds the frontal lobule inferiorly and externally; the middle (2, 2, 2) lies intermediate. These convolutions all spring from and are continuous with the ascending frontal gyrus. So far as my observations go, the origin of the superior frontal gyrus is always superficial, but that of the middle or inferior may be either superficial or concealed; the latter more especially in those cases in which the ascending limb of the Sylvian fissure passes far upwards in front of the ascending frontal gyrus. In the more richly convoluted brains these frontal gyri are still further subdivided by short secondary fissures, and the supero and infero-frontal sulci are bridged across by small secondary gyri, so that it is sometimes difficult to say to which tier a particular portion of convolution should be referred. Wagner has especially recognised this arrangement in the complex human brains he has described; and Gratiolet states that the superior

gyrus is frequently subdivided into two secondary convolutions. When the frontal lobe is largely developed, the superior frontal convolution mounts upwards, and extends backwards for a considerable distance along the margin of the great longitudinal fissure.

FIG. 3.



FIG. 3 displays the Orbital Lobule and the Island of Reil.—17, convolution of margin of longitudinal fissure; O, olfactory sulcus; TR, triadriate sulcus; 1'', posterior; 1''', internal; 1''', external gyri of the orbital lobule; C, Island of Reil with its radiating convolutions; 1, 1, under surface of the inferior frontal gyrus; 4 and 5, under surfaces of the lower ends of the ascending frontal and parietal gyri. The temporo-sphenoidal lobe has been to a great extent removed in order to display the island.

The orbital lobule (Fig. 3) is slightly concave and rests on the roof of the orbit. In it may be recognised a well-marked fissure (O), the olfactory sulcus, which is for the most part concealed by the olfactory bulb and peduncle. The convolution (17), situated internal to this sulcus, forms the anterior part of the great marginal gyrus. External to the olfactory sulcus are several small irregularly arranged fissures which sometimes assume a triadriate form (TR) and subdivide this part of the lobule into three gyri,—an internal, 1'', a posterior, 1'', and an external, 1'''. The internal lies on the outer side of the olfactory sulcus, the posterior is situated in front of the most anterior convolution of the island of Reil (C), and the external is blended with the anterior terminations of the superior, middle, and inferior frontal gyri.

The Parietal lobe (Par) extends from the fissure of Rolando backwards to the parieto-occipital fissure. Four distinct convolutions may be recognised in it, which have been termed ascending parietal gyrus, postero-parietal lobule, lobule of the supra-marginal gyrus, and the angular convolution.

The ascending parietal convolution (5, 5, 5) passes obliquely backwards and upwards from the Sylvian to the great longitudinal fissure. It is named by Gratiolet,¹ deuxième pli ascendant. It bounds the fissure of Rolando posteriorly. It and the ascending frontal form two of the best defined convolutions of the cerebrum, and have frequently been described by anatomists. They constitute the second and third vertical convolutions of Rolando, and are named the first and second superior convolutions by Leuret (Pl. 16), the antero- and medio-transverse parietal by Foville (Pl. 10), the anterior and posterior central gyri by Huschke and Wagner, and the antero- and postero-parietal by Huxley. Gratiolet himself, modifying somewhat the names he employs in his special monograph, terms them, in his explanation to Leuret's seventeenth plate, the ascending frontal and parietal convolutions. The ascending parietal gyrus frequently exhibits several knee-shaped bends and indentations, and is partially, but, so far as my experience goes, never wholly cut across by secondary fissures.

The postero-parietal lobule (Huxley), (Lobule du deuxième pli ascendant, Gratiolet) (5', 5'') springs from the posterior part of the upper end of the ascending parietal gyrus and extends backwards as far as the external parieto-occipital fissure. It corresponds in part to the lobule described by Huschke and some other German anatomists as the præcuneus (Vorzwickel). It is bounded externally by the intra-parietal and internally by the great longitudinal fissure. When the intra-parietal fissure is bridged across by secondary gyri, this lobule is connected to the supra-marginal lobule, or to the angular gyrus or to both. It is frequently subdivided into two parts, an inner (5'') and an outer (5'), by a sulcus running more or less in an antero-posterior direction; and in the more richly convoluted human brains, these are again subdivided by several additional small sulci, so as to exhibit a very complex appearance. This lobule is connected to the upper part of the occipital lobe by the first annectent convolution (*a, a*), the size and position of which modify considerably the extent of the parieto-occipital fissure. When large and arising close to the edge of the longitudinal fissure, it may join almost immediately the occipital lobe, and then the external parieto-occipital fissure is little more than a mere depression. If the annectent gyrus should sweep outwards before joining the occipital lobe, then the fissure is much longer and may even bifurcate or still further subdivide, so as to split up this convolution into secondary gyri. I have seen the first annectent convolution disconnected superficially at its origin from the postero-

¹ Mémoire, pp. 26, 60.

parietal lobule by the parieto-occipital fissure, though in its further passage it came to the surface and assumed a large size before it joined the occipital lobe. Gratiolet attached great importance to the size and position of this convolution in the human brain; by him it was first called *pli supérieur de passage*, a name which has been variously translated by English anatomists as first-bridging, annectent, or connecting gyrus.¹

The lobule of the supra-marginal convolution (Gratiolet) (A, fig. 1) lies between the lower end of the ascending parietal convolution and the summit of the horizontal limb of the Sylvian fissure. It corresponds to that part of the posterior transverse convolution of Foville which forms the anterior boundary of that fissure at its upper end. It is very variable both in size and form. Sometimes it is irregularly quadrilateral, at others more elongated. Not unfrequently it is subdivided by secondary sulci into smaller gyri. Usually it is continuous superficially with the lower end of the ascending parietal convolution, though, when the intra-parietal fissure joins the Sylvian, it is cut off from it (fig. 1). It is often prolonged directly into either the ascending parietal gyrus or the postero-parietal lobule by a secondary gyrus passing across the intra-parietal fissure. It is usually continued posteriorly into the angular gyrus, but in those rare cases in which the Sylvian fissure is prolonged into the intra-parietal it is cut off from it superficially.

The angular gyrus (*pli courbe*, Gratiolet) (6, 6) lies at the summit of the fissure of Sylvius. Its exact limits are not at all times easily defined; but it may be regarded as bounded in front by the supra-marginal lobule, above by the intra-parietal fissure, behind by the occipital lobe, whilst inferiorly it is prolonged into the temporo-sphenoidal lobe with the first and second convolutions of which it is usually continuous, though sometimes secondary fissures disconnect it superficially from one or other or both of these gyri. It is frequently continuous with the postero-parietal lobule by a secondary gyrus bridging over the intra-parietal fissure. Its relation to the occipital lobe is regarded by Gratiolet as one of great importance. He has described a convolution, the second annectent gyrus (*deuxième pli de passage* β), as passing from the angular gyrus to the occipital lobe, and as assuming a size and superficial position very characteristic of the human cerebrum. In twelve brains specially examined with the object of determining the arrangement of this second annectent gyrus, I found it superficial in all, though varying somewhat in size and complexity, and (except in the specimen already alluded to, in which the parieto-occipital fissure extended as far as the lower margin of the outer face of the hemisphere) in all it was superficially continuous with

¹ Some excellent observations and remarks on the variability displayed by the premier pli de passage in the human cerebrum are recorded by Professor Rolleston in the *Natural History Review*, vol. i. p. 211. See also his Lecture in the *Medical Times and Gazette*, 22d February and 15th March 1862.

the angular gyrus, so that the parieto-occipital fissure was in that region completely obliterated. The angular gyrus, the second annectent gyrus, and the part of the occipital lobe into which the latter is prolonged, are almost equivalent to the transverse occipital convolution of Foville.

The Temporo-Sphenoidal lobe (TS) lies behind and below the horizontal limb of the Sylvian fissure. The convolutions which are seen on its outer aspect are simple in arrangement, they lie one below the other, parallel to the fissure of Sylvius, and are distinguished from above downwards, as, first, second, and third, or superior, middle, and inferior temporo-sphenoidal gyri.

The superior temporo-sphenoidal convolution (7,7) forms the posterior lip of the horizontal limb of the Sylvian fissure, and corresponds to the posterior part of the convolution of that fissure as described by Foville. From being parallel to that fissure, Gratiolet has termed it the parallel convolution. It is usually simple in form, presents but few tortuosities, and though secondary fissures do pass into it they usually possess no great depth. It is separated from the middle temporo-sphenoidal gyrus by the parallel fissure. By its upper end it is continuous with the angular gyrus.

The middle (8,8,8) *and inferior* (9,9,9) *temporo-sphenoidal convolutions* lie parallel to each other and to the superior. They are more or less perfectly separated from each other by a sulcus, the postero-temporal, which is not unfrequently bridged across by secondary gyri. Secondary sulci often pass for some distance into the substance of these convolutions, more especially into the second. The upper ends of these gyri are prolonged into the lower part of the occipital lobe by the third (γ) and fourth (δ) annectent gyri (troisième et quatrième plis de passage, Gratiolet). These connecting convolutions are superficial throughout, are more or less tortuous, and subdivided, not unfrequently, into smaller gyri by secondary fissures.

The Occipital lobe (Oc) forms the posterior end of the cerebrum. Its anterior boundary is the parieto-occipital fissure, though its exact limit in this direction, when the first and second bridging convolutions are large and tortuous, is not at all times easy to define precisely in the human cerebrum. Relatively to the size of the brain this lobe is in man especially small. It is described by Gratiolet as subdivided into three stages of convolutions, a *superior* (10), *middle* (11), and *inferior* (12); but from the fissures which indicate this separation frequently not being parallel, from the presence of several secondary sulci, and from the mode in which this lobe is connected to the parietal and temporo-sphenoidal by the annectent gyri, the exact line of demarcation of the three stages is often defined with difficulty.

The Median or Central lobe (C, fig. 3) is more generally known as the insula or island of Reil, after its first describer.¹ It may be

¹ Reil's Archiv, Halle, 1809, p. 144.

recognised at the fifth month of embryo life, and from its constancy and early appearance is to be regarded as one of the primary subdivisions of the hemisphere. In the foetus, and even up to the time of birth, it may be seen without difficulty when the outer surface of the cerebrum is examined; but as the growth and development of the lips of the Sylvian fissure proceed, the island becomes so included within it that the margins of the fissure have to be widely everted, or even partially removed, before its form and relations can be properly studied. In the adult it occasionally happens that the more anterior part of the island is visible without any dissection. I have seen such a condition on both sides of the brain of an aged Scotchwoman; M. Gratiolet has observed it on the left side of a European brain; and Mr Marshall states that, in the brain of the Bushwoman he examined, each Sylvian fissure was so patent as to allow the insula, which was less subdivided than is customary, to be seen. This lobe consists of from five to seven short and almost straight gyri (gyri operi) which radiate outwards and backwards from about the region of the locus perforatus anticus. The sulci between these gyri are short, possess no great depth, and sometimes are so faintly marked that the separation of some of the convolutions from those immediately adjacent is not very apparent. Anteriorly the island is closely related to the orbital lobule, with the postero-internal part of which its most anterior convolution is continuous. Externally it is separated by a deep sulcus from the inferior frontal gyrus, and from the lower ends of the ascending frontal and parietal gyri where they form the anterior lip of the Sylvian fissure. By its deep aspect the island of Reil is closely related to the thin grey lamella, called by Arnold nucleus tæniæformis, which forms the outermost part of the extra-ventricular portion of the grey substance of the corpus striatum.¹

2d, The Internal and Postero-Inferior (Tentorial) Surfaces of the Hemisphere (Fig 4.)

It is convenient to study these surfaces together, as the gyri and sulci of the one are, to some extent, prolonged into the other.

The calloso-marginal fissure (i, i, i) has been represented in all the best drawings of the brain, from the time of Vicq d'Azyr, but, until very recently, few writers have applied to it any descriptive name. Gratiolet has termed it the fronto-parietal fissure, but the name calloso-marginal, employed by Mr Huxley, seems to be more appropriate. Reichert figures it as present at the twentieth week of embryo life. It commences below the anterior end of the corpus callosum, turns round the genu of that body, runs backwards about midway between the corpus callosum and the margin of the longitudinal fissure, and then turns upwards to reach the latter. A fissure is prolonged backwards from it through the quadrilateral

¹ Very illustrative views of the deep relation of the insula are given by Reichert in figs. 35 and 36 of his 6th plate, Part 2.

lobule (18') towards, but in the human brain only occasionally reaches, the parieto-occipital fissure. Short secondary fissures proceed from it into the convolutions between which it lies, and it is not unfrequently bridged across by one, two, or three secondary gyri.

The convolution of the corpus callosum, gyrus fornicatus (18, 18, 18) has been generally recognised since it was described by Sömmering and Vicq d'Azyr. It commences in front of the anterior perforated space, ascends in front of the genu of the corpus callosum, runs backwards in close relation to the upper surface of that body, turns down behind its posterior free end, and becomes continuous with the uncinata convolution (19).

The marginal convolution (17, 17, 17) corresponds to the anterior part of the convolution of the margin of the great longitudinal fissure of Foville, and extends backwards as far as the place where the calloso-marginal sulcus reaches the upper margin of the hemisphere. It is more or less subdivided in different brains by secondary sulci, and is usually broader opposite the anterior end of the cerebrum than in the rest of its extent.

FIG. 4.

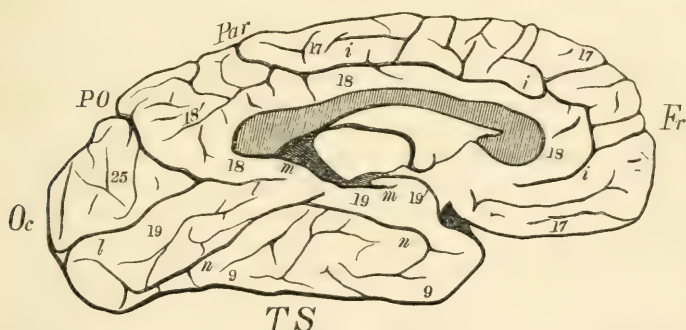


FIG. 4. Inner Face and Tentorial Surface of the Hemisphere.—*i, i, i*, calloso-marginal fissure; *l, l*, calcarine fissure; *m, m*, dentate fissure; *n, n*, collateral fissure; *PO*, internal parieto-occipital fissure; 17, 17, 17, marginal gyrus; 18, 18, convolution of corpus callosum; 18', quadrilateral lobule; 19, 19, uncinata gyrus, of which 19' is the *crochet* or recurved part; 25, the cuneus or occipital lobule; 9, 9, the inner face of the inferior temporo-sphenoidal gyrus.

The internal parieto-occipital fissure (PO) commences at the margin of the great longitudinal fissure, where it is continuous with the external parieto-occipital sulcus, and extends in a curved manner (with the concavity forwards) downwards and forwards until it joins the calcarine fissure. In the human cerebrum no annectent gyrus bridges it across superficially, so that it presents a striking contrast to the external fissure of the same name. This fissure was apparently recognised by Vicq d'Azyr, and regarded by him as separating the middle from the posterior lobe,¹ and it has been carefully figured by Burdach, Foville, Cruveilhier, etc.

¹ Plate xxii. p. 141.

Gratiolet has named it the internal perpendicular fissure, but the term parieto-occipital applied to it by Mr Huxley, expresses more precisely its relations. Between this fissure and the posterior part of the calloso-marginal sulcus, which turns up to the free edge of the marginal gyrus, is an irregularly four-sided convolution, named by Foville the quadrilateral lobule (18'). In different brains this is variously subdivided by smaller fissures into secondary gyri.

The calcarine fissure (Huxley), (posterior part of the fissure of the hippocampus of Gratiolet), (*l, l*), usually commences at the posterior end of the cerebrum in a bifurcated manner, though I have seen it arise quite simply, and at some distance in front of the hinder end of the hemisphere. It runs forwards, is joined by the parieto-occipital fissure, and extends as far as the postero-inferior part of the gyrus fornicatus, which separates it from the dentate fissure. The calcarine fissure possesses great depth, and it indicates, on the surface of the hemisphere, the position, in the interior of the posterior cornu of the ventricle, of the elevation called hippocampus minor or calcar avis. Several ridges of cerebral substance project into it, one of which, at the posterior part of the fissure, corresponds to the calcarine gyrus of Mr Flower.¹ Reichert figures² both the calcarine and internal parieto-occipital fissures, quite continuous with each other, in the brain of a foetus about twenty weeks old, and from their early appearance he puts them in the same class as the Sylvian fissure,—as possessing, therefore, a greater developmental importance than an ordinary sulcus. In the angle between the calcarine and parieto-occipital fissure is a triangular lobe called the *occipital lobule* (25), which is more or less subdivided into secondary gyri. This lobule was first described by Burdach as the cuneus (Zwickel), a name which it still bears in the German descriptive writings.

The dentate fissure (Huxley) (anterior part of the fissure of the hippocampus, Gratiolet) (*m, m*) lies immediately posterior to the lower end of the great transverse fissure of the cerebrum and marks externally the position of the hippocampus major internally. Its floor is formed by the grey matter of the fascia dentata, or *corps godronné*, which lies immediately to the outer side of the tænia hippocampi. By its lower end it is prolonged into the recurved part of the uncinate gyrus (19').

The collateral fissure (Huxley) (*n, n*) is situated below the calcarine fissure, and extends somewhat irregularly along almost the entire length of the tentorial surface of the cerebrum. It is not unfrequently subdivided by short gyri passing across it, and secondary fissures branch off from it. It lies below the floors of the posterior and descending cornua of the lateral ventricle, and in a part of its extent the eminentia collateralis is in relation to it.

The convolutions on the tentorial face of the hemisphere are three in number. *The superior internal temporal* (pli godronné) forms

¹ On the Posterior Lobes of the Cerebrum of the Quadrumana.—Phil. Trans., 1862, p. 192.

² Plate xi. fig. 36.

the floor of the dentate fissure, and is not visible in Fig. 4. *The middle internal temporal*, or uncinat gyrus (19, 19), is bounded, on the one hand, by the calcarine and dentate fissures; on the other, by the collateral fissure. At its lower end it enlarges and forms a recurved portion (19'), called by Vicq d'Azyr the *crochet*, and by Gratiolet the lobule of the hippocampus.

The inferior internal temporal gyrus (9, 9) is inferior to the collateral fissure, and is simply a portion of the inferior external temporo-sphenoidal gyrus, which is turned round the lower margin of the hemisphere and forms a part of the internal aspect.

From the time of Vicq d'Azyr and Gall, when the arrangement of the gyri was first systematically examined, down to the present day, anatomists have observed that the convolutions of the brain are not absolutely symmetrical in the two hemispheres of the same cerebrum. The more precise investigations of later years enable one to say that the want of symmetry especially occurs in the secondary gyri, and that it is most strongly marked in the brains which possess the greatest complexity in the arrangement of their convolutions. The observations of Professor Wagner, who enjoyed several opportunities of examining the brains of men endowed with great powers of intellect, seem to point to the conclusion that the more richly convoluted brains co-exist with great intelligence. Again, it would appear that the average female brain has not such complex convolutions as the average male brain; and from the careful descriptions furnished by Professors Gratiolet and Marshall of the brains of two Bushwomen, it would seem that the number of secondary gyri is smaller in the brain of the savage than in that of the European.

In estimating, however, the comparative value of the convolutions in different individuals, not only should we look at the number of gyri seen on a surface examination, but the depth of the sulci and the quality of the tissue composing the convolutions ought to be considered. A brain with deep sulci, but with comparatively simple gyri, might present as great an extent of grey matter as one the convolutions of which are much more tortuous though less deep. That grey matter is presumably the most active which contains the greatest amount of nerve cells in a given area; but what the comparative quantity of nerve cells may be in a given extent of convolutions from corresponding parts of the brain in individuals of different sexes or of different races, or even in convolutions taken from different parts of the brain of the same individual, we have at present no definite information.

The precise morphological investigations of the last few years into the cerebral convolutions have led to the revival in Paris of discussions, in which the doctrine of Gall and his disciples—that the brain is not one but consists of many organs—has been supported by new arguments, and the opinion has been expressed that the

primary convolutions, at least, are both morphologically and physiologically distinct organs. Hence both the physiologist and pathologist have been induced to take an interest in a subject which, at the first glance, might seem as if it lay almost exclusively within the province of the morphological or descriptive anatomist. The observations made by M. Paul Broca,¹ that loss of the cerebral faculty of speech co-existed with a lesion of the posterior third of the inferior left frontal gyrus, seemed to point to that convolution as the seat of the faculty of spoken language. Dr Sanders has also communicated to the Medico-Chirurgical Society of our city a case in which a similar defect in the power of speech occurred along with a lesion in the same locality.² Such observations have put the subject into that stage that, to use the words of Dr Sanders, "it requires and invites fresh investigation."

In pursuing this inquiry, it should be kept in mind that a convolution, though separated superficially from its neighbours by fissures, yet becomes continuous with them at the bottom of these fissures, so that the grey matter of the one blends with that of another. Again, adjacent gyri, though in many respects they may present precise morphological positions and relations, are not unfrequently connected together by secondary gyri, either altogether superficial or partially concealed within the dividing fissure, so that we cannot with them, as with a group of muscles, make a subdivision into distinct organs without effecting an artificial separation of their constituent parts. A true physiology, and consequently a definite pathology of the cerebral convolutions, can only be arrived at when not only the intimate structure of the grey matter is ascertained, but after the connexions of the numerous fibres passing to and from it at both their extremities have been determined. And as it is desirable before studying the geology of a district to have the surface carefully surveyed and accurately mapped out, so it is advisable that the topography of the convolutions of the human cerebrum should be satisfactorily ascertained before an analysis of the intimate structure and deep connexions of the grey matter can be put forth with the necessary exactness.

¹ Bulletin de la Société Anatomique, 1861-63.

² Edinburgh Medical Journal, March 1866.

ARTICLE IV.—*Purpura Hæmorrhagica in Children.* Two Cases, with Remarks. By G. STEVENSON SMITH, L.R.C.S.E., formerly Resident Medical Officer, Royal Edinburgh Hospital for Sick Children.

PURPURA is an affection of frequent occurrence in youth, and is most commonly met with in children whose general health has become deteriorated either by protracted illness or by an insufficient supply of pure air and proper food. It does occasionally show itself, however, in an acute form, coming on suddenly in a child who previously enjoyed good health; and sometimes it manifests itself during the course of an acute disease, as in fever. During the past year we have observed it in its simplest form in a little girl who was under treatment for phthisis; and in another patient aged 8, who was suffering from typhoid fever, numerous purpura spots made their appearance on the belly a day or two after a sharp attack of hæmorrhage from the bowel.

It is usual in medical works to draw a distinction between purpura and scurvy, and the practitioner is warned against mistaking the one for the other. But, in actual practice, cases not unfrequently occur in which the characteristic symptoms of these two diseases are blended and combined. In the treatment of such cases a combination of tonic and anti-scorbutic remedies is productive of the most beneficial results. The following case will illustrate the truth of these remarks:—

CASE I.—E. A., aged 7, was admitted to the Children's Hospital on 4th October 1865, on the recommendation of Dr Andrew Inglis. She had been falling off in health for some time, and a few days before admission had bled freely from the nose and mouth. On admission, her face was of a dingy green colour, which contrasted strangely with the bright pearly hue of the conjunctivæ. The lips, gums, and tongue were swollen, and covered with a coating of black coagulated blood. The tumid gums bled on the slightest touch. Bloody saliva trickled away almost constantly from the mouth, and the teeth were incrustated with dark-coloured coagula. On the face, trunk, arms, and legs were numerous variously tinted purpura spots, of rounded and oval form, which did not disappear on pressure. Some of them were of a dark claret colour, some of a brighter purple hue, others of a dirty yellowish green appearance.

The pulse was rapid and feeble, and the child lay in a half sleeping prostrated condition. Nothing abnormal could be detected in the chest or abdomen. The urine was as dark as coffee, and deposited a thick dark-brown sediment when allowed to stand a short time. On boiling it, a thick curdy albuminous deposit was produced, which occupied about three-fourths of the bulk of the urine in the test-tube. A microscopic examination showed that the urine was loaded with blood-corpuscles, and a few membranous scales stained with hæmatin were also seen.

A mixture of chlorate of potash and tincture of the muriate of iron, four grains of the former, and eight drops of the latter, was directed to be given every three hours. A gargle of chlorinated soda was to be used for washing the mouth, and strong beef-tea, with a dessertspoonful of sherry to be given frequently; she was also to take lemon-juice.

After two days' treatment there was a most marked improvement in the condition of the patient; the mouth was cleaning and the urine was more natural. By the 8th, the patient was lively, and sitting up in bed, and the urine, though still containing blood-cells, was quite clear. After the 11th, no fresh spots appeared; the bleeding from the gums ceased, and she was out of bed and walking about the ward. The tincture of the muriate was continued in ten-drop doses thrice a-day. On the 18th, no blood-corpuscles could be detected in the urine, the spots were disappearing, and the patient, though pale, was quite well. She improved in strength daily, and was dismissed on 28th October.

In the next case, the disease was more chronic; there were no scorbutic symptoms, no sponginess of the gums or bleeding from them; but there was hæmorrhage both from the stomach and bowels. But notwithstanding a great improvement in her general health and strength, fresh crops of purpura spots continued to show themselves for weeks at intervals of four or five days.

CASE. II.—J. T., a pale unhealthy-looking girl, aged 11, was admitted to the Children's Hospital on 27th March 1865. She belonged to an unhealthy family, and had been in poor health for some months. A brother of hers was admitted about the same time suffering from well-marked rickets and general debility. Of late, this girl had frequently vomited and passed from the bowels dark-coloured blood in considerable quantity. Her feet and legs, too, had been œdematous, and she had for some time had pain across the stomach.

When admitted, the legs were mottled with purple-coloured spots, and there were a few on the arms also. Pulse weak; gums pale, but firm and sound. The urine was dark in colour, albuminous, and contained blood-corpuscles.

The tincture of the muriate of iron was prescribed, eight drops to be taken thrice a-day, and she was to have a nourishing diet. By the 6th of April she was much improved in health and spirits, but fresh crops of spots continued to show themselves every now-and-then on the feet and legs. Dark spots were also observed on the roof of the mouth and all over the surface of the soft palate. No blood was passed from the stomach or bowels, however. She got quite well in her general health, took her food with relish, and complained of nothing; but up to the day of her dismissal, the 17th of May, successive crops of dark-red spots continued to show themselves at intervals, and her urine still contained traces of albumen. She was recommended to continue the iron in increased doses.

ARTICLE V.—*On Dilatation of the Perinæum.* By JAMES MORE, M.D., Rothwell, Northamptonshire.

ON the 7th of July, I was asked to attend Mrs L., about to be confined of her first child, and on examination found the head very high up, and almost out of reach. The pains were good and regular from the first; but finding, after waiting some hours, that the head did not descend, indeed seemed fixed, I again made a careful examination to ascertain the cause of obstruction, and was struck with two things,—

1st, The great thickness and resistency of the perinæum.

2d, The regular straining and relaxation of this structure, which took place during each pain, and its regular return to its normal state the moment the uterine contractions ceased.

Knowing that the doctrine of the schools teaches, that this same dilatation is due to the pressure of the child's head on the perinæal structures, my attention was at once arrested by this apparently peculiar case, and I consequently determined to watch the whole process narrowly.

The report of the case at this stage was as follows:—The external parts moist, but rather narrow, the fourchette being peculiarly thick.

The head high up at the pelvic brim, and presenting such a surface to the touch as to make me quite undecided as to the exact position it might take in the pelvic cavity.

The uterine contractions strong, vigorous, and occurring at regular intervals of five minutes. As soon as the uterus began to contract, I found the thick perinæum, or rather fourchette, gradually thin out and dilate under my finger and thumb, and on the pain subsiding, the structure gradually returned to its former size and resistance. As the case went on, this dilatation or straining became more decided, while the return to its normal state was less perfect.

The head after some hours reached the hollow of the sacrum, and the labour proceeded to its termination in the usual way.

The conclusions I arrived at from the above facts are these.

1st, Nature has provided the perinæal structure with an inherent power of dilatation, and that this dilatation does not always depend on the pressure of the child's head.

2d, This dilatation takes place during a uterine contraction.

3d, When the uterus is quiescent, the perinæal structures return to a state of contraction.

My case was well fitted for the observation of the above facts, for it was a "*first case*," where the perinæum was in its virgin and normal state, never having suffered laceration or great distention, and one in which nature might be expected to come forward with all her means of help. The head of the child remained for six or seven hours at the brim of the pelvis, the pains keeping up regularly

all the time, and of course the perinæum was free from the slightest pressure, and open for free investigation.

As to the cause or *modus operandi* of this gradual and periodical dilatation, I am afraid much cannot be said.

Is it a *vital* process inherent in the perinæal structures themselves, or is it caused by pressure on the pelvic nerves producing a partial paralysis of the muscular tissues composing the perinæal outlet? I think it cannot well be the latter, for in this case the head was fixed and immovable for some time, and the pressure, if there was any, must have been *sustained*; whereas I found the parts relaxing and contracting, *i.e.*, they were in first an active then a passive state.

We know that sustained pressure on a muscle causes paralysis of its nerves, making the former to assume a flaccid and relaxed state; but here there was decidedly something more than mere flaccidity, there was regular relaxation and contraction. But then it might be said, did not the bag of membranes supply that amount of pressure usually given by the foetal head? No; for the waters had broken during the first pains.

Indeed, we have a right to suppose an inherent dilatability in the perinæal tissues for these two reasons,—

1st, In one class of cases the bag of membranes is absent, the liquor amnii escaping at an early stage of labour.

2d, In multiparæ, the foetal head passes too rapidly through, or rather over, the perinæum, to suppose a dilating force from that cause; indeed, in many cases, we should look upon this inherent dilatability as absolutely necessary to bring the labour to a favourable termination.

As to the practical bearing of this fact, it is not for me to speak. It certainly shows the accoucheur that nature has almost always means at her command of bringing even abnormal cases to a successful issue, provided there does not exist direct mechanical obstruction. Much has been written for some years back both for and against the propriety of supporting the perinæum during the expulsion of the child's head; one class of authors averring that support is the only safe treatment; another class, that this is not necessary; while a third set of men declare it does positive harm. Such discrepancy of opinion is only accountable on the plea that nature has not (in such cases) got credit for all she attains in bringing the accoucheur out of difficulty. Dr Leishman has amply shown that the same support is not at all necessary, and does not prevent laceration of the perinæum; indeed, according to him, it does harm, and so it must if nature (as I have endeavoured to show) has provided this process of alternate dilatation and contraction for opening the maternal outlet. It is a wise rule never to interfere with any physiological process, and surely it may be called "*meddlesome midwifery*" for the accoucheur to press his hand or thumb, as the fashion may be, on the muscular diaphragm

of the pelvic outlet, when that same structure is in a state of alternate dilatation and contraction. We find the muscular fibres composing the fundus of the uterus contract during labour, while those round the cervix dilate. Now is it too much to suppose that, while the abdominal muscles contract, the perinæal ones dilate, or that the dilatation found in the lower segment of the uterine cavity may not be extended to the maternal outlet. Dr Murphy teaches an inherent dilatability of the perinæum; but I maintain there is more, there is an action of much greater power, viz., that of alternate dilatation and contraction.

Since making this observation, I have had many cases of midwifery; and though in some there was the same decided action of dilatation and contraction, still in most of them (especially primiparæ) I could make out that force in action to a greater or less extent. It cannot be denied that the fœtal head does act as a dilator in most cases; but still cases do occur in which this inherent power is the only force in action. It remains to be decided whether or no this action is brought into play in every labour.

Part Second.

REVIEWS.

Saint Bartholomew's Hospital Reports. Edited by Dr EDWARDS and Mr CALLENDER. Vol. I. London: Longmans & Co.: 1865.

WE are glad to find that the medical and surgical staff of Bartholomew's is following the example set by Guy's, and very recently by the London, Hospitals, and is entering upon the publication of an annual volume of reports. No hospital in the kingdom affords a wider field of observation than St Bartholomew's; we find from the summary of statistical reports that between five and six thousand cases are admitted annually, among which is included an unusual number of serious and interesting cases. The volume before us is entirely the work of the Hospital staff, as they thought it right to take the initiative in the matter; but the editors express a hope that subsequent volumes may contain contributions from members of the school in other parts of the country.

The first paper is by Mr Paget, and is entitled "Cases of Chronic Pyæmia." Under this designation, Mr Paget describes a state which resembles acute pyæmia in the formation of purulent collections; in the occurrence of rigors, profuse sweatings, and inflammation of joints; and in the probable causation; but which

differs from it in its much more protracted course, and in the absence in its later stages of severe disturbance of the health, and of nearly all danger to life. There is, indeed, an intermediate class of cases, commencing with acute symptoms, assuming a chronic course, and very slowly terminating fatally, of which Mr Paget speaks as follows:—

“It may be generally said of them that, after presenting the ordinary signs of acute pyæmia, the disease continues week after week steadily destroying the health. Its course is indicated by slow wasting; all the tissues becoming dry and shrivelled; by increasing pallor; by decreasing muscular and mental power, the voice becoming weak, the mind slow and dull, and at night often wandering; by quickness and feebleness of pulse and breathing; by frequent and sometimes profuse sweatings, especially when there is much suppuration; by less frequent chills or rigors; by increased thirst and usually aversion from food; by dryness and shrinking of granulations. I do not pretend that the general signs of pyæmia can in every case of this kind be distinguished from those of hectic, or of mere exhaustion; yet commonly they are distinct enough, and the distinction becomes very nearly certain when, as it often happens, there appear occasional patches of redness on the skin, or abscesses with flaccid walls, or cedema of a foot or hand, or indications of pneumonia.”

It is not these cases of which Mr Paget treats, but of those which are chronic from the first and which are distinguished not only by their slow progress, but by the comparative mildness of the general symptoms. Mr Paget describes four cases of this kind; the first occurred after ligature of the subclavian artery; the second after lithotomy; the third after acute necrosis of the os calcis; and the fourth after catheterism on account of stricture of the urethra. Of these all terminated in recovery except the first, where death occurred from secondary hæmorrhage on the sixty-fifth day after the operation.

Dr Andrew communicates a paper “on the Diagnosis of Systolic Endocardial Murmurs, whose point of greatest intensity is at or near the Left Apex of the Heart.” The author’s principal object is to endeavour to show how we are to determine whether a murmur with the first sound at the apex is due to mitral regurgitation or is produced within the ventricle. Dr Andrew’s paper is founded upon the observations of 100 cases, an abstract of the details of 30 of which is given, and the conclusions at which he arrives are the following:—

“1. That of systolic murmurs, audible at or near the apex, a large number, 34 per cent., do not indicate mitral regurgitation, or for the most part any very serious lesion.

“2. That the decision as to the regurgitant or non-regurgitant character of a murmur rests principally upon its presence or absence posteriorly.

"3. That intensification of the second sound in the pulmonary artery, as a gauge of the obstruction to the blood stream on the left side of the heart, is of great value, but that in estimating it there are certain precautions to be taken.

"4. That the occurrence of a non-regurgitant systolic murmur may be explained by changes on the inner surface of the ventricle, or by dilatation of its cavity leading to undue tension of the chordæ tendinæ."

In reference to these conclusions, we have two remarks to make. In the *first* place, it is certain that we may have a murmur at the apex, due to a certain amount of regurgitation, independent of any organic disease of the mitral valve, and occasioned by irregular action of the papillary muscles, or some defect of the innervation of the heart. This murmur of *dynamic* origin is of course far less serious than that due to organic disease, and though it is highly important to be able to distinguish it, we do not find that Dr Andrew alludes to it. And, *second*, Dr Andrew omits all mention of what we believe to be the only *certain* means of determining whether or not a mitral regurgitant murmur indicates real and primary disease of the mitral valve, namely, the presence or absence of an auriculo-systolic or pre-systolic murmur. If a pre-systolic murmur co-exist with a systolic, we may be certain that the mitral valve is diseased; but if it is absent, we have no certainty that the systolic murmur is not of dynamic origin or is produced by some change within the ventricle.

The next paper, entitled "Brief Notes of the Surgical Practice of the Hospital," by Messrs Callender and Willett, two of the assistant-surgeons, contains a notice of some of the groups of cases which came under observation during the year 1864. The classes of cases chiefly alluded to are diseases and injuries of the joints, diseases and injuries of the bones and periosteum, hernia, and tumours. The following extract gives a brief account of a case where the tongue was successfully removed by the *écraseur* :—

"A male, æt. 50, was admitted for cancerous disease of his entire tongue; his difficulty in swallowing and speaking was so extreme that he was advised to have the organ removed. This was done under chloroform, and without any great difficulty.

"The mouth was widely opened and firmly fixed so, and the tongue was drawn forward. To facilitate this latter step the mucous membrane and the soft parts on the floor of the mouth, including the attachment of the genio-hyo-glossi muscles to the inner side of the symphysis, were cut through close to the bone. The tongue was thus in great measure released from its anterior and inferior attachments, and could readily be drawn forward, so that the wire of an *écraseur* was without difficulty passed round its root, including the entire organ to its connexions with the larynx. The wire was tightened by degrees, and there was some free but not dangerous bleeding where the mass was detached.

"The man's recovery was uninterrupted and very rapid. Indeed he regained his power of swallowing in so short a time that he soon ceased to require special attention in feeding, and before the end of a week he could make himself intelligible by imperfect speech; so much so, that it would have been hard, without actual inspection, to have believed that the entire tongue had

been removed. The patient continues well, with tolerable articulation, and it is now more than twelve months since the operation was performed."

The next paper, by Mr Bowater Vernon, describes a case of hypertrophy of the tongue occurring after scarlatina, under the care of Mr Paget, in which the protruding portion was successfully removed by the ecraseur. The case, however, was tedious, as the stump showed a great tendency again to take on excessive growth.

Dr Greenhalgh communicates the first part of an interesting paper on "Tumours of the Pelvis and neighbouring parts, complicating Pregnancy, and impeding Labour." The following extract from Dr Greenhalgh's paper contains a summary of its contents:—"Out of fifteen cases of affections complicating pregnancy and obstructing labour, the nature of the cases and the results were as follows:—

"One case of thrombus of the labium and vagina.

"Four cases of tumours of the ovaries.

"Four cases of tumours of the uterus.

"One case of tumour of the rectum.

"One case of tumour of the kidney.

"Four cases of extra-uterine foetation.

"Eight of the patients reached the full period of uterine gestation; in the remaining seven pregnancy terminated at some period between three and seven and a half months of gestation.

"In four cases labour set in spontaneously.

"In two cases labour was induced artificially.

"Four patients were delivered by the natural efforts at the full period, one at the third month.

"In fourteen cases the labours lasted for periods varying from five to thirty hours.

"Of the fourteen children, eleven presented with the head, one by the feet, one by the arm, and another by the breech.

"Twelve were males, four females.

"Turning was had recourse to in four cases, but was completed only in three, the fourth being subsequently terminated by the Cæsarean operation.

"Two were delivered by the forceps.

"Five of the mothers and eight of the children were lost; of the former three died from exhaustion and two from hæmorrhage."

Mr Thomas Smith records a case of restoration of the lower jaw after its entire removal. A lucifer-match-maker had suffered from symptoms of necrosis of the lower jaw for three years. He was admitted into St Bartholomew's Hospital in September 1864, and under chloroform the jaw was removed by dividing it at the symphysis, and dragging the two parts out separately. The dead bone came away completely denuded of soft parts, and without the slightest remnant of periosteum. "After the removal of the bone, on introducing the finger into the mouth, a firm fibrous mass was found occupying the substance of the gum in front of the chasm which had contained the jaw; but its outline was fused in the

surrounding swelling and induration; the only spot where bone was to be felt was in front of the position of the symphysis." His recovery went on satisfactorily, the only unpleasant symptom being that during the night when in the recumbent position he was frequently seized with fits of choking, due doubtless to falling back of the tongue, the original attachments of which to the jaw had been destroyed, and the new attachment which had formed brought the organ much nearer the hyoid bone and the upper opening of the larynx than was natural. He was discharged six weeks after operation, but died the same night. After his return home he had, it appeared, indulged somewhat freely in stimulants, and during the night woke suddenly with symptoms of suffocation. His wife stated that he caught at his throat with his hands, and after gasping for breath for a minute or two, he sank back and died. The most interesting point in the case is, that on post-mortem examination the jaw was found to have been almost completely restored. The new bone consisted chiefly of three portions, of which two were formed by the coronoid process and condyle together of either side; while the third and largest portion represented the right ascending ramus, the angle, horizontal ramus, and symphysis, and extended as far as the position of the eye-tooth on the left side. In this case the new bone seems to have grown principally from the soft parts around the periosteum, and around the bone where the periosteum was destroyed.

Mr Savory communicates an important paper, "On the Local Effects of Blood-poisoning in Relation to Embolism." It contains the details of many experiments, and the conclusions arrived at are the following, which are essentially the same as those of Virchow.

"The causes of the local congestions and suppurations in pyæmia may therefore be thus classified:—

"Stasis due to mechanical action. A blockade produced by the impaction of solid particles. According to their size:—

"Arterial embolism. By fragments too large to pass through the smaller arteries.

"Capillary embolism. By fragments small enough to pass into the capillaries.

"Stasis due to change in the blood produced by the admixture of morbid fluid. The local effect of blood-poisoning, properly so-called.

"Capillary obstruction.

"Stasis due to a combination of the two above-mentioned causes.

"In all cases stagnation and congestion first ensue. The subsequent changes, whether towards resolution, suppuration, or gangrene, are determined by—

"The action of the morbid fluid or obstructing substance. The changes it provokes.

"The constitution and state of health of the individual."

Mr Alfred Willett gives an account of the cases in which large arteries were tied during the year 1864. The cases were ten in number, and were performed under the following circumstances and with the following results.—First, The external iliac was tied by Mr Wormald, on account of aneurism of the common femoral. Diffuse inflammation and suppuration of the cellular tissue took place, and the patient died on the eighth day. Second, The femoral artery was tied by Mr Laurence on account of aneurism by anastomosis. Death from secondary hæmorrhage occurred on the tenth day. In the third, fourth, fifth, and sixth cases, the superficial femoral was tied for popliteal aneurism by Messrs Wormald, Coote (twice), and Savory. Three of the cases did well, the ligature coming away on the twenty-third, twenty-seventh, and sixteenth days. The fourth case terminated unfavourably, death having occurred, apparently from exhaustion, on the eleventh day after the operation. On post-mortem examination, the femoral artery and vein at the side of the wound had sloughed to the extent of an inch. “The artery was well plugged both above and below the ligature.” The patient, 58 years of age, is described as having been “a feeble, cachectic-looking man;” “the viscera were all more or less fatty. Seventh, The femoral was tied by Mr Paget, an inch and a-quarter below Poupart’s ligament, on account of secondary hæmorrhage, twenty-three days after amputation of the thigh. The ligature came away on the fifteenth day, and the patient made a good recovery. Eighth, The axillary artery was tied by the house surgeon, on account of profuse hæmorrhage coming on thirteen days after primary amputation of the arm. The patient did not rally, but died eight hours after the operation. Ninth, Mr Holden tied the common carotid, on account of hæmorrhage from a suicidal gunshot wound of the cranium. The patient died four hours afterwards, having never rallied from the injury. Tenth, Mr Paget tied the brachial for hæmorrhage from a punctured wound of the upper arm. The patient recovered; the ligature came away on the tenth day.

Dr Reginald Southey communicates a paper on the “Minute Structure of the Human Kidney;” perhaps his most important conclusion is the following:—“We consider the tortuous or glandular tubes proper, as signified by their structure, contents, and intimate annexation with blood supply, to be that portion of the tubes in which the separation of the more solid constituents of the urine and its colouring matters takes place; but we regard the Malpighian bodies themselves neither as the especial seats of water filtration, as assumed by Bowman, nor as that of urea secretion, as Dr Isaacs, of New York, has from his experiments surmised; but rather, as their position and structure appoint them, as blood regulators.”

Mr Holmes Coote contributes “Remarks on the Operation of Excision of the Knee-Joint.” It appears that the operation has been performed in St Bartholomew’s ten times since August 1863.

The results have not been satisfactory, far less, indeed, than they have been elsewhere. We give them in Mr Coote's own words,—

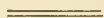
“Of these ten cases two died, being a mortality of one in five. Amputation was performed once, and an unsatisfactory condition of the limb existed in several of the recoveries. Now all these patients were particularly favourable subjects for operation, the eldest being only twenty years of age, the youngest was nine; they were ‘selected cases,’ and, consequently, present this operation under its most favourable aspect.

“The result of my own experience in the treatment of knee-joint-disease, especially in the young, is adverse to the operation of resection. I feel more confidence in the treatment by long continued rest, and the application of those principles comprehended by the term orthopædic surgery. And this opinion seems in consonance with the feeling of the upper classes, among the children of whom I have not yet heard of one instance of the performance of ‘resection.’

“When disease is so far advanced as imminently to threaten life, amputation is the preferable operation. To this rule the exceptions are, I believe, very few.”

The remaining papers in the volume consist chiefly of the records of cases lately under observation in the hospital, but space prevents us from alluding to them more particularly.

It only remains for us to congratulate the hospital staff on having made so auspicious a commencement, and to express a hope that their example may be imitated in other hospitals which do not, as yet, publish any account of their proceedings.



A System of Instruction in Quantitative Chemical Analysis. By Dr C. REMIGIUS FRESENIUS, Professor of Chemistry and Natural Philosophy, Wiesbaden. Fourth Edition. Edited by J. LLOYD BULLOCK, F.C.S., and ARTHUR VACHER, F.C.S. London: Churchills: 1866.

THE works of Fresenius on Qualitative and Quantitative Analysis are too well known both in Germany and in this country to require any recommendation from us. They have long since taken the position of being the best works on the subjects of which they treat. We have only to say of the volume before us that no pains have been spared by the author and editors in bringing it fully up to the present position of chemical science. It has been extended by the addition of about 100 pages as compared with the previous (third) edition, and either the student or the practical chemist may have recourse to it with the full confidence that he will find the newest and most accurate information with regard to any topic connected with quantitative chemical analysis.

Part Third.

PERISCOPE.

PRACTICE OF MEDICINE.

LATERAL DEVIATION OF THE EYES IN HEMIPLEGIA AND IN CERTAIN
EPILEPTIFORM SEIZURES. BY J. HUGHLINGS JACKSON, M.D.

IN relating an important case of hemiplegia from injury to the head in the *Lancet*, Dr Humphry mentions that soon after the accident both eyes were turned to the right, whilst the left was the side of the body paralyzed. "His livid, pale, expressionless face and helpless condition, with partial loss of consciousness *and steady direction of the eyes to the right*, indicated a severe shock to the brain and to the body." Lateral deviation of the eyes in hemiplegia is considered by M. E. Bouchut, in an appendix to his recently published work, "*Du Diagnostic des Maladies du Système Nerveux par l'Ophthalmoscopie*." He says that M. Vulpian has observed that, in hemiplegia produced by cerebral hæmorrhage or by softening of the brain, there is commonly deviation of the eyes towards the side of the body which is not paralyzed. This deviation is, M. Bouchut adds, only temporary. In the case Dr Humphry relates, he says, "During the first twenty-four hours consciousness was gradually restored, and the movements of both eyes became natural."

I have not yet observed this system myself, but I remember hearing Mr Lockhart Clarke, more than a year ago, in relating a case of hemiplegia, specially draw attention to this peculiarity. Again, Mr Hutchinson has long observed it as one symptom in the hemiplegia which attends arachnitis over one cerebral hemisphere.

The precise notation of the particular muscles paralyzed in the common form of hemiplegia is of much importance, and some differences of opinion may be reconciled by doing this very soon after the attack, as well as by observing their condition in chronic cases. The cases of hemiplegia which come under my care at the National Hospital for Epilepsy and Paralysis are nearly all cases in which the patient has recovered from the shock of the hæmorrhage or other damage to the nervous system which has produced the symptom. The cases at the London Hospital in which the patient is brought in very soon after the attack are not very numerous, and most of these I do not see very early. I think it very likely, then, that I have not seen lateral deviation of the eyes in hemiplegia, simply because I have not begun to study the cases of hemiplegia in which the symptom occurs until after it has passed off.

Partial paralysis of the orbicularis palpebrarum is another symptom very rarely observed in cases of hemiplegia of long duration. I have, however, recorded a case in which it remained weakened when the arm and leg had recovered. Yet, according to some observers, weakness of this muscle is nearly always present at first. Dr Todd was so struck by the absence of paralysis of the orbicularis that he even considered the facial nerve was not affected at all, and ascribed the deviation of the face to partial paralysis of the fifth nerve.

Such points, which seem of slight practical importance at first glance, are, I think, of great real importance in helping us to trace the resemblances, and to account for the differences in the particular muscles affected in unilateral paralysis and in unilateral convulsions. I hope to be able to show that, whilst the ordinary form of hemiplegia is generally due to loss of function of the

corpus striatum, there are good reasons for considering that in unilateral convulsions the corpus striatum is stimulated.¹ In one the muscles connected with it are paralyzed, and in the other, in action. Now I have seen lateral deviation of the eyes in some of those cases of epileptiform attacks in which one side of the body only has been convulsed; but in the few cases of the kind I have been able to observe carefully, the deviation has been, as one would expect, towards the side convulsed.

The muscles affected in unilateral convulsions are not only those which are paralyzed in the common form of unilateral paralysis, but I think many of the differences may be accounted for by a consideration of the hypothesis brought forward by Dr Broadbent in a most important paper recently read before the Medical Society of London.—*The Lancet*.

LATERAL DEVIATION OF THE EYES IN HEMIPLEGIA.

BY J. LOCKHART CLARKE, F.R.S.

It is now more than a year and a half since I first observed this symptom in the case of hemiplegia to which Dr Hughlings Jackson has referred in the *Lancet* of 24th March. The following is a brief abstract from my notes, made at intervals during the last three years of the patient's life.

A lady, aged seventy, had for several years been subject to frequent spasmodic attacks, which were sometimes so severe as to threaten suffocation by suspending the respiratory movements. Occasionally the attacks came on without any apparent exciting cause; but generally their recurrence was traced to errors in diet or indigestion, to unusual bodily exertion, and more particularly to sudden exposure to cold air. With the exception of tartar emetic and opium, which I had occasionally prescribed, nothing afforded her so much relief as a tumblerful of very hot water. In the intervals between the paroxysms she was not at all distressed by dyspnoea, nor was there any decided indication of organic disease of the heart: the pulse was regular; there were no bruits; the cardiac sounds, although somewhat feeble, were regular in regard to rhythm, nor were they heard over an unusually large space. The last and severest of her spasmodic attacks occurred one evening on her return home from the Crystal Palace, and she attributed its occurrence to the exertion of ascending the flights of steps from that station. In about an hour and a-half she recovered from the paroxysm; but the next day she was unusually pale and feeble. About a week afterwards she had a sudden attack of left hemiplegia, with momentary loss of consciousness. When I saw her, a few hours after the attack, the paralysis of the leg was complete; but the arm she could raise, although with difficulty, as high as her head. Gradually, however, it became weaker, and on the second day had lost all voluntary power. The left side of the face was partially paralyzed, and the mouth was drawn in the opposite direction. There was flapping of the left cheek during respiration, with deep and frequent sighing. The patient was very drowsy or lethargic, although when roused she would answer questions correctly; but the articulation was thick and almost unintelligible. On the third day of her attack (16th September 1864) she was less drowsy and more sensible, was perfectly aware of everything passing around her, and even made remarks of her own accord. Now I observed that *her eyes were constantly directed a little to the right*; that she could move them still further in that direction when requested to do so, but that *she was wholly unable to turn them to the left*, so that when desired to look in that direction she was obliged to turn her head. In this condition she remained for two days longer, when suddenly she became unconscious, and died in a few minutes. A post-mortem examination was not permitted.

There is another ocular peculiarity that I have since observed in a different form of paralysis, and which I have neither seen recorded nor found to have been noticed by any of my medical acquaintances to whom I have mentioned

¹ I use, for the present, the word "stimulated;" but I think that most clinical evidence tends to show that Dr Radeliffe's views on the condition of the nervous system in convulsive seizures are more correct than those generally received.

the fact. It consists of *a total inability to roll the eyes to one particular side—either to the right or to the left,—although there is not the slightest lateral deviation.* The following is the case in which I observed this symptom:—

A gentleman aged fifty-seven, who had suffered great mental anxiety in consequence of pecuniary losses, found one morning, as he sat on the side of his bed and was engaged in dressing himself, that he had almost lost the use of his lower extremities, that his articulation became thick and embarrassed, and his intellect considerably confused. On the fourth day of his illness I was requested to see him. He was then in bed, and with some difficulty could move his legs; but when taken out with the assistance of two persons, he was wholly unable to stand, the left leg appearing to be a little weaker than the right. Both arms were somewhat deficient in power, but he could grasp my hand with considerable force. His tongue was covered with a dirty-white coat; his breath was offensive; the skin of his face had a slightly yellow tint; the conjunctivæ were thick, and dirty-yellow; the pupils moderately contracted. His mouth was constantly half-open, and he breathed with a loud guttural noise, like a person soundly asleep. His eyes were perfectly straight, but had a peculiar, unmeaning, or indefinite gaze, and, like those of a young infant, were never fixed on particular spots; so that altogether the expression of his countenance was extremely idiotic. Yet he well understood everything that was said, and gave intelligible answers, but his articulation sounded as if there was some impediment in the roof of his mouth. On inspection, however, there was no apparent paralysis of the palate, nor any deviation of the uvula; the tongue also was projected, and moved in every direction without any difficulty. There was no indication of valvular or other disease of the heart.

The patient's spirits were extremely low, and his mind was painfully anxious. His manner was exceedingly childish. Frequently he burst into sudden fits of weeping, and cried out impatiently for his wife or daughter in apparent alarm, but without seeming to know, or being able to explain, the cause of his distress. At my visit on the following day I discovered the peculiarity in the movements of the eyes to which I have already alluded. There was no lateral deviation; but *he was wholly unable to roll his eyes to the right*, while he could readily turn them to the *left*. In the course of a few days this symptom disappeared. At the end of the second week the patient was decidedly improved; he had regained strength in his legs; his eyes were clearer and more intelligent, and the expression of his countenance was less idiotic. In two months more he nearly recovered his usual strength and general health; he could move briskly about, and took daily walks round Victoria Park. But his mind was considerably impaired, and his emotions were strangely excited; for he was subject, at times, to curious hallucinations, and would occasionally start up suddenly from his chair and wander about in apparent alarm, as if in search of something which he seemed unable either to explain or clearly understand. His articulation, although improved, remained considerably embarrassed.—*The Lancet*.

LATERAL (SYNERGIC) DEVIATIONS OF THE EYES IN SOME CASES OF HEMIPLEGIA.

M. PREVOST, formerly interne of the Salpêtrière, and now of the Hôpital St Antoine, has published (*Gaz. Hebdomadaire*, 13th October 1865) some interesting remarks on this subject. He gives an analysis of seven cases, which were very carefully observed during life and dissected after death, either by himself or by M. Vulpian. In every case the cerebral lesion was found either in or very near the corpus striatum of the side towards which the eyes had inclined. The general conclusions which M. Prevost arrives at, from his whole experience, are the following:—1. A synergic deviation of the eyes is tolerably common in apoplectic attacks. 2. This deviation is sometimes accompanied by rotation of the head on the axis of the neck. 3. This deviation follows a constant rule, the optic axis being directed towards the non-paralyzed side—*i. e.*, towards the side of the brain-lesion. 4. That this deviation seems to be a foreshadowing of that sort of gyratory movement which is observed in animals who have suffered wounds of the peduncle at the point of radiation of its fibres.—*The Lancet*.

DR GREENHOW ON ADDISON'S DISEASE.

At a meeting of the Pathological Society of London, held on the 20th March, Dr Greenhow exhibited for Dr Dickinson two supra-renal capsules infiltrated with cancer. They had been taken from the body of a man, aged 56, who had presented no discoloration of skin nor any of the constitutional symptoms of Addison's disease. Malignant disease of the spine was diagnosed during life, and on post-mortem examination cancer of the lumbar vertebræ, pancreas, liver, heart, and other organs was found, together with cancer of the supra-renal capsules. Dr Greenhow likewise exhibited the supra-renal capsules, tongue, penis, and scrotum taken from the body of a man aged 55. Addison's disease had been diagnosed on the day of the patient's admission into hospital. The constitutional symptoms were characteristic, and besides slight discoloration of skin there was deep discoloration of some cicatrices of burns, and also of penis and scrotum, and the tongue presented on its upper surface round the free edge irregular purplish stains. On microscopical examination the pigment causing these stains was found deposited in the papillæ in brown masses, the superficial layer of epithelium remaining uncoloured. The supra-renal capsules had undergone the peculiar morbid change usual in Addison's disease. In the lungs and other organs there were also tubercular deposits. Dr Greenhow said that these two cases were good illustrations of the difference in the effects produced on the general health by different diseases of the supra-renal capsules. A discussion two years before at the Pathological Society, showing that doubts were still entertained as to the reality of Addison's discovery, and that these doubts were mainly fostered by the confusion of genuine and spurious cases, had induced him (Dr Greenhow) to abstract from the British and Irish journals, for the purpose of analysis, all the published cases not of Addison's disease only, but of cancerous and other diseases of the capsules, and also of bronzed skin without any disease of the supra-renal capsules. Subsequently his attention had been drawn to treatises on the subject by M. Virchow and M. L. Martineau, and he had translated from the original sources all the foreign cases quoted in their writings. In all he had collected 195 cases, which he had carefully analyzed, and had found that in 127 the supra-renal capsules had undergone the particular morbid change characteristic of Addison's disease. In the other 68 cases the capsules were either healthy or had undergone cancerous or some other morbid change, or the change in them was too obscurely described to enable him to determine its nature. With a few explainable exceptions, none of these 68 cases presented either the constitutional symptoms or the peculiar discoloration characteristic of Addison's disease. On the contrary, among the 127 genuine cases, 95 presented either well-marked constitutional symptoms or characteristic discoloration of the skin, and 72 of these presented both. In 20 cases some of the constitutional symptoms or some discoloration of skin existed, and in many instances both, leaving only 12 out of the 127 cases which presented neither any characteristic symptoms nor any discoloration of skin. These 12 cases were all among those complicated with advanced tuberculosis or with other serious organic disease, which appeared to be the causes of death rather than the co-existing disease in the supra-renal capsules. On the other hand, only 5 of the 95 cases presenting characteristic symptoms or discoloration were found among the number complicated with serious non-tubercular diseases, and not one among those complicated with advanced phthisis. These facts seemed to be a sufficient refutation of the theory current abroad, that the discoloration of skin peculiar to Addison's disease is due, not to the morbid change in the capsules, but to the general tuberculosis frequently associated with it. At the same time, his (Dr G.'s) analysis of the 127 genuine cases showed that while 46 cases were reported as entirely or virtually uncomplicated, and 16 as complicated with other serious diseases without tubercle, no less than 65, or an absolute majority of the whole number, were complicated with tubercular disease of lungs or other organs, in all gradations of development; so that it was impossible to avoid the conclusion of a very intimate relation between Addison's disease and the tubercular diathesis.

Mr Hutchinson suggested that the bronzing was a question of time, and that if the patient died early of Addison's disease there might be no bronzing.

Mr C. H. Moore asked if Dr Greenhow's cases of cancer of the capsule were cases in which both these organs were quite destroyed, or in which there was a cancerous tubercle in each. Mr Moore then made some remarks on the variety of double cancer, and concluded by saying that cancer was a disease of a texture and not of an organ.

Dr Bristowe mentioned a case in which both capsules had been affected by primary cancer.

Mr Gaskoin thought it questionable whether there really was a relation betwixt the bronzing of the skin and the disease of the capsules. Dr Greenhow had collected cases of Addison's disease, and thus might have excluded cases of disease of the capsules in which there had been no symptom of Addison's disease.

Dr Quain said that he would wish to hear from Dr Greenhow a clear definition of what he meant by the term "Addison's disease." The statistics quoted included a certain number of cases of bronzed skin without any disease of the supra-renal capsules, and a still larger number of cases in which there was more or less disease of the capsules and no bronzing of the skin at all. It did not seem altogether philosophical to take these two conditions and to say that it was their co-existence which alone constituted the disease. Dr Quain then gave the particulars of an interesting case of fatal anæmia in a young gentleman, which he had seen with Dr Addison, Dr Bright, and Dr Williams. It was one of the first two or three cases which had led Dr Addison to state his views on the subject. In that case there was a deep ash-coloured pallor of the skin; the capsules were small, but not diseased. Dr Quain thought there was still much room for investigating the character of the disease.

Dr Greenhow said in reply that the cases collected by him were not of Addison's disease only, but of all diseases whatever of the supra-renal capsules which he had been able to find published from the time of Addison's discovery up to the end of 1865. He had only abstained from giving the well-known description of Addison's disease because he desired to be brief; but he thought it might be clearly defined as a particular lesion of the supra-renal capsules, manifested during life by a certain train of constitutional symptoms attended almost always by a characteristic discoloration of skin. No doubt his collection included a few cases of bronzed skin without any disease of capsules, but the bronzing was not the discoloration characteristic of Addison's disease. It included also a certain number of cases in which the capsules had been diseased without any discoloration of skin; but, with one or two exceptions, the disease either was not Addison's, or, being Addison's, it had been masked by some other predominant disease which appeared to have been the cause of death. As Mr Hutchinson had suggested, when the discoloration was altogether absent, death had probably taken place at an early period of the disease. As regarded the relation between the symptoms and discoloration of Addison's disease on the one hand and the characteristic change in the capsules on the other, he had no more doubt of its existence than he had of the existence of a relation between incompetency of the mitral valve or of acavity in the lungs and the physical signs of these lesions during life. He had at present three cases of Addison's disease under observation, and he was bold enough to say that he should be quite willing to leave the question of the reality of Addison's discovery to be determined by the result of the post-mortem examination in any one of the three.

Dr Murchison said, that as Dr Greenhow had found the disease of the capsules to be invariably tubercular, other disease of the capsules, viz., cancer not producing Addison's disease, it would seem that the symptoms were those due to tubercular disease, and not to disease of the capsule.

In reply to Dr Murchison, *Dr Greenhow* thought that Dr Murchison's suggestion was sufficiently answered by the fact shown by his analysis that, in proportion as the tubercular complications became general or acute, the con-

stitutional symptoms and external signs of Addison's disease became less and less decided until they altogether disappeared.

Dr Risdon Bennett remarked on the peculiar irritability of the stomach in Addison's disease. There was paroxysmal vomiting, followed by alarming exhaustion.—*Medical Times and Gazette*.

ON THE TREATMENT OF DELIRIUM TREMENS. BY PROFESSOR GEORGE JOHNSON

THE treatment of delirium tremens has passed through various phases, and undergone various modifications, in accordance with the theories which have at different times prevailed with respect to the nature of the malady. Until within the last fifty years, it was generally looked upon as an inflammatory disease. Many of the cases which the older writers called "phrenitis" were in fact cases of delirium tremens. The treatment then was by bleeding and other antiphlogistic remedies, and the result was most unfavourable. In the year 1813, Dr Thomas Sutton, of Greenwich, published a tract in which he distinguished these cases of delirium, so commonly occurring in persons of intemperate habits, from cases of inflammation of the brain and its membranes. He first applied the term "delirium tremens" to the former class of cases, and remarked upon it that the name, though not perhaps the most appropriate, would serve to convey a notion of an essential symptom of the disease, and was sufficiently explicit for his purpose. He states that from the year 1798 to 1807 he resided on the eastern coast of Kent, first as physician to the forces, and afterwards as a private practitioner. He there found that delirium tremens was treated in two very distinct ways by different practitioners; the one party employing antiphlogistic remedies, the other using narcotics: and, as he says, he "soon perceived that the latter practice carried with it all the success" (p. 5). He adopted the successful mode of practice, and, after giving it a fair trial, published a very sensible treatise on the disease. He states that, in the course of three years, he had treated twenty-two cases of delirium tremens, and that only four of these had died. He estimates the mortality under the antiphlogistic treatment as two out of three.

Since that time the treatment of delirium tremens by narcotics, and especially by opium, has been gradually established as the orthodox practice; and it is, on the whole, a highly successful practice. But it does not always succeed. The doctrine that opium is to be given in large doses, and repeated fearlessly until the patient sleeps, is a dangerous doctrine, and must often have led to a fatal result. There are many patients who will die from the depressing effect of opium on the heart without being made sleepy by the drug. I had this fact deeply impressed upon my mind more than twenty years ago, when I was house-physician to the hospital. A man who had taken repeated large doses of opium sank rapidly exhausted without a symptom of narcotism, consciousness remaining until within a few minutes of his death.

Recently another theory of delirium tremens has been advocated by Dr Laycock (*Edinburgh Medical Journal*, 1858-9, p. 289). This theory is, that the presence of alcohol in the blood is the immediate cause of the disease, and therefore that the main object of treatment should be to eliminate the alcohol. Opium and alcohol must, according to his theory, be injurious in the treatment, since opium checks elimination, and the administration of alcohol tends to perpetuate the disease; yet, with curious inconsistency, Dr Laycock states that a glass of wine with gruel at bedtime is a very useful soporific in the treatment of delirium tremens.

The three chief modes of treatment, therefore, which have been pursued in this disease are—

1. The antiphlogistic, now by general consent abandoned.
2. The treatment by opium, which is in very general use, and often highly successful; but which is attended with peculiar risks, the nature of which I will presently endeavour to point out to you.
3. Dr Laycock's eliminative treatment, with abstinence from alcohol and opium.

I will now tell you what, in my opinion, are the main points to be attended to in the treatment of delirium tremens, and what are the peculiar dangers which have to be guarded against.

And first, bearing in mind the tendency to suicide, the patient must be constantly and carefully watched by a judicious attendant who has the power to exercise some moral control over him. The doors and windows should be secured, and razors, knives, and other means of mischief, put out of sight and reach. He should be kept quiet and undisturbed by noise or excess of light. It is better, if possible, to avoid direct physical restraint. If the patient be violent and difficult to control, secure the services of two attendants rather than have recourse to a strait-waistcoat. I have known more than one instance of sudden death from exhaustion induced by violent struggling against the restraint of the strait-waistcoat, and I have also known more than one instance of fatal exhaustion from struggling with injudicious attendants. Not long since a medical friend called one morning and desired me to go with him immediately to see a publican whom he had just before seen with an attack of delirium tremens. On entering the room we found the patient gasping for breath and evidently dying; and we learned that immediately after my friend had left his patient, less than half an hour before, he got out of bed and wished to leave the room. Three strong men who were present resisted his attempt to go out; a struggle ensued, and in the midst of it he suddenly fell exhausted as we saw him, and he quickly ceased to breathe. You see, then, how great is the risk resulting from violent exertion on the part of these patients, and how important it is to avoid any restraint which is likely to provoke a struggle. In some instances a patient who cannot otherwise be kept in bed will remain perfectly quiet under the restraint of the waistcoat; but while he is thus restrained he must on no account be left alone. He may begin to struggle at any moment, and this struggle may cause rapid and fatal exhaustion.

To sum up, then, what I have said on the subject of restraint: the guiding principle is to prevent any violent exertion on the part of the patient—any struggle either with the strait-waistcoat or with the attendants. A padded room, when it can be had, is the safest and most effectual means of restraint for a violent patient.

Having ensured the safe custody of the patient, we have next to consider the method of treatment. Now, in the first place, bear in mind that delirium tremens is one of those diseases from which recovery may take place without any assistance from drugs. Medicines are of great use in the treatment; but they are not essential. It is, however, of essential importance that the delirious patient should be *fed*. The history of the disease teaches us that the immediate cause of the delirium is the exhaustion consequent on a defect of wholesome nutriment; and until this defect be supplied, all attempts to procure the wished-for sleep, which is so generally followed by a cessation of the excitement, are often useless and worse than useless.

Whatever else you do or leave undone, never neglect to feed a delirious patient. If he will take food when he first comes under treatment, give it immediately, and let it be repeated as soon as he will take it. If there be, as there often is, a disinclination for food, with nausea and a coated tongue, an emetic of ipecacuanha, followed by a dose of calomel and colocynth, or a saline laxative, will be of use as a preparation for food and for the opiate, which may best be given at bedtime. You may then give from a half to one drachm of tincture of opium, which I believe to be a better soporific in these cases than the salts of morphia; and a smaller dose may be given in three or four hours if necessary. The first object is to procure sleep. But here I wish to impress upon you the necessity of great care and watchfulness. Do not make rash attempts to force on sleep by repeated large doses of opium. These attempts will often fail to procure sleep, and they may kill the patient. It is an undoubted fact, that opium in many of these cases has no soporific effect whatsoever. And not only in cases of delirium tremens is opium uncertain in its operation. When we give opium in ordinary cases of disease

as an anodyne or a soporific, we can never be sure that it will cause sleep. In a considerable proportion of cases opium *prevents* sleep, and makes the patient more wakeful than he would have been without it. Another not uncommon effect of an opiate is to cause nausea and faintness. These two effects of opium should always be borne in mind in the treatment of delirium tremens. Remember that when opium fails to act as a soporific in delirium tremens it is not inert, and must not be given in repeated large doses as if it were; while it fails to procure sleep, it may be exerting a powerful depressing and paralyzing influence upon the heart. The symptoms of opium thus acting on the heart are these:—The patient continues wakeful, excited, and delirious, but grows rapidly weaker; the pulse becomes quick, small, and feeble; the pupils are contracted; the skin is bathed in a profuse sweat; and, if the opium be continued in large and frequent doses, the patient rapidly sinks, but remains wakeful and conscious until perhaps within a few minutes of the end. The opium in these cases acts as a powerful sedative on the heart, and in proportion as it does this it fails to exercise any soporific influence. If you find that opium is acting thus injuriously, you must immediately discontinue it, and give liberal doses of brandy, or the stimulant to which the patient has been accustomed. Full doses of quinine, too, will be of use as a tonic; and nutriment, either in the liquid or solid form, should be freely given.

It is an undoubted fact that the soporific effect of opium is often much more certain and decided when the medicine is given with some alcoholic stimulant than when it is given alone; and I have seen not a few cases of delirium tremens in which, after repeated large doses of opium have failed to procure sleep, a liberal allowance of the patient's accustomed stimulant, more especially when combined with food, has been followed by a long sleep, and this by an entire freedom from delusions and delirium. I scarcely need impress upon you that these unquestionable facts are quite irreconcilable with the hypothesis that the proximate and essential cause of delirium tremens is the presence of alcohol in the blood. If the patient refuse to take the opiate or other medicine which you are anxious to give, he may sometimes be induced to take it mixed with his beer or other beverage. Both food and stimulants may be given by the rectum in case of need; and morphia may be introduced subdermically; but this mode of giving morphia requires at least as much caution as the administration of opiates by the mouth.

Chloroform vapour has the immediate effect of quieting the delirium and excitement; but the quiescence is only temporary, and I have seen no permanent good result from it. The fat and flabby state of the heart in these cases is a bar to the incautious administration of chloroform.

When there is much flushing of the face and heat of the scalp, cold lotions or iced water in a bladder may be applied to the head with advantage.

You may have heard or read of large doses of digitalis having been given for the cure of delirium tremens. I consider this a very dangerous remedy. I have no doubt that some patients have recovered in spite of the treatment. When half-ounce doses of the tincture have been given, it is to be hoped that the digitalis was of bad quality; the patient would then, at any rate, have the benefit to be derived from the stimulant action of the rectified spirit. I have heard of several instances of sudden death after the administration of large doses of digitalis, and it is only natural that these calamitous results should not, as a rule, obtain equal publicity with the cases of apparently successful treatment. A consideration of the natural history of delirium tremens suffices, I think, to show that the treatment by large doses of digitalis is irrational and dangerous.

To sum up, then, the main practical points which I have endeavoured to impress upon you. Bear in mind that an attack of delirium tremens, as a rule, subsides in a few days without help from medicines of any kind. Let the patient be carefully watched and guarded and fed, and the excitement and delirium will gradually subside. In many instances opium is a valuable aid in the treatment, and it will often cut short the disease; but remember that

it is powerful for evil as well as for good. Its use, therefore, requires much care and discrimination. Some form of alcoholic stimulant will often be of great use in calming the nervous excitement and procuring sleep. Lastly, when the patient has recovered from his attack, do not fail to impress upon him the moral lesson which his sufferings have been designed to convey. The tortures of delirium tremens are most assuredly intended to exercise a wholesome deterring influence on the self-indulgent and the intemperate, and in that light he should be taught to view them. The poet Wordsworth has given the true interpretation to such spectral visions as form so marked a feature of delirium tremens, in the following lines from his "Sonnets on the Punishment of Death:"—

"Ye brood of conscience,—Spectres! that frequent
The bad man's restless walk and haunt his bed,—
Fiends in your aspect, yet beneficent
In act, as hovering angels when they spread
Their wings to guard the unconscious Innocent."

—*The Lancet.*

Part Fourth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLVI.—MEETING VIII.

2d May 1866.—Dr MOIR, *President of the Society*, in the Chair.

I. APHASIA WITH RIGHT HEMIPLEGIA,—SOFTENING IN THE ISLAND OF REIL, AND EXTERNAL FRONTAL CONVOLUTION.

Dr Sanders exhibited a drawing taken by Mr Strethill Wright, of the base of the brain in a case of aphasia, recently fatal. The patient, Robina S., a female aged 50, had been under observation in the Royal Infirmary since 23d August last, and died on 17th April. She had been taken ill three days before admission. She was hemiplegic on the right side, and had entirely lost the power of speech, although her intelligence remained. She could only mumble inarticulately; no single word, not even "Yes," or "No," could be made out. Her writing consisted of unintelligible scratches. She was evidently anxious to express herself, and wept at her failure. Curiously, she could by signs make her husband understand her, and, by the nurse's report, they contrived to quarrel. There was no paralysis of the tongue or larynx; the hemiplegia affected chiefly the arm (which afterwards became rigid in the flexed position), also the leg and face on the right side, both of which, however, recovered a considerable amount of voluntary motion. She died of exhaustion, bedsores, and, ultimately, pneumonia. At the autopsy, the arteries at the base of the brain were found highly atheromatous. On the left hemisphere the principal lesion was softening, occupying the anterior and external portion of the island of Reil, and extending outwards and forwards into the under surface of the posterior part of the external frontal convolution (Broca's localization), which was affected, however, only to a small extent. The softening also penetrated inwards to the external and anterior part of the corpus striatum. There was also a small separate softening at the posterior part of the corpus striatum. There were also small detached softened portions in the white matter above and also behind the left ventricle. No lesion of the right hemisphere. Mr Turner examined the brain along with Dr S. This case partly supported, partly differed from, Broca's views. It suggested the island of Reil as a seat

of the lesion in aphasia, the lobule of the island being of great importance in the conformation of the brain. Dr S. mentioned, that since his case of aphasia had appeared in the *Edinburgh Medical Journal* of March, he had been asked to see three cases, and had received from friends information in regard to other three cases of aphasia; in all of the six cases, the aphasia was associated with hemiplegia, always of the right side of the body.

II. ON PARACENTESIS THORACIS IN THE TREATMENT OF PLEURAL EFFUSIONS, ACUTE AND CHRONIC.

Dr Warburton Begbie read a paper on this subject, which will be found at page 1076 of this number of the *Journal*.

Dr Haldane had listened with great pleasure to *Dr Begbie's* paper, which he regarded as a valuable contribution to practical medicine. With *Dr Begbie's* conclusions *Dr Haldane* entirely agreed; he was satisfied that by means of paracentesis recovery might be accelerated, and even lives saved in cases of pleuritic effusion. *Dr Haldane* had seen two cases where death resulted from simple pleurisy. The first case occurred about twelve years ago. The patient was a stout young woman, a servant, twenty-five years of age, and had been suffering from symptoms of pleurisy for about a week. When *Dr Haldane* first saw her, there were the signs of copious effusion into the left pleura; there was dullness of percussion extending up to the clavicle, and extending to nearly the right border of the sternum; there was displacement of the heart, its apex beating below the right nipple. The symptoms were, however, by no means urgent; the pulse was of good strength, and the respiration not very greatly interfered with. A diuretic mixture was accordingly ordered. Next morning there was little or no change; the patient had had a pretty good night, but there was no increased flow of urine, and no diminution in the pleuritic effusion. On the same afternoon, *Dr Haldane* happening to be in the house of the patient's master at the time, he was suddenly called upon to go down stairs to see her; he found her in a chair in a state of syncope. She was immediately laid down in bed, but did not recover consciousness, and died almost immediately. The patient it appeared had been tired of lying, and had persuaded two of her fellow-servants to help her out of bed and put her on a chair; almost immediately she fainted, and death occurred within about five minutes. On post-mortem examination, with the exception of copious effusion into the left side of the chest, and the presence of some recent lymph on the pleura, the body was found quite healthy. The second case occurred about three years ago. The patient, a servant, twenty-three years of age, was admitted into the Royal Infirmary under *Dr Haldane's* care, suffering from extensive effusion into the right side of the chest. As the effusion did not yield to treatment, *Dr Haldane* determined to tap the side. Before doing so, however, he requested *Dr Warburton Begbie* to see her. *Dr Begbie* examined her and agreed as to the propriety of performing the operation. As it was late in the afternoon, and the students had left the hospital, it was agreed, as the symptoms were by no means urgent, to delay the operation till next day, in order that the members of the clinical class might have an opportunity of being present. On arriving at the hospital next morning between eleven and twelve o'clock, *Dr Haldane* went straight to the ward, and found to his horror that the patient had died a few minutes before. She had had a very fair night, and seemed in the morning in her usual condition. About eleven o'clock, while sitting up in bed, she was seized with a fainting fit, and died before the arrival of the resident physician. In this case also nothing was found except copious effusion into the left side of the chest, and the ordinary appearances of pleurisy. In both these cases, death had been manifestly due to syncope, which could only be ascribed to the presence of copious effusion, while the tendency to it might possibly have been aggravated by the very considerable displacement of the heart which had taken place. Accordingly, in similar cases *Dr Haldane* had now no hesitation in performing paracentesis; indeed, in this respect, he went rather farther than *Dr Begbie*; he did not wait

till signs of failure of the circulation showed themselves, but in cases of extensive effusion, so soon as he was satisfied that it was not yielding to treatment, he recommended the operation, and he had never seen cause to regret having done so. And it must be borne in mind that the operation was of the very simplest character. In cases of acute effusion it was by no means necessary to endeavour to empty the side of the chest; the removal of twenty or thirty ounces of fluid was generally quite sufficient. Relief was given to the breathing, and what was of very great importance was, that absorption of the remaining fluid appeared to be greatly stimulated. Dr Haldane had seen cases where, previous to operation, the kidneys could not be got to act, but where, after part of the fluid had been removed, a copious flow of urine was speedily established. The operation, as stated by Dr Begbie, was unattended by risk; as it was not desired to empty the chest, there was little probability of any air entering, and even if a little air got in, he believed that this really did no harm, as it was very speedily absorbed. With regard to the propriety of performing paracentesis in cases of empyema, there was, Dr Haldane believed, little difference of opinion with regard to the expediency of that course.

Dr *Halliday Douglas* considered that Dr Begbie had made out a strong case for the performance of paracentesis as a curative measure. At the same time, it occurred to him that all the cases recorded (especially the hospital ones) had been seen late in the disease, and were in a different position from what they would have been in if seen at first. Dr Douglas had never seen a case of simple pleurisy terminate fatally. It might be of great importance that the cases were seen early, and that energetic treatment was put in force within the first twenty-four hours. He thought that perhaps in Dr Begbie's cases the necessity for operation depended upon the circumstances in which they had been placed. Treatment had been neglected in the early days, and the cases had got into a new phase in which paracentesis was required. Dr Begbie had expressed regret that he had not performed the operation in his first case; had the case been Dr Douglas's, he would have been glad that he had not done it.

Dr *Warburton Begbie* still considered that, in his first case, paracentesis might have saved life. Had he had the same knowledge of the operation at that time as he had now, he would certainly have performed it. Dr Douglas's experience as to the non-fatality of simple one-sided pleurisy was not singular; Louis and Walshe had both made the same statement. Indeed, there seemed to be something peculiar about pleurisy; it seemed, as Pidoux said, to vary, so that at some times there was a greater tendency to effusion and death than at others.

MEETING IX.

14th May 1866.—DR MOIR, *President*, in the Chair.

I. ON THE MODE OF ACTION OF STRYCHNIA.

Dr *A. J. Spence* read a paper on this subject, which will appear in the next number of this Journal.

II. ON THE DIFFUSION AND FIGURES OF LIQUIDS.

Dr *Strethill Wright* read a communication on this subject, which was illustrated by the oxy-hydrogen microscope.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXV.—MEETING X.

11th April 1866.—SIR JAMES YOUNG SIMPSON, *Bart.*, *President*, in the Chair.

I. CASE OF CONGENITAL JAUNDICE. BY DR JAMES CAPPIE.

The following note refers to a case of congenital jaundice, which proved fatal on the eighth day by hæmorrhage from the umbilicus. No post-mortem

was allowed, and the case is therefore incomplete; but as such cases are somewhat rare, the present may not be without interest to members of the Society.

On the 16th March last, I attended Mrs F. in her third confinement. In her second confinement, which took place in June 1864, she had the misfortune to be delivered of an anencephalous fœtus. Throughout her third pregnancy, she had a good deal of nervous anxiety lest any malformation should again present itself, but her general state of health was very good. When the child was born (at eleven o'clock P.M.), it was to all appearance healthy. It cried immediately, and I was glad to be able to assure the mother that she had got a fine, plump, well-formed girl. But as soon as daylight came in, it was observed to be jaundiced all over the body. The face was of quite a saffron colour, and the conjunctivæ were tinged. From the first the cloths were stained of a yellow colour by the urine. Infantile jaundice is usually considered a simple and easily remedied condition; but from its intensity and early appearance in the present case, I certainly felt uncomfortable as to the probable result. But there also appeared good reason to hope that the jaundice did not depend on any organic cause, such as obstruction of the gall-duct. When the bowels were opened, the usual dark-coloured meconium came away. The child was drowsy, and slept a good deal, but it could readily be roused up, and it would cry lustily when washed. It took the breast well, the mother being conscious that it sucked strongly. For a few days there was no change in its condition. The movements of the bowels were so natural that there did not appear much need for medicine, but it got some grey powder and small doses of castor-oil. As the saffron hue continued very high, and even the gums appeared yellow, I thought it advisable to warn the nurse of the risk of hæmorrhage when the umbilical cord should come off, and instructed her what to do if such an accident occurred. On the evening of the seventh day the cord came away, and on calling next forenoon I felt relieved when told that not a drop of blood had been lost. But at two o'clock on the afternoon of the same day, when the mother opened up its dress, she found its under-clothing saturated with blood. Pressure was instantly applied with a simple pad, and continued till I saw the child. It was then pulseless, the mouth and surface cold, and the breathing came on in hurried starts and then intermitted. It had got, first, a little wine-whey, and then diluted spirits, but both were instantly vomited. As the bleeding was checked by the pressure, I of course could not interfere further, and the child died at four o'clock.

A peculiarity of the above case is the fact that the jaundice was congenital. The yellow hue was not recognised in gas-light, but was instantly observed in day-light. I may mention that the liquor amnii must also have been yellow, for the cloths taken away from the bed were deeply stained of that colour, and there was no such mess as would have been caused by the bowels of the child having been opened before birth. On the first morning after birth, the parts of the white dress in contact with the skin appeared as if they had been stained with mustard. Another peculiarity was the comparatively healthy appearance of what came from the bowels. On no occasion was it of a pale colour. Once or twice a little white curd was mixed with green matters; but during the eight days the child lived, the movements were usually either dark in colour or of a bright yellow.

Sir James Simpson had seen several cases of secondary umbilical hæmorrhage, most of which were fatal, and some of them combined with jaundice and thrombosis in the umbilical vein. He had seen cauterization, ligature, a needle and figure-of-eight suture, and other means usually fail; but lately visited a case with Dr Moir, where the simple and free application of the saturated solution of perchloride of iron in glycerine arrested the bleeding after other measures seemed useless. Dr Churchill had recommended a trial of plaster-of-Paris as a solidifying compress; but he was not aware whether it had been employed in practice.

Prof. Hubbard mentioned that such cases were not uncommon with him in Connecticut, but were sometimes accompanied with purpura, and were generally fatal.

Dr Burn was equally unsuccessful, as he had tried all plans, even red-hot iron wire.

Dr Watson mentioned a case of umbilical hæmorrhage he had seen, where there was no jaundice; the child was well-formed, but died from the hæmorrhage on the seventh day; cord still attached, but ulcerated.

Dr Gordon said that he knew of three children which were all jaundiced, one more so than the others, but there was no hæmorrhage. Two of them (twins) died, and one recovered. On examination, the liver was found smaller than usual.

II. CASE OF INTERMITTENT FEVER IN A CHILD IN UTERO. BY DR HUBBARD.

In September 1846, during the universal prevalence of an epidemic form of malarial fever in Illinois, Mrs C. was delivered of her second child. She had been for many days the subject of intermittent fever of the tertian type, and had noticed that, on the days of the recurrence of the chill, there was an entire cessation of the motions of the child, which on her "well days" were resumed with their accustomed vigour. The administration of quinine was purposely withheld until after her confinement, on account of its well-known effects in causing uterine congestion and inducing premature uterine action.

She was delivered on her "well day," and the next day suffered a return of the ague. The child also had a well-marked chill, followed by fever. Quinine was administered to the mother, promptly arresting the disease, at the same time apparently arresting it in the child. It had been observed by physicians practising in malarious districts, that in seasons of unusual prevalence of intermittent fever, in cases where the mother has had the disease during the whole or a greater part of her pregnancy, that it was not very uncommon for children to be born with enlarged spleens, anasarca of extremities, and other appearances which follow protracted ague.

Sir James Y. Simpson stated, in relation to Dr Hubbard's interesting case of intra-uterine ague, that the disease was alluded to by Dr Morton and some of the older authors on medicine. Dr Russell, the author of a celebrated monograph on the Plague at Aleppo, alludes to some instances of children being born with plague-spots upon them; and has given, in a separate paper, the details of a case of ague in the fœtus. In this instance the mother had attacks of tertian ague every second day in the last weeks of utero-gestation, and the unborn infant had what were reckoned shakings and rigors on the intermediate days. That these shakings were tertian ague also in the fœtus was proved by the fact, that the child after birth continued to take them every second day till they were arrested by the use of bark.

III. PRACTICAL OBSERVATIONS.

Professor Martin of Berlin, said that *first* children generally were found to weigh less than second or third children, but in the seventh child born this was reversed. Dr Frankenhæuser had collected and published several years ago notes of 2000 births, and he almost universally found that the first children were smaller, and that young females married soon after puberty have lighter children than others. Dr Frankenhæuser said that he could generally tell the sex of the child before birth from the rapidity of the fœtal pulsations, those of the female being more frequent. He (Dr M.) said that it was his practice in Berlin to cut the margin of the os uteri in place of dilating with the finger, and from this treatment he saw no bad consequences follow; but he only adopted this course where the os uteri was thin. In cases of first labours, he generally waited a long time before doing it, and sometimes used a douche of tepid water.

Professor Hubbard said that some German practitioners adopted the cutting plan in western cities of America; but in the eastern parts they dilated sometimes, and used belladonna, which acted in a similar way applied to the os uteri, as it dilated the pupil (3j ad ʒi).

Professor Martin defended the plan of incising the os, and said he did it in one in sixty cases. In each case he cut in three or four places, and he never found rupture or tearing follow.

With regard to the action of ergot of rye, Professors Martin and Hubbard both bore testimony to its beneficial effects, in doses of twenty grains, two or three times, especially in the second stage of labour, and when there was no obstruction.

Dr Inglis said he had frequently given large doses without producing any effect.

Sir James Simpson said, that besides ergot of rye, tansy, digitalis, and Indian hemp had been used, but the ergot was, he believed, the best oxytotic yet proposed. When it did act, it changed the character of the pains, by diminishing the length and completeness of the intermittence between the uterine contractions.

Dr Burn always used the ergot in second and subsequent labours. In one or two cases there was no effect, but it generally did good; likewise, in cases of flooding.

Dr Moir said that the cases at the Maternity Hospital proved equally for and against the use of ergot of rye.

Professor Hubbard said that he never used forceps applied to the breech. He used the blunt hook or fillet, or the finger, in breech cases, to aid delivery.

Sir James Simpson had used the forceps in delayed breech cases, but more frequently he had pushed up the fœtus, and made it a footling. Once or twice he had used the air tractor successfully in tedious case of nates presentations.

IV. SPECIMEN OF LUMBRICUS.

Dr Cairns showed a lumbricus passed from a child three weeks old. The infant sister, when only a week old, passed one also, but much larger.

SESSION XXV.—MEETING XI.

April 25, 1866.—*Sir James Y. Simpson, Bart., President*, in the Chair.

I. REMARKS ON NEW MIDWIFERY FORCEPS, WITH JOINTED BLADES. BY *SIR JAMES Y. SIMPSON, BART.*

Sir James Simpson exhibited a set of midwifery forceps to the Society, which were separated in their shanks by a new joint, which was easily and firmly fixed. The only advantage proposed to be attained by this mechanism consisted in obtaining an instrument that was easily portable in the pocket,—a matter of some importance when “long forceps” were coming to be used by many as their sole form of the instrument. The joint consisted of an oblique division of each shank. The two parts when placed together were secured by a ring passing over the site of the division. In the line of the oblique division a squared projection on one side fitted into a corresponding squared indentation on the opposite side, which rendered the joint readily fixed and unfixed at pleasure and made it perfectly firm.

Dr Cappie showed his jointed forceps, and said that he found them do admirably; he generally passed one blade and then fixed the handle before applying the second blade.

II. DOUBLE PLACENTA.

Dr James Young showed a specimen of a large double placenta, where there had been only one child. The patient was attended by *Dr Burn*, who unfortunately was absent from the meeting.

Sir James Simpson considered the specimen a very interesting one, and suggested that the placenta should be injected. He said it was very rare to meet such a specimen of complete double placenta in cases where there was only one child.

SESSION XXV.—MEETING XII.

9th May 1866.—*Dr Burn, Vice-President*, in the Chair.

I. DISPLACEMENT OF THE UTERUS, GIVING RISE TO FATAL RESULTS. BY *DR THOMSON OF DALKEITH.*

This paper will appear in a future number of the Journal.

Dr Keiller said that retroversion of the uterus was a serious case during

pregnancy. It was rarely fatal, but might be from peritonitis supervening. During pregnancy the uterus was regarded as a pelvic organ for three months, thereafter it rose above the brim, and might be tilted back, and thus cause obstruction of the bowels. Dr K. related three cases of retroversion of the pregnant uterus at the third month. The first case was in 1855, where the patient suffered much from the distention of the bladder during her second pregnancy. The water was frequently drawn off. A round tumour could be distinctly felt in the vagina, and to this Dr K. applied the stethoscope, and easily and distinctly heard the usual bruit of pregnancy. He pushed up the fundus of the uterus, and the patient went on well. The second case was one of a similar kind, where the retroversion was extreme, but which was easily rectified, and she went on favourably. The third likewise was successful. In Dr Thomson's patient, her case was aggravated by the menorrhagia, which was probably caused by erosion of the cervix.

Dr Ziegler mentioned a case of retroversion of the pregnant uterus at the third month, which had occurred in the practice of his late father, and which proved fatal. On examination of the body, the bladder was greatly distended, and nearly ulcerated through.

After some conversation on the use of the long tube in cases of obstruction of the bowels,

Dr Ziegler remarked, that in using the long tube he had frequently found that when it had seemingly been introduced as far as it would go, by drawing it back a little, and injecting some water, you could then succeed in passing it still further up.

Dr Bryce stated that he had had a patient for whom he had frequently to pass the tube. At first he was in the habit of using the tube of the ordinary stomach-pump, but it at last proved too short. He then got a tube considerably longer, and had frequently passed it.

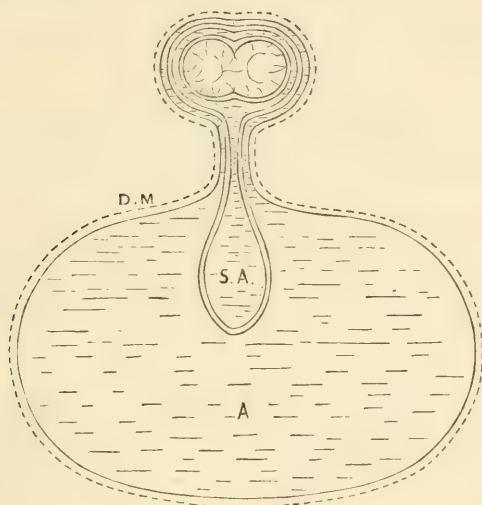
II. NOTES ON SIR J. Y. SIMPSON'S CASE OF SPINA BIFIDA (*continued from p. 1050*).

BY DR HANDYSIDE.

Dr H. said that, though many cases of spina bifida had been recorded, there had been very few dissections made in such cases. Hydiorachis might occur in seven different positions:—(1.) In the central primitive canal of the spinal cord—the effusion being enclosed in a terminal bag prolonged from the ventricular sub-arachnoid. (2.) On the surface of the cord between the pia mater and visceral sub-arachnoid. (3.) The most common form has its seat between the visceral and parietal layers of the sub-arachnoid membrane, *i.e.*, in the sub-arachnoid space. (4.) In the proper arachnoid membrane: This form is the second in point of frequency, but at the same time is comparatively rare. (5.) Surrounding the ordinary arachnoid is the areolar tissue connecting it to the dura mater, and in this position fluid may accumulate and form a spina bifida. (6.) External to all the membranes of the cord, yet within the vertebral column, and protruding through one or more of the intervertebral spaces. (7.) “In a distinct cyst, leaving all the membranes unaffected,” according to Dr P. S. K. Newbigging (*Prob. Ess.* 1834, p. 20). Authors have not hitherto defined sufficiently or recognised these different forms. Sir James Simpson's case was an example of the 3d and 4th forms combined.¹ What is the structure of the lining membrane of spina bifida? Kölliker thinks, that it is not serous membrane at all. In this case Dr H. used the microscope and found Kölliker's opinion so far correct; but Dr H. believes the cause of this absence of serous membrane to be a pathological alteration of the tissues in the process of disease. In Sir James Simpson's case, the second or internal sac *was* lined by serous membrane. Dr H. said he could find no description of such a condition as was presented in the case under examination by this second or small sub-arachnoid sac,—nor had he read of any case where the sub-arachnoid sac

¹ Explanation of “illustrative woodcut,” referred to at p. 1050. In the annexed diagram the initials of Dura Mater, Arachnoid, and Sub-arachnoid, indicate the relative position of these several membranes. The internal bilobed ovate line, within which some divergent lines proceed, is intended to mark the Pia Mater investing closely the *medulla spinalis*.

was so small in proportion to the whole tumour. Dr H. had been carefully looking at the preparation again, and had found lymph connecting the inner or visceral wall of the larger sac (where spread over the expanding neck of the contained ampulla) with its outer or parietal wall. This effusion he believed to be the commencement of a spontaneous cure; and quoted two cases, one from the 8th volume and the other from the 16th volume of the Transactions of the Pathological Society of London, where spontaneous cures had resulted from inflammatory action in the sac. As to the treatment of spina bifida, Dr H. suggested the slow draining off of the fluid as the safest. Compression after parietal abscision had sometimes succeeded, as in Sir William Newbigging's and Mr Johnson's cases.



The anatomical deduction Dr H. drew from the preparation exhibited, was one in connexion with a view put forth by Professor Sharpey of London, to the effect that the sub-arachnoid space is a distinct serous *cul de sac*. Dr S.'s speculation is confirmed by Dr H.'s dissection, *narrated at p. 1050*; on communicating which to Dr Sharpey, Dr H. received the following answer:—"Your reference to my view of the spinal sub-arachnoid membrane is quite correct. Since I first entertained the notion, I have not been able to carry the demonstration farther, as there were difficulties in the way. Probably your very interesting observation may help to clear up the matter." Meckel's opinion was that both arachnoids are one continuous membrane. This opinion, always made known by Dr H. in his lectures, has not been particularly alluded to since Meckel propounded it. Dr H. expressed his willingness to undertake the further investigation of the subject of the cerebral and spinal membranes, if the Fellows of the Society would furnish him with fresh specimens of young fetuses.

Dr Keiller showed a cast of a case of spina bifida, taken in the Sick Children's Hospital in Edinburgh, from a child four years old. The tumour was a very large one, and was situated at the lower part of the lumbar region, and covered by thick skin. The tumour was apparently composed of more than one sac—as Sir James Simpson's was—from the feel of it. The tumour appeared to have been punctured before the child was brought into the Sick Children's Hospital, for there was a small cicatrix. At last meeting Dr K. had mentioned a case where there was doubtless a double cyst as in Sir James Simpson's case. Dr K. had punctured the tumour in that instance, and had come in contact with an inner cyst. The child had died convulsed in the course of a few days. The tumour was in the neck. Dr K. spoke also of

another case of spina bifida where he had punctured the tumour with a needle several times. Neither good nor harm seemed to follow the acupuncture, and the child lived on with the tumour unaltered. It was ten years old when Dr K. last saw it in Dundee, and Dr K. believed it to be alive still. Dr K. asked Dr Handyside, if any spinal marrow tissue was found spread over the sac in Sir James Simpson's case?

Dr Handyside said that only a few nervous filaments were found spread over the interior of the larger sac. These formed a plexus. The caudal extremity of the spinal cord did not traverse the cavity of either sac, but terminated in a blind extremity surrounded by the smaller sac of sub-arachnoid.

Dr Keiller said that in his present case there was no paralysis of the lower extremities, and that he considered a favourable sign.

Dr Thomson of Dalkeith said, that all the cases of spina bifida he had seen had terminated fatally.

DEATH OF DR CRAIGIE.

WE record with great regret the death of Dr David Craigie, which took place in his house in Queen Street, on Thursday the 17th of May. In our next number we shall give some particulars regarding his life and works.

(To the Editor of the *Edinburgh Medical Journal*.)

42 QUEEN STREET, May 24, 1866.

SIR,—In answer to Dr Charles Bell's letter, allow me space simply to say that I used the same pessary as early as December 1852, and that Mr Young made them for me.—I am, Sir, yours, etc.

JAMES A. SIDLEY.

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